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## IN THIS ISSUE

DOUGLAS S. MASSEY is the Dorothy Swaine Thomas Professor of Sociology and chair of the Sociology Department at the University of Pennsylvania. His latest book, *Miracles on the Border* (University of Arizona Press), coauthored with Jorge Durand, won a 1996 Southwestern Book Award.

KRISTIN E. ESPINOSA is currently completing her Ph.D. in sociology at the University of Chicago. For her dissertation, she is investigating the effects of social networks and social capital on Mexico-U.S. migration. She is also the project manager for the Mexican Migration Project at the University of Pennsylvania.

ROBERTO PATRICIO KORZENIEWICZ is assistant professor of sociology at the University of Maryland in College Park. Besides his work on inequality, he is also investigating child care and female labor force participation in Latin America and continues to work on social movements and democratization in the world economy. He recently coedited with William C. Smith *Latin America in the World-Economy* (Greenwood Press, 1996) and *The Politics of Social Change and Economic Restructuring in Latin America* (Lynne Reinner, 1997).

TIMOTHY PATRICK MORAN is a Ph.D. candidate in sociology at the University of Maryland. His major research interests include global processes of stratification, income inequality, and issues in comparative political sociology.

SCOTT J. SOUTH is professor of sociology at the State University of New York at Albany. His research examines the influence of social and geographic context on intergroup relations, life-course trajectories, and demographic events, with particular emphasis on family information, marital dissolution, and patterns of residential mobility.

KYLE D. CROWDER is a doctoral candidate in sociology at the State University of New York at Albany. His research interests include urban politics, residential mobility, residential segregation, and their impact on family formation behaviors and economic attainment.

FANG DENG is assistant professor of sociology at North Central College. She received her M.A. from Beijing University in 1984 and her Ph.D. from the University of Chicago in 1996. Her research interests include social movements, social policy, and organization. She is currently studying strategic choices of public enterprises in China's economic reform.

JOHN MARKOFF is professor of sociology, history, and political science at the University of Pittsburgh. His current research deals with the interplay of social movement challengers and elite reformers in the history of democracy. He is the coauthor, with Gilbert Shapiro, of the forthcoming *Revolutionary Demands: Content Analysis of the Cahier de Doléances of 1789*.

DAVID A. COTTER is visiting assistant professor in sociology at Union College in Schenectady, New York. His research interests are in gender inequality and rural poverty.

JOANN DEFIORE is a sociologist and assistant professor in the Department of Liberal Studies, University of Washington, Bothell. Her areas of research are gender and race inequality and the effects of friendship on inequality.

JOAN M. HERMSEN is a doctoral candidate at University of Maryland, where she is completing her dissertation on the growth of women's higher education enrollments in the Western European context.

BRENDA MARSTELLAR KOWALEWSKI is assistant professor of sociology at Weber State University in Ogden, Utah.

REEVE VANNEMAN is associate professor of sociology in the Center for Population, Gender, and Social Inequality at the University of Maryland, College Park. In addition to research on gender stratification across U.S. metropolitan areas, he is investigating race, class, and gender inequality across districts in India.

ANNETTE BERNHARDT received her Ph.D. in sociology from the University of Chicago in 1993 and is senior research associate at the Institute on Education and the Economy, Columbia University. Her current research focuses on the effect of economic and firm restructuring on labor market outcomes.

MARTINA MORRIS is associate professor sociology and statistics at Pennsylvania State University. With Bernhardt and Handcock, she is analyzing the changes in work histories for two cohorts of the National Longitudinal Survey. She is also continuing work on variations in network structure on the spread of HIV in several international contexts.

MARK S. HANDCOCK is associate professor of statistics in the Department of Statistics and the Population Research Institute at Pennsylvania State University. His research develops statistical models for the analysis of job mobility, wage trajectories, and economic inequality; he also works on spatial statistics and inference for stochastic processes.

JONATHAN Z. SMITH is the Robert O. Anderson Distinguished Service Professor in the humanities at the University of Chicago. He is the editor of *The HarperCollins Dictionary of Religion* (1995) and the author of *Drudgery Divine: On the Comparison of Early Christianities and the Religions of Late Antiquity* (University of Chicago Press, 1996).

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## IN THIS ISSUE

IMMANUEL WALLERSTEIN is Distinguished Professor of Sociology and director of the Fernand Braudel Center for the Study of Economies, Historical Systems, and Civilizations at Binghamton University. He is the author of *The Modern World-System*.

DOUGLAS GUTHRIE is a graduate student in sociology at the University of California, Berkeley. He is currently finishing a dissertation on the structure and practices of firms in China's economic transition. He is also currently working on a study of technological changes and management practices adopted by U.S. organizations. In fall 1997 he will join the sociology department at New York University.

LINGXIN HAO is assistant professor of sociology at the Department of Sociology, Johns Hopkins University. Her research interests focus on the family and public policy and quantitative methods. Her current research concerns the impact of public and private support on child development, with one project focusing on single-mother families and a second project focusing on immigrant families.

MARY C. BRINTON is associate professor of sociology at the University of Chicago. Her interests are in the areas of gender stratification and social inequality, especially in East Asian societies. She is currently working on a book on how the school-to-work transition is managed by urban high schools in Japan, South Korea, and the United States.

KAREN BARKEY is associate professor of sociology at Columbia University. She recently published *Bandits and Bureaucrats*, a study of state centralization and rural movements in 17th-century Ottoman lands and is currently doing research on postimperial nation building.

RONAN VAN ROSSEM is assistant professor of sociology and criminal justice at the University of Delaware. He has recently published in several journals, including the *American Sociological Review*.

PETER BEARMAN is associate professor of sociology at the University of North Carolina at Chapel Hill. His current research focuses on models of identity formation, adolescent health-risk behavior, and the diffusion of heterodoxy.

NORMAN K. DENZIN is research professor of communications, University of Illinois, Urbana-Champaign. He is editor of *Sociological Quarterly* and coeditor of *The Handbook of Qualitative Research* (1994). His most recent book is *Interpretive Ethnography* (1996).

JOAN HUBER is senior vice president and provost emeritus at Ohio State University and former dean of its College of Social and Behavioral Sciences and a past president of the American Sociological Association. She is currently doing research in sex stratification.

JOHN MIROWSKY, professor of sociology at Ohio State University, studies the effects of mastery, equity, and distributive justice on emotional well-

being and physical health. He is currently exploring changes in the sense of control across the life course. With Catherine Ross, he coauthored *Social Causes of Psychological Distress*.

MICHAEL BURAWOY is professor of sociology and department chair at the University of California, Berkeley. He is the coauthor of *The Radiant Past: Ideology and Reality in Hungary's Road to Capitalism* (with János Lukàs).

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## IN THIS ISSUE

R. W. CONNELL teaches at the University of Sydney and was formerly at the University of California, Santa Cruz. He is the author of *Masculinities*, *Schools and Social Justice*, *Gender and Power*, and studies of class, politics, and social theory.

RANDALL COLLINS is the author of *Conflict Sociology* (1975) and *Weberian Sociological Theory* (1986); his book, *The Sociology of Philosophies: A Global Theory of Intellectual Change*, will be published in 1997.

MIGUEL ANGEL CENTENO is assistant professor of sociology at Princeton University. He is the author of *Mexico in the 1990s* (1991) and the editor of *Toward a New Cuba* (1997) and *The Politics of Expertise in Latin America* (1997). He is currently working on a book that analyzes the role of war in Latin American state making and explains the long peace in that continent.

HEATHER A. HAVEMAN is associate professor of organizational behavior at Cornell University's Johnson Graduate School of Management. Her research investigates firms' responses to shifting internal and external conditions, the impact of organizational change on subsequent performance and survival, and the consequences of organizational dynamics on the careers of employees and the composition of firms' workforces.

HAYAGREEVA RAO is associate professor in organization and management and adjunct associate professor in the Department of Sociology at Emory University. He studies how institutional and ecological processes lead to the creation, transformation, and extinction of organizational forms and routines.

TONY TAM is associate research fellow at the Institute of European and American Studies, Academia Sinica. He is interested in labor market inequality, the interface between career and social structure, educational stratification, and organizational behavior. His current research focuses on gender inequality, the earnings advantage for the graduates of elite schools, and a theory of strategic trust relations.

PAUL M. HIRSCH is the James L. Allen Professor of organization, strategy, and sociology at Northwestern University's Kellogg Graduate School of Management. He is also a faculty associate at Northwestern's Institute for Policy Research. He has straddled the boundaries dividing culture and social structure for two decades, usually seeking more balance by taking the side of whichever one seems to be getting the worst of it from the other. His present research interests are the study of markets as social constructions, work and family, and the cultural reframing of "losing" (jobs, life opportunities, etc.) as random or unavoidable, rather than discretionary and subject to challenge and changes.

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(Rev. 1/93)

# What's Driving Mexico-U.S. Migration? A Theoretical, Empirical, and Policy Analysis<sup>1</sup>

Douglas S. Massey  
*University of Pennsylvania*

Kristin E. Espinosa  
*University of Chicago*

Using data gathered in 25 Mexican communities, the authors link individual acts of migration to 41 theoretically defined individual-, household-, community-, and macroeconomic-level predictors. The indicators vary through time to yield a discrete-time event-history analysis. Over the past 25 years, probabilities of first, repeat, and return migration have been linked more to the forces identified by social capital theory and the new economics of migration than to the cost-benefit calculations assumed by the neoclassical model. The authors find that Mexico-U.S. migration stems from three mutually reinforcing processes: social capital formation, human capital formation, and market consolidation.

In this article, we seek to evaluate contemporary theories of international migration using Mexico-U.S. migration as a test case. A committee of the International Union for the Scientific Study of Population recently published two reviews that sought to evaluate theories of migration conceptually (Massey et al. 1993) and empirically (Massey et al. 1994). The theories reviewed by the committee included those advanced by neoclassical economics, the new economics of labor migration, segmented labor market theory, social capital theory, and world systems theory.

Although the committee's review of empirical research in North America found some support for all these theories, it noted the remarkable absence of any attempt to test the different models directly against one another within a common analytic framework. As a result, the committee concluded, it is "unclear . . . how well the various models perform

<sup>1</sup> Send correspondence to Douglas S. Massey, Population Studies Center, University of Pennsylvania, 3718 Locust Walk, Philadelphia, Pennsylvania 19104-6298.

against each other, and how much of an *independent* contribution to explanatory power each model might retain in a simultaneous examination of theoretical propositions" (Massey et al. 1994, p. 739; emphasis added).

Our purpose here is to conduct such an empirical test, one in which indicators derived from each theory are allowed to compete against one another directly in predicting the course of migration between Mexico and the United States. We draw upon a unique source of longitudinal data that allows us to link individual acts of migration—documented and undocumented—to a set of predictors defined at the individual, household, community, and macroeconomic levels. All indicators vary independently through time to yield a discrete-time event-history analysis of migration to the United States.

In performing this exercise, we also seek to evaluate the efficacy of recent U.S. policy initiatives designed to curb the flow of undocumented Mexican migrants to the United States. These initiatives generally assume that undocumented migrants make a cost-benefit calculation in deciding whether to migrate internationally. Drawing on neoclassical assumptions, U.S. policymakers have sought to deter illegal migration by raising the costs and lowering the benefits of undocumented movement. In this sense, an evaluation of the efficacy of recent immigration policies constitutes a test of neoclassical theory.

We begin by estimating a model to determine which factors initiate migration between Mexico and the United States and then estimate a model to discern which factors perpetuate this movement once it has begun (thus maintaining the conceptual distinction established elsewhere [Massey et al. 1993]); we conclude with a final model that assesses which factors govern the decision to return to Mexico. We find that variables derived from neoclassical economics are generally limited in their explanatory power. Over the past 25 years, probabilities of first, repeat, and return migration have been linked more to the forces identified by social capital theory and the new economics of migration than to the simple cost-benefit calculations assumed by the neoclassical model.

Our empirical analysis of the forces driving Mexico-U.S. migration suggests that the theoretical foundations of U.S. immigration policy are flawed. The dynamic expansion of migration between Mexico and the United States does not follow from simple changes in the objective costs and benefits of international movement but from the operation of self-perpetuating, interlocking, and mutually reinforcing processes of social capital formation, human capital formation, and market consolidation. Rather than discouraging these forces, the thrust of U.S. policies in recent years has been to amplify and reinforce them.

## DATA AND METHODOLOGICAL APPROACH

Our data come from simple random samples gathered during the winter months of 1987–92 in 25 communities located in the Mexican states of Jalisco, Michoacán, Guanajuato, Nayarit, and Zacatecas, which together constitute a region (western Mexico) that historically has sent the majority of migrants to the United States (see North and Houstoun 1976; CONAPO 1986; Jones 1988). Information about the samples is summarized in table 1.

In most cases, the sample consists of 200 households, but, in smaller communities, fewer households were chosen and, in one location, a larger sample was compiled. Sampling frames were constructed by carrying out a house-to-house census of each community. Usually the entire town or city was canvased, but in large urban areas this was not possible and specific working-class neighborhoods were sampled instead. Sampling fractions ranged from .029 to .803 and averaged about .228. Across the communities, our procedures yielded a total sample of 4,853 households representing a hypothetical population of 2.3 million people.

December and January are generally the best times to locate and interview seasonal U.S. migrants within Mexico because most return to spend the Christmas holidays with their families. In the case of one community, however, initial fieldwork revealed that a large number of migrants also returned in July (as reported in Reichert and Massey [1979]), so we sent an interviewer there during the summer to complete the survey. In general, however, the Mexican community samples are representative of dwellings occupied during the winter months of 1987–92.

These data were supplemented with nonrandom samples of out-migrants located in the United States during the summer subsequent to each winter's survey. From the Mexican community samples, we determined where in the United States the migrants went and sent interviewers to those areas to survey people who had settled abroad. Snowball sampling methods (Goodman 1961) were used to compile the sample of settled out-migrants. In most communities, 20 out-migrant households were interviewed, but in some cases smaller numbers were questioned, yielding a total sample size of 415 U.S. households.

Although the U.S. samples are not strictly representative of the settled out-migrant communities, we developed a set of weights to reflect their relative contribution to the binational sample. The weights, which are applied to each case in our analyses, are the inverse of the sampling fraction employed at each Mexican and U.S. site (Sudman 1983). In Mexico, sampling fractions were computed by dividing the number of households in the sample by the number of eligible households on the sampling frame.

TABLE 1  
MEXICAN COMMUNITIES SAMPLED

STATE AND TYPE OF COMMUNITY	1990 POPULATION	MEXICAN SAMPLE		U.S. SAMPLE	
		Sample Size	Sampling Fraction	Sample Size	Sampling Fraction
Guanajuato:					
Metro area .....	867,920	200	.232*	0	.000
Metro area .....	362,915	200	.100*	20	.999
City .....	52,291	200	.256*	20	.121
City .....	33,123	200	.072	15	.023
City .....	23,726	200	.113	15	.217
City .....	20,614	200	.053	20	.047
Town .....	16,535	200	.073	20	.816
Rancho .....	1,737	150	.605	20	.999
Rancho .....	1,080	100	.699	10	.999
Jalisco:					
City .....	74,068	201	.119*	20	.052
City .....	30,882	200	.113	20	.038
Town .....	4,760	200	.250	20	.642
Town .....	3,516	200	.392	20	.127
Rancho .....	3,098	200	.375	15	.260
Rancho .....	894	100	.467	7	.425
Michoacán:					
Metro area .....	492,901	200	.056*	20	.098
Metro area .....	217,068	200	.184*	13	.065
City .....	32,474	200	.029	20	.009
Town .....	7,025	200	.139	20	.248
Rancho .....	6,429	200	.143	20	.035
Rancho .....	2,240	150	.335	20	.999
Nayarit:					
City .....	19,645	200	.045	20	.012
Rancho .....	11,541	200	.074	20	.014
Zacatecas:					
Town .....	7,750	365	.213	20	.017
Rancho .....	5,785	187	.803	0	.000
Total .....	2,300,017	4,853	.228	415	.305

\* Sample of specific neighborhood rather than entire community

In the United States, sampling fractions were *estimated* by dividing the number of sample households by the *estimated* number of households in the out-migrant community.

We derived the size of each community's out-migrant population using data on the current location of offspring of the household head who were no longer household members. Our survey gathered information about *all* children of the household head, whether or not they were presently mem-

bers of the sample household. Nonmember children were generally those who had grown up and moved out to form their own households. As relatives of sample members, they constitute a network sample of the binational community (also known as a multiplicity sample; see Somoza 1981; Hill 1981; Kalton and Anderson 1986).

Following the procedure described in Massey and Parrado (1994), we determined the number of nonmember children who were living in the United States and Mexico at the time of the survey and formed the ratio between them to indicate the relative size of the U.S. community. We then applied this ratio to the Mexican sampling frame to estimate the total size of the out-migrant community, allowing us to calculate the U.S. sampling fractions. The latter ranged from .009 to .999 and averaged .305.

The Mexican community samples are intended to represent conditions in the core migrant-sending region at the time of the survey, while the U.S. surveys are designed to depict conditions in the corresponding U.S. settlements at roughly the same time. When pooled and weighted, they offer a comprehensive portrait of 25 binational communities created through recurrent processes of international migration and settlement.

In choosing our Mexican study sites, we sought to include a range of community population sizes, ethnic compositions, and economic bases. Communities were not chosen because they contained U.S. migrants *per se*, and in fact we obtained a wide range of migration prevalence ratios, ranging from one community where just 9% of adults had been to the United States to another where 60% had migrated (see Massey, Goldring, and Durand 1994). Although our sample is not strictly representative of the states of western Mexico, it contains a broad cross-section of households and communities in Mexico's largest migrant-sending region.

Respondents were interviewed using ethnosurvey methods (Massey et al. 1987). Within each household we gathered basic information about the social, economic, and demographic characteristics of the head, the spouse, the head's children, and other household members. We also determined which members had been to the United States and from them gathered basic data about the first and last U.S. trips: their dates, durations, and destinations, as well as the migrant's legal status, occupation, and wages. From each household head, we collected a detailed life history that included labor, migration, property, marital, and fertility information. For household heads who had been to the United States, we also asked a battery of questions about experiences on the last U.S. trip.

We supplemented these individual- and household-level data with community and macroeconomic information. An inventory completed for each Mexican community gathered basic information on the social, economic, and demographic characteristics of the community at various points in time, drawing on the Mexican census and other published sources. We

also traced the evolution of the town's infrastructure using data gathered from archival sources and from interviews we conducted with community officials. Macroeconomic statistics were compiled from the International Monetary Fund (IMF 1994) and the U.S. and Mexican decennial censuses.

Individual life histories were linked to the community histories and macroeconomic indicators to create the basic data used in this analysis. We selected respondents who were male household heads at the time of the survey and reconstructed their lives from age 15 onward to create an annual record of their personal and household characteristics and of the community and macroeconomic conditions they faced. We then used this event-history file to specify and estimate three statistical models: one predicting the annual odds of taking a first trip to the United States; another predicting the annual odds of taking an additional U.S. trip, given that at least one had already occurred; and a final model predicting the annual odds of returning home to Mexico during the years following entry into the United States.

The first model considers the process whereby international migration is initiated. Given each respondent's individual, household, community, and macroeconomic circumstances in year  $t$ , we use multinomial logit regression to predict whether or not the person left for the United States as either a documented or undocumented migrant in year  $t + 1$ . The second model considers the process whereby U.S. migration is perpetuated. It follows respondents from the moment they return from a trip to the United States and records their characteristics year by year until they make another U.S. visit, legal or illegal. Given individual, household, community, and macroeconomic circumstances in year  $t$ , we predict whether or not the respondent took an additional trip in year  $t + 1$ , controlling for the number of prior trips taken and the amount of U.S. experience accumulated. The third and final model considers the process of return migration. It follows respondents from the moment they enter the United States on any trip and records their characteristics year by year until they return home to Mexico. Given individual, household, community, and macroeconomic circumstances in year  $t$ , we predict whether or not the respondent left for home that same year.

#### THEORIES AND INDICATORS

The foregoing procedures yield a series of discrete-time event-history models with person-years as units of analysis (Allison 1984). The independent variables are time varying and are defined in table 2. In total, we investigate the effect of 41 variables grouped into 11 conceptual categories. These categories refer to the nature of the variables themselves rather than the theories to which they pertain, as different theories put forth

TABLE 2  
DEFINITION OF VARIABLES

Variable	Operational Definition
<b>Demographic background:</b>	
Age .....	Age at last birthday
Married .....	Respondent in formal or informal union
No. of minors in household .....	No. of own children under age 18
<b>General human capital:</b>	
Labor force experience .....	No. of years since first job
Education .....	No. of years of school completed
<b>Migration-specific human capital:</b>	
Cumulative U.S. experience .....	Total months spent in United States
No. of prior U.S. trips .....	Total no. of trips taken to the United States
Unskilled urban job .....	Unskilled nonagricultural occupation in the United States
Skilled urban job .....	Skilled nonagricultural occupation in the United States
<b>General social capital:</b>	
Parent a U.S. migrant .....	Subject's parent was a U.S. migrant
No. of U.S. migrant siblings .....	No. of siblings with U.S. experience
% U.S. migrants in community .....	Proportion over age 15 with U.S. experience
<b>Migration-specific social capital:</b>	
Wife a U.S. migrant .....	Wife has begun migrating to the United States
No. of U.S. migrant children .....	No. of children who have begun migrating
U.S.-born children .....	Whether any children were born in the United States
<b>Physical capital:</b>	
Land .....	Household owns farmland
Home .....	Household owns home
Business .....	Household owns a business
<b>Community infrastructure:</b>	
Preparatory school .....	Preparatory school in municipio
Paved road .....	Paved road between community and highway
Bank .....	Bank office open in municipio
<b>Community economic context:</b>	
% earning twice minimum wage .....	Proportion of workers earning at least twice the legal minimum wage
% self-employed .....	Proportion of workers who are self-employed
% females in manufacturing .....	Proportion of female workers employed in manufacturing
<b>Community agrarian context:</b>	
Agrarian economy .....	"1" if more than 50% of male labor force is employed in agriculture, "0" otherwise
Agrarian population density .....	Population divided by arable land
Proportion of land that is arable .....	Cultivable land divided by total land base
<i>Ejido</i> established .....	"1" if community had <i>ejido</i> , "0" otherwise



TABLE 2 (Continued)

Variable	Operational Definition
<b>Macroeconomic context</b>	
Expected wage ratio .....	Ratio of wages predicted from equations estimated from data on migrants to the United States and migrants within Mexico (United States/Mexico; in 1990 U.S. dollars)
Peso devaluation .....	Rate of change in dollar value of Mexican peso over prior year
Mexican inflation rate .....	Rate of change in Mexican consumer index over prior year
U.S. employment growth .....	Rate of change in total U.S. employment over prior year
Growth in foreign investment .....	Rate of change in direct foreign investment over prior year
Mexican real interest rate .....	Average cost of funds in Mexico - Mexican inflation
<b>U.S. policy context:</b>	
Availability of visas .....	Legal immigration divided by sum of legal immigration and gross illegal entries
Probability of apprehension .....	Likelihood of arrest while attempting to cross border without documents
Employer sanctions enacted .....	"1" if employer sanctions in force, "0" otherwise
Amnesty recipients in household .....	"1" if any member of household received amnesty under IRCA; "0" otherwise
<b>Expected value of U.S. services:</b>	
Welfare .....	Estimated likelihood of using AFDC or food stamps if respondent were to migrate to United States $\times$ average value of monthly AFDC and food stamp payments in states receiving Mexican immigrants
Medical care .....	Estimated likelihood of receiving unreimbursed medical services if respondent were to migrate to United States $\times$ average value of Medicaid payments in states receiving Mexican immigrants
Education .....	Estimated likelihood of using public schools if respondent were to migrate to the United States $\times$ average per pupil school expenditures in states receiving Mexican immigrants

contrasting hypotheses about the same variables so that they cannot be linked to one and only one theory.

### Neoclassical Economics

Neoclassical economics argues that prospective migrants make a cost-benefit calculation in deciding whether or not to migrate internationally (Todaro and Maruszko 1987). In the present case, Mexicans are hypothesized to determine the difference between what they can expect to earn in Mexico and what they can expect to earn in the United States. They cumulate this quantity over some future-discounted time horizon to derive an expected benefit from moving to the United States, and, from this expected benefit, they subtract the fixed costs of making the trip. The difference between the expected benefits and the anticipated costs yields the expected net return from international migration, which, if positive, leads a rational Mexican worker to leave for the United States.

According to the neoclassical model, therefore, the leading factor explaining Mexico-U.S. migration is the binational wage gap (i.e., the difference between what people can expect to earn in the United States and what they can expect to earn in Mexico). Accordingly, some indicator of the Mexico-U.S. wage gap has been included in virtually all prior attempts to account for the flow of migrants between the two countries (see Frisbie 1975; Jenkins 1977; Blejer, Johnson, and Prozacanski 1978; White, Bean, and Espenshade 1990; Bean et al. 1990; Espenshade 1990).

Our study is no exception, but, rather than relying on published wage data, we form a ratio between expected wages that we estimate directly from our sample data. The survey asked each household member to report wages earned on first and last trips within Mexico and first and last trips to the United States. We used these data to estimate equations that regressed hourly wages (in constant 1990 U.S. dollars) on selected individual attributes. Because characteristics that influence wages also influence migration, we adopted the method of Heckman (1979) and first estimated a probit equation to predict internal or international migration and then estimated a second OLS regression to predict Mexican and U.S. wage rates while controlling for the hazard of selection into the migrant workforce.

The equations used to estimate Mexican and U.S. wage rates are presented in appendix table A1. In each case, wages are predicted as a function of demographic background, education, labor force experience, U.S. experience, documentation, kinship ties to the United States, and period (expressed in five-year intervals). We use these equations to predict the wages respondents could expect to earn if they were to migrate within Mexico or to the United States within a particular person-year, given their characteristics in that year. The wage ratio defined in table 2 simply di-

vides the predicted U.S. wage by the predicted Mexican wage, each expressed in constant 1990 U.S. dollars.

In addition to influencing the likelihood of migration by determining expected wages, personal characteristics also affect the odds of movement by determining other potential benefits of international migration, such as occupational status, working conditions, and the prospects of job mobility. Although the neoclassical model posits that human capital significantly influences the probability of international migration, the direction of the effect cannot be determined without knowing whether and how different forms of human capital are likely to be rewarded at places of origin and destination (Massey et al. 1993).

In general, human capital acquired in Mexico is not well remunerated in the United States, especially if a migrant lacks documents. No matter what their education or years of labor market experience in Mexico, undocumented migrants are confined to the same menial jobs in the United States: busboy, maid, dishwasher, gardener, factory worker, and agricultural laborer. In fact, because there are few returns to human capital in the secondary labor market of the United States (see Portes and Bach 1985; Portes and Jensen 1989; Zhou and Logan 1989), prior studies have found undocumented migrants from Mexico are *negatively* selected with respect to human capital variables such as education (Massey and García España 1987; Taylor 1986, 1987; Borjas 1992).

We also include several indicators of migration-specific human capital, which, unlike general human capital, is more likely to be rewarded in the U.S. labor market (Chiswick 1978, 1979, 1984, 1988). This form of human capital consists of skills, knowledge, and abilities acquired as a direct result of participation in the U.S. economy. It does not come into play until a person has migrated to the United States at least once, and we measure it with three indicators: months of U.S. experience acquired before the person-year in question, number of prior U.S. trips, and occupational skill. The latter variable is measured with two dummy variables: whether or not the respondent held a skilled urban job on his prior U.S. visit and whether or not he held an unskilled urban job; agricultural jobs served as the reference category.

Table 2 also contains other variables that define the potential costs and benefits of international migration. A devaluation in Mexico tends to increase the costs of movement for undocumented migrants, who need more pesos to pay border-crossing guides (whose fees begin at around \$300 and are always denominated in U.S. dollars; see Donato, Durand, and Massey 1992). By raising the cost of surreptitious entry, a devaluation acts to discourage undocumented movement, but it should have no influence on the movement of legal migrants.

The rate of inflation in Mexico also affects the perceived costs and bene-

fits of international movement. During periods of rapid inflation, Mexicans expect the real value of domestic wages to decline. This anticipation of falling real wages in Mexico implies an expectation of rising real returns from U.S. wage labor, thereby increasing the discounted present value of international migration and raising the odds of going to the United States.

Recent policy initiatives in the United States are also connected to the cost-benefit model put forth by neoclassical economics (see Todaro and Maruszko 1987). At the most basic level, Congress has sought to deter migration from Mexico by reducing the number of visas available to its citizens. Prior to 1965, there were no numerical limits to the legal entry of Mexicans, but since then various restrictions have been successively applied. In 1968 Mexico was placed under a hemispheric quota of 120,000 (forcing it to compete with other Latin American and Caribbean countries for visas); in 1976 it was placed under a country quota of 20,000; in 1978 it was included under a global ceiling of 290,000 (forcing it to compete worldwide for visas); and in 1980 the global ceiling was reduced to 270,000 (Jasso and Rosenzweig 1990). These restrictions, moreover, came at a time when the demand for visas from Mexico was rising.

These actions have raised the costs of migration by increasing the size of visa backlogs, multiplying the legal expenses associated with documented entry, and inflating the waiting time for the receipt of papers. We measure the availability of visas by forming the ratio between annual legal Mexican immigration and the sum of legal immigration plus gross illegal entries. Figures on legal Mexican immigration (i.e., the annual number of permanent resident visas granted to Mexicans) were obtained from the Immigration and Naturalization Service (U.S. INS 1994), and figures on gross illegal Mexican entries are those published in Massey and Singer (1995). The index varies from zero to one and reaches a minimum when no legal visas are available to Mexicans and a maximum when all those seeking to enter the United States legally are able to do so.

Congress has also endeavored to increase the costs of migration by granting additional resources to the U.S. Border Patrol for enforcement purposes (Calavita 1992; Bean et al. 1994; Heyman 1995). As enforcement resources have gone up and down over time, so has the probability of apprehension (Espenshade and Acevedo 1995). According to the neoclassical model, raising the odds of getting caught should lower the expected returns to undocumented migration and thus deter Mexicans from deciding to leave for the United States (see Todaro and Maruszko 1987; Espenshade 1994). We used the estimates of the annual probability of apprehension along the southern U.S. border from Massey and Singer (1995).

In 1986, Congress also attempted to reduce the net returns to undocumented migration by imposing sanctions on employers who knowingly hire illegal workers. By lowering the odds of employment for undocu-

mented migrants, Congress hoped to reduce the expected value of U.S. wages and, in doing so, to reduce the expected gain from illegal entry. We thus include a dummy variable to indicate whether employer sanctions were in force during the person-year in question.

During the fall of 1994, voters in California joined Congress in trying to influence the distribution of costs and benefits associated with undocumented migration. Proposition 187 was approved by the electorate in November and sought to bar undocumented migrants from receiving public education, nonemergency medical services, and public assistance in the state of California. The proposition is premised on the assumption that publicly subsidized social services represent a potential benefit for undocumented migrants and, hence, an inducement to illegal entry.

In order to test this assumption, we include in our model the expected value to migrants of three U.S. social services: welfare and food stamps, medical services, and education. For our purposes, the expected value of a social service is the probability of receiving it times the average value of the benefit. During each year of a respondent's life, we estimated the probability of using welfare or food stamps, unreimbursed medical services, and public education while in the United States and then applied these estimated probabilities to average benefit values obtained from published sources.

The survey asked all household heads whether they or any member of their household had used each of the three services as of their last U.S. visit. These data were used to estimate a bivariate probit model that predicted the odds of using the service in question while controlling simultaneously for the probability of being in the United States. Estimated probabilities of service usage were then generated using the probit equations to predict the likelihood of receiving welfare or food stamps, medical services, and education during each year of a respondent's life, given his characteristics in that year and controlling for simultaneous selection into migrant status. The model predicts the odds of service usage from a respondent's age, the number of minors in the household, education, cumulative U.S. experience, documentation, whether the spouse had begun migrating to the United States, the number of children who had begun migrating to the United States, and whether or not the respondent reported children born in the United States.

The probit equations predicting service usage are shown in appendix table A2 (the simultaneously estimated migration selection equations are available on request). In general, the likelihood of using U.S. social services declines at a decelerating rate with age, rises with the number of minors in the household and level of education, and grows with increasing time spent in the United States. It also rises sharply if the spouse and children have begun migrating and if the respondent has children born

in the United States. The odds of using U.S. welfare or food stamps and schools increase with the receipt of documents, but the likelihood of using unreimbursed medical expenses declines with legalization. In general, these patterns confirm the results of field research carried out among Mexican migrants in the United States (Massey et al. 1987; Chavez 1992; Hondagneu-Sotelo 1994; Hagan 1994).

We computed the expected value of welfare and food stamps by multiplying the predicted usage probability times the average monthly value in constant 1990 dollars of Aid to Families with Dependent Children (AFDC) and food stamps in states that receive Mexican immigrants. We obtained annual data on the real value of AFDC and food stamps from Moffitt (1990) and computed a weighted annual average across states, where the weights were the proportion of Mexican immigrants going to each state (U.S. INS 1994).

We estimated the expected value of U.S. medical services by multiplying the probability of receiving unreimbursed medical care times the average monthly value of Medicaid payments in constant 1990 dollars. The latter data were again obtained from Moffitt (1990) and were arrayed by year and averaged across states using the proportion of Mexican immigrants going to each state as weights.

Finally, we estimated the expected value of public education by multiplying the probability of using public schools times the average per pupil expenditure in states receiving Mexican migrants. Per pupil expenditures were obtained by state and year from the U.S. Department of Education (1970–90), adjusted to constant 1990 dollars, and then averaged across states using the proportion of immigrants going to each state as weights.

### Social Capital Theory

Social capital refers to potential value that inheres in social relationships between people (Loury 1977; Coleman 1988, 1990). According to Bourdieu and Wacquant (1992, p. 119), "Social capital is the sum of the resources, actual or virtual, that accrue to an individual or a group by virtue of possessing a durable network of more or less institutionalized relationships of mutual acquaintance and recognition." Among people considering a trip to the United States, ties to current or former U.S. migrants represent a valuable social asset since these connections can be used to acquire information and assistance that reduce the costs and risks of entering the United States and raise the odds of getting a good U.S. job (see Browning and Rodríguez 1985; Massey et al. 1987; Massey 1990a, 1990b; Massey et al. 1994; Hondagneu-Sotelo 1994; Hagan 1994).

Although the concept of migrant networks can be accommodated within the cost-benefit model of neoclassical economics, social capital the-

ory yields insights not anticipated under neoclassical theory. According to economists such as Nelson (1959) and Dunlevy and Gemery (1977, 1978), networks act to facilitate the achievement of equilibrium between labor markets by transmitting information about wage opportunities. The analysis is static in the sense that networks do not influence the costs and benefits of migration *per se*; they serve only as conduits of information.

Social capital theory, in contrast, posits a direct connection between networks and the costs and benefits of migration, and it emphasizes the nonrecursive nature of the relationship between international movement and network formation. Nonmigrants are hypothesized to draw on the social capital embedded in ties to migrants to lower their costs and risks of movement and to raise their benefits of U.S. employment. As a result of these raised benefits and lowered costs and risks, some people decide to migrate, which expands the set of people with ties to the destination area, which, in turn, lowers the costs and risks and raises the benefits for a new set of people, causing some of them to migrate, and so on.

Over time, migration tends to become self-perpetuating because each act of migration creates additional social capital that promotes and sustains more migration, which creates more social capital, which produces more movement. The steady accumulation of social capital through the expansion of networks yields a feedback loop that is particularly powerful in the case of Mexican migration. Elsewhere, this self-feeding process is labeled "the cumulative causation of migration" (Massey 1990a); Reichert (1981) calls it the "migrant syndrome," and Alarcón (1992) refers to the "northernization" of Mexican communities.

We measure access to social capital in four ways. First we include a dummy variable indicating whether either of the respondent's parents had begun migrating to the United States by the person-year in question. Second, we include a count of the number of the respondent's siblings who had begun migrating during the year under observation. Third, we include a measure of social capital available within the community by estimating the proportion of persons 15 years old or older who had been to the United States during each person-year (computed using the procedure of Massey et al. [1994]).

Our last measure of social capital is listed under the policy rubric; that is, whether or not any member of the respondent's household had been legalized under the terms of the Immigration Reform and Control Act (IRCA) of 1986. At the time that Congress passed employer sanctions, it also authorized an amnesty program that ultimately legalized some 2.3 million undocumented Mexicans (U.S. INS 1994). This massive program created a new and potentially important source of social capital for people in Mexico because those receiving the amnesty were better able to sponsor the migration of friends and relatives at home.

The kinds of social capital considered to this point may accrue to any respondent, whether or not he has migrated himself. We also include three indicators of migration-specific social capital—ties to the United States that are typically acquired after someone has begun migrating. Although wives may move independently of husbands (either as single women or as members of parental households), the most common pattern is for them to begin migrating for purposes of family reunification or as part of a strategy of household survival negotiated (not always amicably) with the head and other family members (see Hondagneu-Sotelo 1994; Kanaiaupuni 1995). We thus include an index of whether or not the wife had begun migrating by the person-year under observation. We also include a count of the number of the respondent's children who had begun migrating, and a dummy variable to indicate whether or not any children had been born in the United States.

### The New Economics of Migration

The new economics of labor migration was developed by Stark and colleagues to counter the narrow focus of neoclassical economics on labor markets and wages (Stark and Bloom 1985; Stark and Levhari 1982; Stark and Taylor 1989, 1991; Taylor 1986, 1987; Stark 1991). In contrast to the neoclassical model, which assumes that all markets are complete and well functioning and that migrants move to take advantage of a temporary disequilibrium in geographically distinct labor markets, the new economics of migration assumes that key markets besides the labor market—futures, capital, and insurance—are imperfect, inaccessible, or nonexistent. Given these sorts of market failures, which are common in developing countries such as Mexico, people migrate not only to reap a higher stream of lifetime earnings but also to manage risk and gain access to capital that will enable them to finance consumer purchases and production activities. According to the new economics, in other words, the relevant economic variables in explaining migration are not wages but measures of risk and the need for and access to capital.

Controlling for fluctuations in the expected wage ratio, the principal risks to Mexican family income over the past decades have come in the form of price inflation and currency devaluation. The level of inflation is indicated by the rate of annual change in Mexican consumer prices, and the degree to which the Mexican peso is undergoing devaluation is measured by the annual rate of change in its rate of exchange with the U.S. dollar, both obtained from the IMF (1994). The overall accessibility of capital is indicated by the real interest rate, here defined as the difference between the average cost of funds reported to the IMF (1994) and the Mexican inflation rate.



The demand for capital is difficult to assess but is related to a variety of indicators included in our statistical model. Considerable work suggests that the acquisition of housing, the purchase of land, and the establishment of small businesses constitute primary motivations for international labor migration (see the reviews by Taylor et al. [1996a, 1996b]). If people migrate to finance these ends, then those who already own a home, land, or a business should have less need of capital and, hence, lower odds of migration. Thus we include dummy variables for these three categories of property ownership among our predictors.

The demand for capital is also likely to be influenced by community economic conditions. People who come from places with dynamic, entrepreneurial economies characterized by high wage rates, high levels of self-employment, and well-developed manufacturing are more likely to demand capital than people who come from economically stagnant areas. Likewise, people from communities with well-developed schools, roads, and banking institutions offer better opportunities for investment than places without such infrastructure.

Other things equal, we also expect agrarian economies to offer fewer opportunities for household enterprise than industrial or service economies, so we have included a dummy variable indicating whether or not the community economy was predominantly agricultural during the person-year in question (whether more than 50% of the community's labor force worked in agriculture). We also include indicators of demographic pressure on the land as well as land quality, hypothesizing that areas characterized by low rural densities and high-quality farmland offer more opportunities for agricultural investment.

Finally, in agrarian communities where an *ejido* was established, we hypothesize that farmers have a greater incentive to migrate because they have access to land but not the capital necessary to make it productive (see Massey et al. 1987). *Ejid*os were created by Mexico's postrevolutionary governments as part of an agrarian reform program. Parcels of land were allocated to specific families for their perpetual use but were held in common by members of the community. Until recently, *ejido* lands could be inherited but not rented, sold, or used as collateral on loans. The formation of *ejidos* thus created a demand for capital by giving poor campesinos access to land but not the credit required to engage in production (see Massey et al. 1987; Taylor 1992).

### Segmented Labor Market Theory

Segmented labor market theorists such as Piore (1979) argue that immigration is inherent to the structure of postindustrial economic life. According to this view, Mexico-U.S. migration is not caused by disparities

in wage rates, the effects of social capital, or failures in Mexican capital and insurance markets but is caused by a built-in demand for immigrant labor that is intrinsic to advanced industrial societies (see Massey et al. 1993). We assess the role of U.S. labor demand in generating migration from Mexico by including the annual rate of growth in U.S. employment as a predictor in the model. A better indicator would have been the growth rate of jobs in those sectors of the economy where Mexican immigrants are concentrated, but such data are not available by year. In general, we expect a surge in the rate of U.S. job creation to yield a higher effective demand for immigrant workers, boosting the odds that a Mexican worker will leave for the United States (holding constant expected wages).

### World Systems Theory

Building on the work of Wallerstein (1974), world systems theorists link the origins of international migration to the expansion of the global market economy (Portes and Walton 1981; Petras 1981; Castells 1989; Sassen 1988, 1991; Morawska 1990). They argue that the penetration of capitalist relations into peripheral societies such as Mexico creates a mobile population that is prone to migrate. World systems theory sees capitalist development as inherently disruptive, bringing about social and economic transformations that displace people from traditional livelihoods and force them onto transnational labor markets. We indicate capitalist penetration of Mexico by the rate of growth in direct foreign investment (see Sassen 1988). In his study of out-migration from 18 Caribbean nations, Ricketts (1987) found that growth in direct foreign investment strongly predicted annual emigration to the United States.

Like the new economics of migration, world systems theory is linked to community-level indicators as well. Both models see emigration as originating in communities that are in the throes of economic development rather than in backward, stagnant areas disconnected from national and international markets. Thus we expect the probability of U.S. migration to be greater in communities where wage rates, levels of self-employment, and the proportion of women employed in manufacturing are higher, and where an infrastructure of roads, schools, and banks has been established. At the community level, the new economics of migration and world systems theory converge in their predictions.

### THE INITIATION OF MIGRATION

As explained above, our analysis of how international migration is initiated follows respondents year by year from age 15 to the date of their first U.S. trip, age 65, or the survey date, whichever comes first. Person-years

lived before 1965 and after the first trip are excluded from the analysis, yielding a total of 55,762 person-years lived by 3,697 respondents. During each year the respondent did not migrate, the outcome variable was coded as "0," and during the year when the first trip occurred it was coded as "1" if the trip was made without documents and "2" if it was legal.

A multinomial logit model was used to regress this trichotomy on the set of independent variables described above, designating nonmigration as the reference outcome. We began the analysis in 1965 in order to minimize recall error among respondents and because that year represents a watershed in U.S. immigration history: not only did the U.S. Congress pass landmark amendments to the Immigration and Nationality Act that placed new restrictions on the entry of Mexicans and expanded legal immigration from Asia (Reimers 1985; Massey 1995); it also ended the Bracero Program, which over 22 years had imported some 4.5 million Mexicans as temporary agricultural workers (Calavita 1992).

Means and standard deviations were computed across person-years leading up to the first trip and are shown in the left-hand columns of table 3. During the years under study, the average respondent was 32 years old with 17 years of labor market experience; he was married (66% were in a union) with 2.2 children and 5.5 years of schooling. Even though none of the respondents had ever been to the United States, many had access to social capital connecting them to that country: 18% had a migrant parent; about a third had migrant siblings (the average number was .35); and the typical person lived in a community where 12% of all adults had been north of the border. The large majority of respondents were landless (only 6% reported owning farmland), but 29% possessed homes, and 14% reported owning a business.

Only 39% of the respondents lived in a community with a preparatory school, but over 90% lived in places served by a bank and a paved highway. In the typical community, about 21% of local workers earned twice the minimum wage, 34% were self-employed, and 19% of female workers were employed in manufacturing. A majority (62%) of the communities were agrarian and the average density was 8.5 persons per cultivable hectare. An average of around 68% of the local land base was arable, and 98% of the communities had established an *ejido*.

The macroeconomic indicators reveal Mexico's weak position relative to the United States. In the average person-year, expected wages in the United States were about 14 times those in Mexico, Mexican inflation ran at 26%, the peso had lost 34% of its value, and real interest rates hovered around 4%. On the U.S. side, the average rate of employment growth was 2% and direct foreign investment in Mexico had increased by 25% over the prior year. Most of these indicators display substantial variation across

time, yielding high standard deviations that suggest considerable economic instability.

The restrictive nature of U.S. immigration policy toward Mexico is indicated by several policy variables. In the average person-year, the supply of legal visas was sufficient to cover only 9% of Mexico's potential demand, thereby guaranteeing that a large share of the flow would be illegal. The average probability of apprehension at the border was .35 and employer sanctions had been implemented in 9% of the person-years under study. In a very few sample households (0.3%), someone had received amnesty under IRCA.

The bottom rows of the table suggest the potential value of social service benefits for Mexicans anticipating a first trip to the United States. In general, these values are not very large. The expected value of welfare plus food stamps is only \$21 per month, and the expected value of Medicaid is just \$4 per month; the value of public education is around \$131 per year. Recall that, in each case, the expected value is the probability of usage times the average benefit payment reported in major immigrant-receiving states.

Table 4 presents the results of a multinomial logit analysis of first trips to the United States. The left-hand columns show how different variables influence the odds of taking a first U.S. trip without documents, and the right-hand columns show effects on the odds of taking a first trip with documents. The vast majority of first trips (95%) were illegal, meaning that the respondent either crossed the border without inspection or entered with a tourist visa and then worked.

In general, the odds of taking a first illegal trip fall with marriage and age. As we hypothesized, moreover, the odds of undocumented migration tend to be reduced by having more education, reconfirming the pattern of negative selectivity found earlier (Massey and García España 1987; Taylor 1986, 1987; Borjas 1992). The effect of education, however, is weak ( $P > .08$ ).

Access to social capital, in contrast, substantially and significantly increases the odds of taking a first undocumented trip. People with ties to migrant family members and who live in communities where U.S. migration is prevalent are far more likely to migrate illegally than people without access to these social resources. In particular, having migrant parents and a large number of migrant siblings are both highly significant in raising the odds of undocumented migration, and living in a place where a relatively large number of community members have been to the United States is especially powerful in promoting undocumented movement. Thus, the evidence suggests that social capital plays a crucial role in initiating undocumented migration between Mexico and the United States.

TABLE 3

## MEANS AND STANDARD DEVIATIONS OF VARIABLES

	PERSON-YEARS UP TO FIRST U.S. TRIP		PERSON-YEARS UP TO ADDITIONAL U.S. TRIP		PERSON-YEARS UP TO RETURN FROM U.S. TRIP	
	Mean	SD	Mean	SD	Mean	SD
Demographic background:						
Age	31.97	12.87	38.66	12.23	33.52	10.69
Married	.66	.47	.86	.35	.76	.43
No. of minors in household	2.21	2.56	3.21	2.67	2.42	.27
General human capital:						
Labor force experience	16.59	13.40	24.12	12.67	18.55	11.19
Education	5.55	4.73	4.02	3.89	5.70	3.87
Migration-specific human capital:						
Cumulative U.S. experience			42.95	58.80	50.49	64.42
Months on current trip					62.79	80.74
No. of prior U.S. trips			3.96	4.77	3.55	5.18
Unskilled urban job			.19	.39	.45	.50
Skilled urban job			.22	.41	.30	.46
General social capital:						
Parent a U.S. migrant	.18	.38	.30	.46	.48	.50
No. of U.S. migrant siblings	.35	.83	1.37	1.64	2.03	1.87
% U.S. migrants in community	12	.05	.15	.06	.16	.05
Migration-specific social capital:						
Wife a U.S. migrant			16	.37	.56	.50
No. of U.S. migrant children			.50	1.35	.72	1.71
U.S.-born children			.08	.28	.41	.49
Physical capital:						
Land	.06	.24	.14	.35	.09	.29
Home	.29	.46	.43	.49	.31	.46
Business	.14	.35	.18	.39	.18	.38

Community infrastructure					
Preparatory school .....	39	49	31	46	35
Paved road .....	93	26	94	23	97
Bank .....	91	28	93	26	96
Community economic context:					
% earning twice minimum wage .....	21	11	22	11	24
% self-employed .....	34	12	35	11	34
% females in manufacturing .....	19	11	18	11	16
Community agrarian context:					
Agrarian economy .....	62	49	62	48	60
Agrarian population density .....	8.45	77.19	4.56	50.36	3.02
Proportion of land that is arable .....	68	30	61	29	57
Ejido established .....	98	12	97	18	97
Macroeconomic context:					
Expected wage ratio .....	13.91	30.57	20.55	58.85	22.93
Peso devaluation .....	34	65	44	70	45
Mexican inflation rate .....	26	24	32	26	33
U.S. employment growth .....	02	01	02	01	02
Growth in foreign investment .....	25	65	29	76	34
Mexican real interest rate .....	04	09	03	10	03
U.S. policy context:					
Availability of visas .....	09	08	07	08	06
Probability of apprehension .....	35	06	34	06	34
Employer sanctions enacted .....	09	29	15	36	17
Amnesty recipients in household .....	003	05	04	19	08
Expected value of U.S. services:					
Welfare .....	20.69	20.93	26.38	37.81	78.76
Medical care .....	3.56	4.27	4.05	5.88	12.04
Education .....	131.12	135.33	383.82	697.03	1,189.80
Total no. of person-years .....	55,762	27,813			13,150

TABLE 4

MULTINOMIAL LOGISTIC REGRESSION OF SELECTED VARIABLES ON THE ODDS OF  
TAKING A FIRST TRIP TO THE UNITED STATES IN YEAR  $t + 1$

SITUATION OF SUBJECT IN YEAR $t$	WITHOUT DOCUMENTS		WITH DOCUMENTS	
	B	SE	B	SE
Demographic background:				
Age .....	-.004	.031	-.055	.119
Age <sup>2</sup> .....	-.001*	.0004	.001	.001
Married .....	-.341*	.078	-.432	.444
No. of minors in household .....	.011	.020	-.005	.118
General human capital:				
Labor force experience .....	.013	.010	-.057	.040
Education .....	-.014	.008	-.002	.039
General social capital:				
Parent a U.S. migrant .....	.461*	.060	.720*	.263
No. of U.S. migrant siblings .....	.388*	.021	.676*	.073
% of U.S. migrants in community .....	5.016*	.817	-7.254	4.496
Physical capital:				
Land .....	.298*	.127	.759	.666
Home .....	-.446*	.093	-1.368	.759
Business .....	-.245*	.102	.400	.457
Community infrastructure:				
Preparatory school .....	-.249*	.075	-.061	.385
Paved road .....	-.107	.125	-.256	.527
Bank .....	.527*	.143	-.148	.549
Community economic context:				
% earning twice minimum wage .....	2.209*	.596	-7.730*	3.241
% self-employed .....	-.024	.412	-13.204*	2.490
% females in manufacturing .....	1.214*	.370	-6.337*	2.170
Community agrarian context:				
Agrarian economy .....	.480*	.078	2.034*	.765
Agrarian population density .....	-.001*	.0005	-.268	.155
Proportion of land that is arable .....	-.322*	.119	.214	.573
<i>Ejid</i> o established .....	.321*	.221	-2.880*	.892
Macroeconomic context:				
Expected wage ratio .....	.003*	.001	-.005	.008
Peso devaluation .....	-.115	.067	-.028	.376
Mexican inflation rate .....	-.702*	.298	2.744	1.472
U.S. employment growth .....	4.734*	1.938	11.637	10.220
Growth in foreign investment .....	-.228*	.067	.108	.351
Mexican real interest rate .....	2.264*	.531	-.842	2.490
U.S. policy context:				
Availability of visas .....	-2.828*	.511	-.568	1.965
Probability of apprehension .....	2.891*	.783	3.119	3.302
Employer sanctions enacted .....	.304*	.149	.135	.836
Amnesty recipients in household .....	2.561*	.353	4.656*	.874
Expected value of U.S. services:				
Welfare .....	-.019*	.006	.026	.017
Medical care .....	.019	.024	-.020	.066
Education .....	.002*	.0002	-.003	.015
Constant .....	-5.172*	.785	1.239	3.152
Log likelihood .....		6,648.100*		
$\chi^2$ .....		2,181.600*		
No. of person-years .....		55,762		

NOTE.—Event-history data gathered among male household heads from 25 Mexican communities.

\*  $P < .05$ .

The effects of physical capital generally are not consistent with expectations derived from neoclassical economics. Assets such as land, homes, and businesses theoretically provide a means of financing a trip to the United States, and according to the neoclassical model should yield a higher likelihood of migration, other things equal. Given two men with the same expected gain from working in the United States, the one with more capital assets should have a higher likelihood of going because he is in a better position to finance the trip and absorb the costs of moving and looking for work.

As table 4 reveals, however, the possession of homes and businesses tends to lower the odds of international movement, a pattern more consistent with the new economics of migration, which argues that people do not move to earn higher net incomes, but to secure capital to finance specific purchases. People who already own homes and businesses have less need of capital and therefore display lower likelihoods of undocumented out-migration. Only the possession of land seems to act in accordance with neoclassical theory by exhibiting a positive effect on the odds of undocumented movement. Land ownership, however, could also indicate a need for capital to engage in agricultural production, an interpretation that is consistent with the new economics of migration.

The decision to migrate without documents is apparently conditioned by two aspects of community infrastructure. Since the economic returns to education historically have been quite large within Mexico, a preparatory school provides an alternative mobility ladder that constitutes a realistic option to out-migration, lowering the odds of undocumented movement. The presence of a bank, in contrast, raises the odds of illegal migration by providing a convenient and secure place to accumulate, invest, and exchange dollars earned abroad. In our fieldwork, we found that banks in migrant-sending communities, especially those in rural areas, are generally awash with dollars deposited by returning migrants: one town of 3,500 people received \$1.4 million in remittances and savings during 1988 (Massey and Parrado 1994).

The effects of community economic indicators on the likelihood of illegal out-migration are also more consistent with expectations derived from the new economics of migration and world systems theory than with those derived from neoclassical economics. Living in a community with high levels of industrial development and wages does not reduce the likelihood of undocumented migration to the United States; on the contrary, high levels of development and wages *raise* it. The larger the proportion of workers earning twice the minimum wage and the higher the proportion of females working in manufacturing, the greater the odds of undocumented out-migration. Rather than offering employment alternatives to forestall out-migration, economic growth and development appear to create conditions that increase the need for foreign wage labor.



As expected, agrarian economies are generally more likely to send migrants to the United States than industrial/service economies. But within agrarian communities, the odds of undocumented migration are lowered by a higher quality land base: the higher the fraction of land that is cultivable, the lower the probability of out-migration. The odds of out-migration are increased by the presence of an *ejido*, however, and by lower agricultural densities. As explained earlier, *ejidos* create a need for capital by providing farm families with access to land but not the capital needed to begin or expand agricultural production. A low ratio of population to cultivable land, moreover, indicates an agricultural economy with room for expansion and, hence, a greater demand for capital to finance land acquisition and production.

In keeping with the fundamental hypothesis of neoclassical economics, the likelihood of undocumented migration is positively related to the U.S.-Mexico wage ratio: the higher the real value of expected wages in the United States relative to Mexico, the greater the odds of international out-migration. Contrary to the beliefs of most U.S. citizens and politicians, however, the wage ratio is not the leading predictor of Mexico-U.S. migration. The coefficient is rather small and does not compare favorably to other effects estimated in the model (a conclusion we document below).

Also contrary to what many in the United States believe, high rates of price inflation and peso devaluation in Mexico tend to reduce, not increase, the likelihood of taking an initial undocumented trip to the United States (although the effect of devaluation is only significant at the .09 level). By increasing the costs of entry for those possessing pesos, inflation and devaluation lower the odds that people without prior migration experience will attempt to enter the United States without documents because surreptitious border crossing requires paying a sizable fee in dollars to a smuggler, or "coyote."

Macroeconomic indicators also provide some support for a key prediction of segmented labor market theory—that undocumented migration is fomented by U.S. labor demand. As table 4 indicates, an increase in the rate of U.S. employment growth tends to be followed by an increase in the likelihood of illegal migration, suggesting that Mexicans do, in fact, respond to labor market opportunities when they decide to begin migrating to the United States.

The main prediction of the new economics of labor migration also receives strong support from macroeconomic indicators. According to the new economics, the higher the real interest rate at any point in time, the less access people have to credit and capital and the more they select labor migration as a means of acquiring the funds they need to finance consumer purchases and productive activities. The coefficient associated with the real interest rate is positive and highly significant, indicating that the ris-

ing real cost of capital sharply increases the odds of undocumented movement.

Only one theoretical model does not receive much direct support from the macroeconomic indicators: world systems theory. Its leading hypothesis is that out-migration stems from capital penetration, but our results indicate that the rate of growth in direct foreign investment does not increase the odds of undocumented migration, rather, it reduces it. If we could have measured capital penetration at the community level, results might have been different, but at the national level, at least, direct foreign investment appears to lower the likelihood of international movement, possibly by creating jobs that provide domestic alternatives to emigration.

Finally, we consider whether recent U.S. policy initiatives have influenced the likelihood of undocumented movement. The strong negative effect of visa availability suggests that, to a significant degree, the secular increase in undocumented migration from Mexico reflects U.S. actions to restrict the supply of visas during a period when demand was rising. The coefficient clearly shows that a larger supply of visas reduces the odds of undocumented movement and directs a larger share of the migratory flow into legal channels.

At the same time, recent policies intended to deter undocumented migration appear to have failed; indeed, they may have backfired. According to our estimates, the imposition of employer sanctions and the concentration of enforcement resources along the border *increased* the odds of taking a first illegal trip to the United States. Prospective migrants appear to interpret a crackdown as evidence of even more stringent policies to follow and seek to gain entry while they still can. Likewise, employer sanctions may encourage migrants already in the United States to hang onto jobs they have for fear of not getting others later on, thereby creating greater pressure for the migration of undocumented family members.

Recent policy initiatives have backfired in another way. When Congress implemented employer sanctions at the end of 1986, it also enacted a large-scale amnesty program, and the subsequent legalization of millions of former undocumented migrants appears to have encouraged additional illegal migration by relatives who remained at home. As table 4 indicates, being from a household where someone legalized under IRCA greatly increases the odds of taking a first illegal trip to the United States.

Finally, coefficients shown in the bottom rows of the table do not provide much encouragement for the backers of California's Proposition 187, who are attempting to discourage migration by withholding state-funded social welfare programs from undocumented residents. If the assumptions underlying this referendum are correct, then we would expect to observe a positive relationship between service benefits and the odds of undocumented migration. The effect of welfare benefits, however, is negative,

and the effect of medical benefits is statistically insignificant. The only effect consistent with the assumptions underlying Proposition 187 is the expected value of U.S. schools, but its effect is rather weak. Given one insignificant and two significant but offsetting effects, the implementation of California's Proposition 187 can be expected to have little overall influence on the arrival of new undocumented migrants.

The right-hand columns of table 4 analyze *legal* first migration. Taking a first trip as a documented migrant is quite unusual and is accomplished in one of two ways: having skills or abilities in high demand in the United States or being related to someone who already has legal papers. Since nearly all the migrants in our sample lack the skills or education to qualify them for the first route, the most frequent means of legal entry is the latter. In fact, the typical first-time legal migrant is the son of a father who legalized earlier.

Relatively few variables are significant in predicting first trips with documents, owing to the small number of people in this category (just 5%). The significant effects generally fall into three categories. First, documented out-migration is strongly influenced by social capital. Consistent with the means by which most Mexicans obtain documents, the odds of taking a first trip with documents are markedly higher for those who have parents or siblings who are U.S. migrants or who come from a household where someone received amnesty. Second, in contrast to the case of undocumented migration, community economic circumstances are *negatively* related to the odds of documented movement. A high wage rate, a high rate of self-employment, and a high proportion of women in manufacturing all lower the odds of legal migration. Finally, the likelihood of documented migration is increased by coming from an agrarian community, although it is decreased if that community also has an *ejido*.

These findings suggest that documented migration occurs on a first trip mainly when household heads sponsor the legal immigration of their sons (and to a lesser extent their brothers) and that such sponsorship is especially likely in rural communities but not in those with an *ejido* or with a strong local economy. Given access to communal farmland and a strong economy, household heads appear less amenable to soliciting documents for their sons and brothers, possibly because they see a brighter future for them in Mexico.

Results to this point suggest that all the theories of international migration are technically "correct" in the narrow sense that many of their hypotheses are consistent with the data. In accordance with neoclassical model, the likelihood of illegal migration is positively related to the U.S.-Mexico wage differential. Being related to a migrant sharply boosts the odds of taking a first U.S. trip, which is consistent with social capital theory. In keeping with the new economics of migration, high real interest

rates increase the probability of undocumented movement. Following segmented labor market theory, undocumented migration is linked to the growth of U.S. employment. And, consistent with world systems theory (as well as the new economics of migration), the odds of undocumented migration are greatest in dynamic, developing communities, not stagnant areas with low wages and marginal levels of industrialization.

More interesting than the technical correctness of each theory, however, is the issue of which one is most powerful in explaining undocumented migration to the United States. The relative strength of the effects is difficult to judge from logit coefficients alone, as each variable has a different scale of measurement. We therefore used the logit model of table 4 to generate predicted probabilities of first undocumented migration, which are presented in the two left-hand columns of table 5. Owing to the small number of legal migrants, we do not generate predicted probabilities for them.

In generating these figures, we hold age and labor force experience constant at 18 and 3 years, respectively, to consider the situation of a young man coming of age in the communities under study. In each line of the table, we generate predicted probabilities under two assumptions: one when the variable in question is at its fifth percentile, representing an effective minimum, and the other when the variable takes its ninety-fifth percentile, representing an effective maximum. We assume mean values for all other variables. Each line in the table therefore shows the range of probabilities that result when the variable in question goes from its fifth percentile (minimum) to its ninety-fifth percentile (maximum) while holding all other variables constant at their means. Ranges that are more than 50% of the mean probability are marked with an asterisk to indicate unusually strong effects.

The average yearly probability of taking a first undocumented trip is .04, meaning that in any given year the average 18-year-old man in our sample has a 4% chance of taking a first trip to the United States without documents. If 1,000 young men were to go through life subject to this risk of migration, 388 would migrate by age 30. This probability, however, shifts up and down in response to several important variables.

The most powerful effects are generally observed in response to variation in the quantity of social capital at a man's disposal. Having parents with prior U.S. experience boosts the probability of undocumented migration from .037 to .057, and varying the number of U.S. migrant siblings from the fifth percentile (zero migrant siblings) to the ninety-fifth percentile (two migrant siblings) more than doubles the probability of first undocumented movement from .035 to .073. Likewise, coming from a community where just 3% of the adults have been to the United States (the fifth percentile) yields a predicted migration probability of .027, but com-

TABLE 5

## EFFECT OF INDEPENDENT VARIABLES ON UNDOCUMENTED MIGRATION

INDEPENDENT VARIABLE	PROBABILITY OF FIRST MIGRATION		PROBABILITY OF REPEAT MIGRATION		PROBABILITY OF RETURN TO MEXICO DURING FIRST YEAR OF MIGRATION	
	Minimum	Maximum	Minimum	Maximum	Minimum	Maximum
<b>Demographic background:</b>						
Married	.050	.036	.477	.425	.280	.328
No. of minors in household	.039	.043	.378	.517	.317	.300
<b>General human capital:</b>						
Education	.043	.036	.465	.377	.379	.249
<b>Migration-specific human capital:</b>						
Cumulative U.S. experience			.321*	.746*	.524*	.035*
No. of prior U.S. trips			.312*	.841*	.181*	.880*
Unskilled urban job			.414	.514	.361	.256
Skilled urban job			.428	.450	.335	.267
<b>General social capital:</b>						
Parent a U.S. migrant	.037*	.057*	.416	.471	.299	.329
No. of U.S. migrant siblings	.035*	.073*	.431	.438	.326	.285
% U.S. migrants in community	.027*	.062*	.366	.538	.299	.331
<b>Migration-specific social capital:</b>						
Wife a U.S. migrant			.381*	.702*	.345	.268
No. of U.S. migrant children			.423	.498	.348*	.143*
U.S.-born children			.410	.548	.310	.321
<b>Physical capital:</b>						
Land	.039	.052	.437	.405	.298*	.519*
Home	.045	.030	.468	.388	.302	.355
Business	.041	.032	.460	.316	.316	.276

Community infrastructure:						
Preparatory school .....	.044	.035	.421	.459	.298	.335
Paved road .....	.044	.040	.474	.430	.325	.312
Bank .....	.025	.042	.450	.431	.234	.316
Community economic context:						
% earning twice minimum wage .....	.028*	.058*	.408	.456	.426*	.235*
% self-employed .....	.040	.040	.427	.441	.257*	.424*
% females in manufacturing .....	.036*	.060*	.438	.412	.359*	.144*
Community agrarian context:						
Agrarian economy .....	.028*	.049*	.402	.451	.340	.297
Agrarian population density .....	.040	.039	.434	.432	.312	.313
Proportion of land that is arable .....	.047	.036	.445	.422	.304	.321
<i>Ejido</i> established .....	.029	.040	.430	.433	.384	.311
Macroeconomic context:						
Expected wage ratio .....	.039	.044	.429	.443	.313	.311
Peso devaluation .....	.042	.035	.435	.420	.315	.300
Mexican inflation rate .....	.047	.029	.495	.325	.250*	.425*
U.S. employment growth .....	.035	.044	.401	.454	.294	.327
Growth in foreign investment .....	.049	.032	.475	.389	.342	.281
Mexican real interest rate .....	.028*	.056*	.368	.493	.261	.368
U.S. policy context:						
Availability of visas .....	.048*	.022*	.472	.280	.330	.252
Probability of apprehension .....	.030	.049	.480	.390	.314	.311
Employer sanctions enacted .....	.039	.052	.446	.359	.308	.359
Amnesty recipients in household .....	.040*	.350*	.416*	.806*	.311	.331
Expected value of U.S. services:						
Welfare .....	.055*	.020*	.753*	.022*	.405	.190
Medical care .....	.037	.045	.271*	.786*	.341	.266
Education .....	.032	.051	.460	.353	.341	.253
Mean .....	.040			.433		.312

NOTE.—Probability of first migration at age 18, probability of repeat migration at age 25, and probability that a 25-year-old will return to Mexico during the first year of migration.

\* Ranges greater than 50% of mean probability

ing from one where 21% have been north of the border (the ninety-fifth percentile) increases it to .062. Finally, being in a household where someone received amnesty under IRCA raises the yearly probability of undocumented migration from .040 all the way to .350.

As social capital accumulates in various forms, therefore, the odds of undocumented migration become rather high. For someone with a migrant parent, with two migrant brothers, and living in a community where 21% of adults have been to the United States, the probability of taking an undocumented trip at age 18 is .16 (other factors held constant at their means); and, if someone in that young man's household were to receive amnesty under IRCA, his likelihood of undocumented migration would go up to .71.

A second set of effects stand out in the table: those pertaining to economic conditions in the community. As the proportion earning twice the minimum wage moves from the fifth percentile (5%) to the ninety-fifth percentile (39%), the probability of undocumented migration grows from .028 to .058, and, as the share of females in manufacturing moves from its effective minimum (10%) to its effective maximum (55%), the likelihood goes from .036 to .060. Holding constant these economic conditions, men from communities whose economies are predominantly agrarian have a higher probability of illegal out-migration (.049) compared with men who come from communities with industrial/service economies (.028).

In general, therefore, the highest probabilities of out-migration are observed in rural communities undergoing rapid economic growth and development. In an agrarian community where 51% of workers are employed in agriculture but where 39% earn twice the minimum wage and the proportion of women working in manufacturing is 55%, the probability of illegal out-migration is .11. The economic transformation of the countryside creates rather than prevents international migrants.

Several communities in our sample fit this description. One community, for example, is a small agrarian town in the highlands of Jalisco with good highway access to the metropolitan areas of Guadalajara and León. In this small community of just over 3,000 people, local entrepreneurs have established several factories to produce cheese and Christmas ornaments, with the latter employing an exclusively female workforce (Arias and Durand 1988). Wages are kept high through competition with the booming industrial city of San Francisco del Rincón, which is within daily commuting distance (Arias 1992).

The third powerful factor predicting undocumented migration to the United States is access to capital and credit in Mexico, as indicated by the real interest rate. At the fifth percentile, the real interest rate is -13%,

indicating that money can be paid back cheaply in inflated pesos worth less than those originally borrowed. Under these circumstances, the cost of capital is low, yielding a relatively low predicted probability of undocumented migration, just .028. At the ninety-fifth percentile; however, the real interest rate is 19%, indicating a high real cost to borrowing that yields a high predicted probability of .056.

In addition to the presence of an amnesty recipient in the household, two other policy-related variables have relatively strong effects on the likelihood of undocumented migration. According to table 5, the relative availability of U.S. visas effectively channels Mexican migrants in and out of documented status. As the index of visa availability moves from its fifth to its ninety-fifth percentile and the supply of entry visas shifts from 2% to 30% of potential demand, the annual probability of undocumented migration drops from .048 to .022.

In addition, as the expected value of U.S. welfare increases from \$3 to \$59, a range that reflects variation in the odds of usage rather than shifts in the value of payments (AFDC and food stamps have declined in real terms since the early 1970s), the probability of migration drops from .055 to .022. Contrary to the assumptions of those supporting Proposition 187, therefore, Mexicans who stand to reap the highest gains from the U.S. welfare system are the least likely to migrate illegally. The same factors that make them more likely to use U.S. welfare (in particular, rising education) simultaneously function to keep them in Mexico.

In summary, the forces initiating undocumented migration between Mexico and the United States are not those that most policymakers and citizens think of when they consider the issue. Illegal migration does not appear to be driven much by the lure of high wages or generous welfare benefits north of the border, by the forces of inflation and devaluation in Mexico, or by poverty and a lack of development in sending communities. While these factors play a role in promoting international migration, they are not among the most powerful determinants, and, in the case of economic development, the effect is opposite that popularly believed (in keeping with Massey [1988]).

Our analysis suggests that migration is initiated when rural Mexican communities undergo dynamic development and industrialization, which raises wages and draws women into manufacturing. These transformations usher in a period of uncertainty and change in local economic life that creates a need for capital and risk management on the part of ordinary households, which, given the high real interest rates and market failures that prevail in Mexico, is satisfied through international migration. Transnational movement is enabled and facilitated by social connections to people who have already made the trek northward. Whether the migra-



tion is observed as legal or illegal depends largely on the supply of visas made available by the United States and by the number and range of kinship ties that Mexicans have to legal resident aliens and U.S. citizens.

#### THE CONTINUATION OF MIGRATION

In theory, the same factors responsible for initiating international migration could also explain its perpetuation over time, but, once someone has crossed the border and lived and worked in the United States, new and potentially more powerful forms of human and social capital come into play. In addition to personal traits and social ties that anyone in Mexico might possess, the process of migration itself creates new and more specific forms of human and social capital accessible only to those who have made the journey northward.

The middle column of table 3 suggests the advantages enjoyed by experienced migrants compared to those who have not yet made a trip northward. It shows means and standard deviations computed across person-years spent in Mexico before the next U.S. trip. During the average person-year, respondents contemplating an additional U.S. trip had some 43 months of experience in the United States accumulated over the course of four prior trips. Some 19% had attained a skilled U.S. job by the time of their last visit, and another 22% held an unskilled job (with 59% remaining in agriculture).

In addition to this migration-specific human capital, migrants also reported considerable migration-specific social capital. On average, 16% of the respondents had spouses who had begun to migrate, 8% stated that a child had been born in the United States and that other children (on average, 0.5) had begun migrating to the United States. Experienced migrants also had access to a larger quantity of general social capital than nonmigrants: 30% reported having a migrant parent and the average number of migrant siblings was 1.37 (compared to figures of just 18% and 0.35 for nonmigrants contemplating their first trip).

As a result of this greater quantity of human and social capital, the potential rewards of U.S. migration grew. The ratio of expected wages stood at 21 for experienced migrants compared to just 14 for novices considering their first trip. Likewise the expected benefit of welfare rose from \$21 to \$26, while the expected value of education grew from \$131 to \$384; the expected value of medical care stayed roughly the same at \$4.05 (it was \$3.56 before the first trip).

According to the theory of social capital, migration-specific human and social capital combine to perpetuate international migration. Table 6 examines this hypothesis by performing a multinomial logit analysis of the odds of taking an additional U.S. trip, documented or undocumented,

TABLE 6

MULTINOMIAL LOGISTIC REGRESSION OF SELECTED VARIABLES ON THE ODDS OF  
TAKING AN ADDITIONAL TRIP TO THE UNITED STATES IN YEAR  $t + 1$

SITUATION OF SUBJECT IN YEAR $t$	WITHOUT DOCUMENTS		WITH DOCUMENTS	
	B	SE	B	SE
Demographic background:				
Age .....	-.156*	.021	-.005	.034
Age <sup>2</sup> .....	.001*	.0003	-.001	.001
Married .....	-.207*	.057	.004	.107
No. of minors in household .....	.071*	.012	.041*	.020
General human capital:				
Labor force experience .....	-.076*	.008	-.041*	.014
Education .....	-.033*	.007	.029*	.011
Migration-specific human capital:				
Cumulative U.S. experience .....	.012*	.001	.012*	.001
No. of prior U.S. trips .....	.176*	.008	.226*	.008
Last U.S. job unskilled urban .....	.404*	.052	.919*	.093
Last U.S. job skilled urban .....	.093*	.005	.354*	.087
General social capital:				
Parent a U.S. migrant .....	.224*	.043	.452*	.076
No. of U.S. migrant siblings .....	.006	.013	.090*	.020
% of U.S. migrants in community .....	2.992*	.558	6.430*	.956
Migration-specific social capital:				
Wife a U.S. migrant .....	1.340*	.118	2.482*	.163
No. of U.S. migrant children .....	.075*	.031	.304*	.040
U.S.-born children .....	1.114*	.138	1.376*	.164
Physical capital:				
Land .....	-.134	.071	.382*	.095
Home .....	-.327*	.048	-.324*	.079
Business .....	-.611*	.064	-.500*	.100
Community infrastructure:				
Preparatory school .....	.158*	.060	-.236*	.102
Paved road .....	-.177	.101	-.537*	.173
Bank .....	-.078	.097	-.021	.156
Community economic context:				
% earning twice minimum wage .....	.618	.389	-5.066*	.677
% self-employed .....	.143	.305	-6.107*	.582
% females in manufacturing .....	-.211	.253	-.732	.440
Community agrarian context:				
Agrarian economy .....	.200*	.061	.346*	.107
Agrarian population density .....	-.001	.001	-.001	.002
Proportion of land that is arable .....	-.113	.099	.968*	.169
<i>Ejido</i> established .....	.088	.133	-1.317*	.180

TABLE 6 (Continued)

SITUATION OF SUBJECT IN YEAR <i>t</i>	WITHOUT DOCUMENTS		WITH DOCUMENTS	
	B	SE	B	SE
<b>Macroeconomic context</b>				
Expected wage ratio .....	.001	.001	-.012*	.002
Peso devaluation .....	-.023	.040	-.009	.008
Mexican inflation rate .....	-.883*	.191	-.004	.331
U.S. employment growth .....	4.344*	1.462	4.440	2.691
Growth in foreign investment .....	-.167*	.048	-.157*	.078
Mexican real interest rate .....	1.593*	.375	2.142*	.656
<b>U.S. policy context</b>				
Availability of visas .....	-2.900*	.409	1.617*	.639
Probability of apprehension .....	-2.182*	.527	1.923*	.824
Employer sanctions enacted .....	-.364*	.096	.235	.160
Amnesty recipients in household .....	1.767*	.143	3.748*	.160
<b>Expected value of U.S. services:</b>				
Welfare .....	-.060*	.003	.043*	.020
Medical care .....	.186*	.011	-.190*	.012
Education .....	-.0003*	.0001	-.002*	.0001
Constant .....	3.892*	.558	-1.309	.000
Log likelihood .....		11,829.000*		
$\chi^2$ .....		18,059.000*		
No. of person-years .....		27,813		

NOTE.—Event-history data gathered among male household heads from 25 Mexican communities

\*  $P < .05$ .

given that at least one had already occurred. After each trip to the United States, male household heads were followed year by year up to the point at which they took their next U.S. trip, turned 65, or reached the survey date. All time spent in the United States was excluded from the analysis, as were years before 1965, yielding 27,813 person-years lived by 2,091 men. The year the additional trip was taken was coded as "1" if the trip was undocumented and "2" if documented, and all years spent in Mexico preceding the trip were coded as "0."

In general, our expectations about the importance of migration-specific human and social capital are borne out by the analysis: all indicators under these rubrics have strong and highly significant effects on the odds of taking an additional U.S. trip. Under the category of migration-specific human capital, the odds of repeat migration progressively rise as the amount of prior U.S. experience grows and as the number of U.S. trips increases. The effect of both variables is roughly the same for legal and undocumented migrants. Clearly, then, the more one migrates, the more one is likely to continue migrating, suggesting the self-perpetuating nature of the process.

The relative likelihood of repeat migration is also boosted substantially by occupational achievement in the United States. The shift out of agriculture into an urban occupation significantly increases the odds of undertaking another U.S. trip, and the shift from an unskilled to a skilled job raises these odds even more. The effect of occupational achievement interacts significantly with legal status, however: job skill has a stronger effect in promoting additional trips among those with legal documents. A lack of documents inevitably places a ceiling on job mobility that appears to diminish the effect of prior occupational achievement on the likelihood of repeat migration.

As expected, the likelihood of taking another U.S. trip is strongly enhanced by the acquisition of migration-specific social capital. Having a wife and children who have been to the United States and having children born in the United States substantially raise the likelihood of taking an additional U.S. trip. Once again the effects are strongest for documented migrants, suggesting that a long-term commitment to the United States is both more attractive and more viable for those with documents.

Once indicators of migration-specific human and social capital are taken into account, variables that were crucial in determining the odds of taking a first trip become much less important in predicting the odds of making subsequent visits. Whereas general social capital was the most powerful force predicting first undocumented trips, its role in predicting subsequent visits is marginal. The coefficient for having a migrant parent drops by half (from .461 to .224, a significant shift;  $P < .05$ ), and the coefficient for the prevalence of migrants falls by 40% (from 5.016 to 2.992;  $P < .05$ ). Finally, the coefficient for the number of U.S. migrant siblings falls into insignificance (.006, compared to a robust .388 in the model for first trips).

We observe similar declines in the importance of community economic indicators, which were the second most important set of variables in determining the odds of first undocumented trips. Neither the proportion earning twice the minimum wage nor the proportion of females in manufacturing has any significant effect in predicting the odds of taking an additional undocumented trip, and the effects of these variables are significantly reduced in predicting additional documented trips as well. Likewise, the effect of an agrarian economy is lower in predicting additional trips than first trips, a pattern that holds for documented as well as undocumented migrants.

Among variables that were important in predicting the odds of a first trip, only the real interest rate and the existence of amnesty recipients seem to retain their explanatory power in predicting later visits. In keeping with expectations derived from the new economics of labor migration, a high real interest rate substantially increases the odds of making an

additional U.S. trip, legal or illegal; consistent with the theory of social capital, granting amnesty to one family member greatly raises the odds of additional migration by other family members, both documented and undocumented.

The negative selectivity of undocumented migration with respect to general human capital shows up very clearly in predicting additional trips. As labor force experience and education increase, the odds of taking another undocumented trip fall sharply, reflecting the fact that human capital is more likely to be rewarded within Mexico than in the United States among those without legal documents (see Massey and García España 1987; Taylor 1986, 1987).

A pattern of demographic selectivity also emerges quite clearly among undocumented migrants. The likelihood of taking an additional U.S. trip declines at a decelerating rate with age, falls with marriage, and rises as the number of minors increases. Thus, the making of repeated undocumented trips is associated with changes in the family life cycle, rising for young, unmarried men, falling with marriage, and then increasing again as children and a rising dependency burden increase the household's consumption needs (see Massey et al. 1987).

In keeping with expectations derived from the new economics of labor migration, the ownership of land, home, and businesses—prime targets of migrant investment—sharply lower the odds of taking another undocumented trip (although the coefficient for land just misses significance at  $P = .058$ ). If one already possesses these assets, then the need for capital is lower and the motivation for additional migration is correspondingly reduced. The possession of a home and business also sharply lowers the odds of repeat migration by documented migrants.

Once again we find modest support for segmented labor market theory. As expected, higher labor demand in the United States is associated with a higher likelihood of repeat migration. Growth in aggregate U.S. employment significantly boosts the odds of taking an additional trip, documented or undocumented (although the effect for legal migrants is only significant at  $P = .098$ ). Once again, however, there is little support for the leading hypothesis of world systems theory: growth in direct foreign investment is associated with *lower* probabilities of repeat migration among both legal and illegal migrants.

Finally, our analysis of repeat migration reveals little support for the principal hypothesis of neoclassical economics. The ratio of expected wages in the United States and Mexico has no effect on the odds of additional migration among undocumented migrants, and among legal migrants the effect is *negative*. Likewise, expected values of social services tend to have contradictory and offsetting effects. Among undocumented migrants, a rising expected medical benefit increases the likelihood of ad-

ditional migration, but greater expected welfare and educational benefits lower it. Among documented migrants, an increase in expected welfare benefits is associated with a higher likelihood of repeat migration, but greater medical and educational benefits yield lower odds of taking an additional U.S. trip.

There is some evidence that recent U.S. immigration policies have influenced the propensity for repeat migration. As with first trips, the availability of visas plays a very significant role in determining the odds of taking an additional trip: as visas become more available, the odds of undocumented migration fall while the odds of documented migration rise. As mentioned before, the U.S. policy of restricting the number of visas works mainly to channel the ongoing flow of Mexican migrants from documented to undocumented status; it does not stop the flow itself.

Whereas border enforcement and employer sanctions backfired in determining the odds of first migration (increasing the propensity to migrate illegally), the same policies appear to be more successful in reducing the odds of taking additional undocumented trips. As the probability of border apprehension rises, the likelihood of additional undocumented migration falls. Likewise, the implementation of employer sanctions reduces the likelihood of repeated undocumented movement.

Whether these effects translate into meaningful reductions in the probability of undocumented migration is considered in table 5, which presents predicted probabilities of taking an additional trip to the United States without documents. (To conserve space, we do not present predicted probabilities of documented migration.) These figures were computed using the same approach that we used to generate predicted probabilities of first migration, except that we now assume a more experienced person who is 25 years of age with 10 years of experience in the labor market.

Given the greater store of human and social capital accruing to experienced migrants, we generally expect the probability of taking an additional U.S. trip to be higher than the probability of taking a first trip, and this supposition is amply borne out by the predicted probabilities. Whereas the mean probability of going to the United States without documents was .04 initially, it is 10 times greater at .433 for additional trips. Given this risk of repeat migration, 1,000 men returning to Mexico from the United States would have a 94% chance of going again within five years.

As before, the probability of out-migration shifts upward and downward in response to several key variables. The most powerful effects on the likelihood of repeat migration are associated with two indicators of migration-specific human capital: months of prior U.S. experience and number of U.S. trips. As the former moves from its fifth to its ninety-fifth percentile (from two months to 159 months of U.S. experience), the probability of taking an additional illegal trip more than doubles, going

from .321 to .746. Likewise, as the latter goes from its fifth to its ninety-fifth percentile (from one to 15 U.S. trips), the probability of repeat undocumented migration rises from .312 to .841, other factors held constant at their means.

Although indicators of general social capital still have a positive influence on the odds of U.S. migration, their effect is not very strong. Only the presence of a legalized family member retains its high explanatory power. Coming from a household where someone received amnesty under IRCA nearly doubles the probability of repeat migration from .416 to .806. In general, however, migration-specific social capital is far more important in predicting the odds of repeat migration than general social capital, and the migration of the spouse is particularly important: having a migrant wife increases the odds of taking another undocumented trip from .381 to .702. Having U.S.-born children likewise raises the probability from .366 to .538.

In two cases, expected values of social services have rather large effects on probabilities of repeated undocumented migration, but they work in opposite directions. Whereas shifting the value of welfare from its fifth to ninety-fifth percentile (from \$3.11 to \$85.54) lowers the probability of taking another undocumented trip from .753 to .022, making the corresponding shift in the value of medical care (from \$0.20 to \$12.49) raises the likelihood from .271 to .786. Thus, the knowledge that they may benefit from medical services while in the United States may play some role in promoting the decision of undocumented Mexican migrants to take another trip, especially since spouses and children are more likely to accompany them. Given the offsetting effects of expected welfare and medical benefits on the odds of repeat illegal migration, however, the net effect of Proposition 187 on the flow of migrants is likely to be nil.

A few predicted probabilities that do not satisfy the criteria for acquiring an asterisk in table 5 deserve comment. First, the effect of the real interest rate, although not as powerful as in the model predicting first trips, still has a modest effect in raising the probability of repeat migration. As the real interest rate moves from its fifth to its ninety-fifth percentile, the probability of taking an additional undocumented trip rises from .368 to .493. Second, the availability of visas continues to exert a rather strong effect on the odds of additional undocumented migration. As the availability index moves from its fifth to its ninety-fifth percentile (.02 to .30), the probability of undocumented migration drops from .472 to .280.

Finally, although recent policy initiatives would seem to move the probabilities of repeat undocumented migration in the direction expected by the U.S. Congress, the effects are modest, roughly on the same order as the effect of real interest rates. According to our estimates, increasing the probability of apprehension from .26 to .42 (moving it from the fifth to

the ninety-fifth percentile) lowers the likelihood of repeat migration from .48 to .39, which is still quite high, and implementing employer sanctions reduces the probability of taking an additional undocumented trip from .446 to .359.

Taken together, currently popular U.S. policy actions seem likely to backfire. Suppose, for example, that we assume the most stringent package of immigration policies imaginable by setting the index of visa availability to its observed minimum (.017), increasing the apprehension probability to its observed maximum (.435), reducing the expected value of all services to zero, and retaining employer sanctions. Given the fact that reducing the supply of visas and reducing the expected values of welfare and education are predicted to *increase* the likelihood of taking another undocumented trip, this set of policies is predicted to increase the probability of repeat migration from .433 to .578.

Suppose instead that we hold visa availability and the expected values of welfare and education constant at their means while increasing the apprehension probability to its observed maximum and implementing employer sanctions. This scenario does, in fact, reduce the predicted probability of taking an additional undocumented trip from .433 to .177. But this figure represents an *annual* probability that is still quite high: out of a cohort of 1,000 young men subject to an 18% annual chance of repeat migration, 623 would take another undocumented journey to the United States within five years.

Whatever the modest effects of border enforcement and employer sanctions are, moreover, they are likely to be offset by the more powerful effects of human and social capital accumulation arising out of the migration process itself. Suppose that, in addition to raising the apprehension probability to its maximum and implementing employer sanctions, we allow indicators of migration-specific human and social capital to assume their ninety-fifth percentile values (not even their absolute maxima). Under these assumptions, the annual probability of taking an additional undocumented trip rises to .989, irrespective of the stringent immigration policies we have assumed.

#### THE PROCESS OF RETURN MIGRATION

A full understanding of Mexico-U.S. migration not only requires analyzing the process of entry, we also must consider the process of return. Jasso and Rosenzweig (1982) estimate that as many as 56% of the 1971 cohort of legal Mexican immigrants may have left the United States by 1979; and numerous field studies indicate that large numbers of legal immigrants continue to maintain homes, families, and businesses in Mexico while moving back and forth seasonally (see Reichert and Massey 1979; Mines



1981; Dinerman 1982; López 1986; Massey et al. 1987; Goldring 1992; Durand and Massey 1992). As for undocumented migrants, Massey and Singer (1995) estimate that 86% of all illegal Mexican entries over the past 25 years have been offset by departures.

Thus, implied in every decision to enter the United States is a corresponding decision about whether to stay or return, and this decision can be as important as those about taking first or later trips in determining the course of international migration. In order to derive a complete understanding of the process of Mexico-U.S. migration, therefore, we conducted an event-history analysis of the decision to return. Migrants in the United States from 1965 through 1989 were followed year by year from the point of entry until their return to Mexico or the survey date, whichever came first. During each year the migrant did not go home to Mexico, we coded the outcome as "0" and we assigned a value of "1" during the year in which a return trip occurred.

Means and standard deviations for independent variables were computed across person-years spent in the United States and are presented in the right-hand columns of table 3. Compared to respondents observed in Mexico, those captured in the United States have much larger quantities of human and social capital connecting them to that country. These people had accumulated more than 50 months (4.2 years) of U.S. experience and taken 3.5 prior trips. During their current trip, they had accumulated another 63 months (5.2 years) in the United States.

Nearly half (48%) of the respondents had a migrant parent, 56% had migrant spouses, and 41% reported U.S.-born children. The average number of migrant siblings was two, and the average number of migrant children was 0.7. Three-quarters of the respondents had shifted from agriculture into an urban U.S. occupation, and 45% held a skilled U.S. job. As a result of this enhanced human and social capital, the ratio of expected wages had grown to 22.9, and the expected service benefits had increased to \$61 for welfare, \$10 for medical care, and \$1,290 for education.

We use separate dichotomous logit models to predict the return migration of men with and without legal documents. We follow migrants year by year from the moment they enter the United States until they return to Mexico or the survey date. Given individual, household, community, and macroeconomic characteristics in year  $t$ , we predict the odds of leaving the United States that same year, and the resulting coefficients are shown in table 7. In total, 1,609 undocumented and 611 documented migrants contributed 8,394 and 4,733 person-years of observation, respectively.

The estimated models clearly show that the accumulation of migration-specific human and social capital deters return migration to Mexico. As the number of months of U.S. experience grows and the duration of the

TABLE 7

LOGISTIC REGRESSION OF SELECTED VARIABLES ON THE ODDS OF RETURNING TO  
MEXICO FROM THE UNITED STATES IN YEAR *t*

SITUATION OF SUBJECT IN YEAR <i>t</i>	WITHOUT DOCUMENTS		WITH DOCUMENTS	
	B	SE	B	SE
Demographic background:				
Age .....	.002	.047	-.002	.097
Age <sup>2</sup> .....	-.0002	.0006	.001	.001
Married .....	.224*	.108	-.658*	.239
No. of minors in household .....	-.010	.027	.049	.055
General human capital:				
Labor force experience .....	-.007	.015	.042	.033
Education .....	-.048*	.013	-.087*	.029
Migration-specific human capital:				
Cumulative U.S. experience .....	-.025*	.002	-.035*	.002
Duration of trip in months .....	-.221*	.008	-.079*	.006
No. of prior U.S. trips .....	.270*	.022	.276*	.020
Holds unskilled urban job .....	-.607*	.096	-.124	.211
Holds skilled urban job .....	-.323*	.102	.289	.203
General social capital:				
Parent a U.S. migrant .....	.140	.087	.121	.170
No. of U.S. migrant siblings .....	-.039	.027	.065	.041
% of U.S. migrants in community .....	.653	1.115	-2.503	2.169
Migration-specific social capital:				
Wife a U.S. migrant .....	-.360	.198	-2.174*	.369
No. of U.S. migrant children .....	-.387*	.077	-.844*	.081
U.S.-born children .....	.050	.242	-1.326*	.367
Physical capital:				
Land .....	.931*	.168	.994*	.221
Home .....	.241*	.109	.216	.182
Business .....	-.193	.148	-.046	.226
Community infrastructure:				
Preparatory school .....	.172	.119	.875*	.223
Paved road .....	-.063	.174	1.332*	.469
Bank .....	.414*	.207	-.387	.413
Community economic context:				
% earning twice minimum wage .....	-2.782*	.761	-3.883*	1.548
% self-employed .....	1.939*	.596	-1.012	1.397
% females in manufacturing .....	-2.424*	.525	-6.072*	1.235
Community agrarian context:				
Agrarian economy .....	-.200	.120	-.127	.235
Agrarian population density .....	.001	.001	.014	.005
Proportion of land that is arable .....	.097	.202	-.624	.364
Ejido established .....	-.326	.288	-1.158*	.507

TABLE 7 (Continued)

SITUATION OF SUBJECT IN YEAR <i>t</i>	WITHOUT DOCUMENTS		WITH DOCUMENTS	
	B	SE	B	SE
<b>Macroeconomic context:</b>				
Expected wage ratio .....	-.0003	.001	.0003	.002
Peso devaluation .....	-.027	.083	-.245	.151
Mexican inflation rate .....	1.098*	.396	3.032*	.724
U.S. employment growth .....	2.936	2.797	-5.879	5.616
Growth in foreign investment .....	-.136	.100	.530*	.168
Mexican real interest rate .....	1.560*	.760	-.326	1.443
<b>U.S. policy context</b>				
Availability of visas .....	-1.990*	.848	-2.549	1.517
Probability of apprehension .....	-.090	1.126	-4.761*	1.937
Employer sanctions enacted .....	.232	.228	-1.133*	.332
Amnesty recipients in household .....	.092	.295	-.198	.281
<b>Expected value of U.S. services:</b>				
Welfare .....	-.010	.008	-.028*	.008
Medical care .....	-.014	.030	.297*	.045
Education .....	.0002	.0002	.0009*	.0002
Constant .....	3.565*	1.191	5.620	.225
Log likelihood .....	2,147.800*		743.340*	
$\chi^2$ .....	6,169.900*		2,963.300*	
No. of person-years .....	8,394		4,733	

NOTE.—Event-history data gathered among male household heads from 25 Mexican communities

\*  $P < .05$ .

trip lengthens, the likelihood of returning to Mexico falls rather steeply, irrespective of whether the trip is documented or undocumented (although the effect of trip duration is especially strong for undocumented migrants). In addition, the attainment of an urban job lowers the odds of return among those without documents, especially if the job is skilled.

The accumulation of prior U.S. trips, however, increases the odds of going home on any given trip. Building up a large number of prior trips indicates the adoption of a strategy of recurrent migration, which involves making regular trips back and forth from the United States for seasonal employment while maintaining families, households, and numerous social and economic interests in Mexico (Reichert and Massey 1979; Mines 1981; López 1986; Massey et al. 1987).

While general social capital has no significant effect on the odds of return migration, migration-specific social capital has a very profound influence but with interesting differences between legal and illegal migrants. Among those without documents, only the presence of migrant children acts to deter return migration; among those with documents, however,

having a migrant wife and U.S.-born children, as well as migrant children, strongly and sharply lowers the odds of return migration. The settlement process, therefore, is mediated by the receipt of documents; once legalization occurs, wives and children act more strongly to deter return migration to Mexico.

Marriage also has different effects on the odds of return among illegal and legal migrants. Being married increases significantly the probability that undocumented migrants will return to Mexico, but, among those with documents, marriage sharply reduces the odds of going home. Ethnographic studies suggest that Mexican men are generally reluctant to bring their wives to the United States (Reichert 1979; Massey et al. 1987; Hondagneu-Sotelo 1994; Kanaiaupuni 1995; Goldring 1995; Hagan 1995; Espinosa 1996). Accordingly, wives and small children are generally the last family members to migrate (see Massey et al. 1994).

Although Mexican men often rationalize their reluctance to bring wives as an unwillingness to expose them to the rigors of clandestine border crossing and undocumented life, they also fear a loss of patriarchal control (Hondagneu-Sotelo 1994). Once a man acquires documentation, however, this rationalization disappears, and wives are more successful in pushing their husbands to settle north of the border and to petition for documents for themselves and their children (Hondagneu-Sotelo 1994; Hagan 1994).

Since education opens doors to economic advancement in the United States, it is not surprising that the odds of return migration fall as education rises. It is also not surprising that the negative effect of schooling is stronger for documented than undocumented migrants, since the former have full liberty to capitalize on their skills in the U.S. labor market, whereas the latter encounter strong barriers to economic mobility owing to their illegal status. As a result, undocumented migrants with education have less incentive to remain in the United States and more inducements to return to Mexico.

The odds of return migration are substantially increased by the possession of land in Mexico and to a lesser extent by home ownership. The prospect of return is also enhanced by several aspects of community infrastructure. The presence of a bank in the community, for example, raises the odds of return migration for undocumented migrants, probably by providing a secure place to repatriate savings. Among documented migrants, the existence of schools and highways also raises the odds of going home.

Whereas dynamic local economies strongly promoted initial out-migration, they sharply lower the odds of return migration. Contrary to expectations derived from neoclassical economics, migrants are *less likely* to return to communities with high wage rates and high proportions of females employed in manufacturing. Lindstrom (1996) found the same pattern in his hazard analysis of return migration to a rural town in Zacatecas. He

concluded that a strong local economy gave migrants an incentive for productive investment at home and, hence, a greater need for capital, causing them to lengthen their trips in order to accumulate more cash before returning. Migrants from economically stagnant areas used U.S. migration as a source of income rather than capital and thus had less need to remain in the United States to accumulate savings, so they tended to return more quickly and to migrate more frequently.

Lindstrom's interpretation is consistent with behavior postulated under the new economics of migration, providing additional support for that theory. His interpretation is lent credence by the fact that the proportion of workers who are self-employed in a migrant's home community has a strong *positive* effect on the likelihood of return migration. People tend to return to communities where self-employment is common and entrepreneurial activity well established.

Agrarian conditions apparently have little bearing on the odds of return migration. Migrants are just as likely to return to agricultural as to industrial/service economies, and both land densities and the relative amount of cultivable land have insignificant effects on the odds of going home. The existence of an *ejido* in the migrant's home community appears to deter the likelihood of return migration somewhat but only for those with documents.

The prospects of return migration are quite strongly linked to macro-economic conditions, however, although once again not in ways envisioned by most policymakers and citizens. In particular, the binational wage rate has no significant effect on whether a Mexican migrant stays or goes, and devaluations likewise do not appear to influence settlement decisions. The relative likelihood of return migration is most strongly and positively connected to rates of inflation and real interest in Mexico. A high rate of inflation means that people possessing dollars can purchase goods and services cheaply in Mexico, providing them with a strong incentive to return; and high real interest rates mean that migrants with savings can earn a good return on their capital if they go home, providing yet another impetus for returning.

In our fieldwork, we have encountered many examples of settled U.S. migrants cashing in their U.S. assets and returning to Mexico to take advantage of unusual opportunities offered by a sudden burst of hyperinflation and elevated interest rates. Indeed, the family profiled in Massey et al. (1987, pp. 280–83) as the archetype of a settled U.S. migrant household returned to Mexico just after the book's publication, during the round of hyperinflation in 1987–88. They sold their house and business assets in Los Angeles and went home during a time when inflation and interest rates were quite high. With the large quantity of pesos they obtained for their dollars,

they established a factory in their hometown, built themselves a palatial home, and lived well off of their factory income and investments.

Finally, our analysis provides little encouragement for those seeking to stimulate return migration through restrictive immigration policies. Among undocumented migrants, the odds of return are not affected by fluctuations in the probability of border apprehension, the implementation of employer sanctions, or the expected value of social services. The only policy variable to influence the behavior of undocumented immigrants significantly is the availability of visas: as legal visas become more available, undocumented migrants grow less likely to return, probably in expectation of being able to legalize their status. Thus, the legalization of 2.3 million Mexicans under IRCA acted to deter the return migration of undocumented migrants living in the United States, even those who did not qualify directly for the amnesty.

Paradoxically, repressive immigration policies seem to have stronger effects on the behavior of documented migrants. As the probability of border apprehension increases and employer sanctions are enacted, the odds of return migration for legal immigrants fall. It may be that the imposition of restrictive policies motivates those with documents to remain in the United States and petition for the legal entry of their undocumented family members, since they fear it will be harder for their dependents to gain entry through the usual clandestine channels.

We used the logit model of table 7 to generate predicted probabilities of return migration for undocumented migrants, prepared using the same approach as before. We consider the case of a 25-year-old man with 10 years of labor market experience and estimate his probability of returning to Mexico during the first year of a U.S. trip. As before, we vary each explanatory factor from its fifth to its ninety-fifth percentile while holding other variables constant at the mean. The resulting predicted probabilities are shown in the right-hand columns of table 5, and ranges greater than 50% of the mean probability of return migration are indicated with an asterisk.

In general, undocumented migrants are relatively likely to return during the first year. The mean probability is .312, which implies that roughly a third of all undocumented migrants leave within 12 months of arriving. Out of an entering cohort of 1,000 arriving migrants, 845 would be expected to return to Mexico within five years subject to this yearly risk. As before, however, the probability of return migration shifts sharply upward and downward in response to certain variables.

Decisions about return migration appear to be dominated by five basic factors, the first of which is the quantity of migration-specific human capital, in particular the amount of time accumulated north of the border on prior trips. Undocumented migrants are unlikely to return to Mexico if

they have built up a large quantity of time in the United States, unless they have already established a clear pattern of back-and-forth movement (indicated by a large number of prior trips). Other things equal, as months of U.S. experience go from the fifth to the ninety-fifth percentile (from zero to 139 months), the probability of return migration drops from .524 all the way down to .025.

A second factor influencing the odds of return migration is the amount of migration-specific social capital a person has accumulated. In general, migrants are less likely to return to Mexico if other members of their families have also begun migrating. Among undocumented migrants, children appear to be key. As the number of migrant children goes from zero (the fifth percentile) to four (the ninety-fifth percentile) the predicted probability of return migration during the first year drops from .348 to .143.

The third factor influencing the decision about whether to stay or return is physical capital, in particular land ownership. Migrants who own land in Mexico are much more likely to go home than those who do not. The possession of farmland increases the probability of returning during the first year from .298 to .519.

The fourth consideration influencing the decision to return is the economic condition of the home community. In general, migrants tend to stay away longer from places with high wages and high proportions of females employed in manufacturing in order to accumulate more savings for investment. Increasing the proportion earning twice the minimum wage from its fifth to ninety-fifth percentile (from 5% to 37%) lowers the probability of return from .426 to .235, while shifting the share of women in manufacturing from 8% to 58% (its fifth and ninety-fifth percentile cut-points) reduces the likelihood from .359 to .144. If the community offers an unusually hospitable climate for self-employment, however, return migration becomes more likely, with the probability of return going from .257 to .424 as the percentage self-employment goes from 21% to 60% (the fifth and ninety-fifth percentiles).

Finally, the last relevant variables are macroeconomic, notably the Mexican inflation rate and to a lesser extent the real interest rate. As inflation increases from its fifth percentile value (4% per year) to its ninety-fifth percentile value (84% per year), the probability of return migration goes from .250 to .424. Likewise, shifting the real interest rate between these two percentile points (from -14% to 19%) raises the likelihood of return from .261 to .368.

#### THEORIES OF MIGRATION RECONSIDERED

In this analysis, we have estimated a series of discrete-time event-history models to determine which factors influence the likelihood of taking a first

U.S. trip, which determine the odds of taking an additional U.S. trip and which affect decisions about whether to stay in the United States or return to Mexico. We considered both documented and undocumented migrants and examined variables defined at the individual, household, community, and macroeconomic levels. We are now in a position to take stock of how the various theories stand up against a close and systematic empirical scrutiny.

### Neoclassical Economics

Although hypotheses consistent with neoclassical economics were frequently confirmed, its leading explanatory variable—the differential in expected wages between Mexico and the United States—does not appear to be a major factor explaining the course of Mexican migration over the past 25 years. Among undocumented migrants, the expected wage ratio is weakly associated with the odds of taking a first undocumented trip, is not related to the odds of taking additional trips, and is unrelated to the likelihood of going home. Among documented migrants, the expected wage ratio is not related to decisions made at any phase of the migration process—initiation, continuation, or return.

The effect of inflation and devaluation, which are often thought to increase the propensity for Mexicans to migrate by increasing the relative value of U.S. earnings, actually work to decrease the probability of both first and subsequent trips by undocumented migrants because they drive up the cost of surreptitious border crossing, a cost that must be paid in dollars. As legal migrants do not have to pay these costs, their decisions are unaffected by either inflation or devaluation.

The effects of recent U.S. policy initiatives also do not provide much support for neoclassical precepts. Attempts to raise the costs and lower the benefits of migration by increasing the probability of apprehension and by enacting employer sanctions generally have not lowered the likelihood of taking a first trip without documents, nor have they influenced the odds that undocumented migrants in the United States return home to Mexico. We do observe modest effects on the odds of repeat migration, but they are not strong in comparison with other effects in the model.

Expected values of U.S. social services do not appear to influence the migration process in any consistent way either. If Mexicans were migrating in anticipation of receiving social service benefits in the United States, then we would expect to observe a positive relationship between the size of the expected benefit and the odds of initial and repeat migration and a negative relationship between the size of the benefit and the likelihood of returning home to Mexico.

Of the three social services we consider—welfare, medical care, and



education—we observe one positive, one negative, and one insignificant effect on the odds of initial undocumented migration; we find one positive and two negative effects on the odds of repeat undocumented migration; and we observe no significant effects on the likelihood of returning home to Mexico. Among those with legal papers, benefit levels do not influence the odds of taking a first trip at all, and, in predicting additional legal trips, we find one positive and two negative effects. Finally, for return migration among legal immigrants, we find one negative and two positive effects. Given the potential for error in our estimation of expected benefits, it is difficult to find much support for the cost-benefit model of neoclassical economics in these contradictory results, which essentially balance out to overall effects near zero.

The only neoclassical hypotheses consistently sustained in our analysis are those that concern the effects of human capital. We found that undocumented migrants are negatively selected with respect to education on both first and later trips and that documented migrants are unselected with respect to education on first trips but positively selected on later trips. We also found that, irrespective of legal status, migrants are progressively less likely to return home the more education they have but that the effect of schooling is stronger for documented than for undocumented migrants.

These findings are consistent with a central thesis of neoclassical economics: that migrants move to places where their skills and abilities are most highly rewarded (see Sjaastad 1962). Owing to differences of language and culture, education acquired in Mexico is more likely to be rewarded in that country than in the United States (yielding the negative selectivity), and, given the constraints on economic mobility imposed by a lack of documents, the rewards of schooling are likely to be greater for documented than undocumented migrants (yielding differing patterns of selectivity by legal status).

We also found strong effects for indicators of migration-specific human capital consistent with neoclassical theory. As migrants accumulate experience and acquire job skills in the United States, the odds of taking another U.S. trip increase while odds of returning home decline, a pattern that holds for documented as well as undocumented migrants. Once a man migrates, he is likely to continue migrating, and the more he migrates the higher his odds of going again and the lower his ultimate odds of returning home.

According to neoclassical economics, however, human capital variables should influence migration and settlement decisions through their effect on expected wage rates. Because we have already incorporated this effect into our analysis by including the expected wage ratio as a regressor in all models, even our finding of strong relationships between human capital and migratory behavior does not provide unequivocal support for the neo-

classical model: the effects of general and migration-specific human capital may reflect social rather than economic dimensions of these variables.

### The New Economics of Migration

In general, precepts arising from the new economics of labor migration receive very strong support from our analyses. Consistent with the view that Mexicans migrate to gain access to scarce capital rather than to reap higher lifetime incomes, migration decisions at all stages of the process are more strongly linked to fluctuations in Mexican interest rates than to variations in the ratio of expected wages. Higher real interest rates make capital less accessible and force households onto the transnational labor market to acquire funds that they cannot obtain at home owing to failures in capital and credit markets. Interest rates, not wages, appear to be the key macroeconomic factor determining the course of Mexico-U.S. migration. As they go up, circulation within the migration system accelerates: more Mexicans leave for the United States to gain capital, and more migrants return to Mexico to invest what they have saved.

The effect of property ownership on migration also supports predictions derived from the new economics of migration. A primary motivation for international migration is the acquisition of a home, and studies from around the world show that more remittances and savings are channeled into housing than to any other end (Taylor et al. 1996a, 1996b). In this sense, the possession of a home constitutes a major disincentive for international movement, as one of the principal needs for capital has already been satisfied. We found that home ownership sharply lowers the odds of both initial and repeat migration among documented and undocumented migrants, and we generally found similar, though weaker, effects of business ownership, another important target for migrant investment. Home ownership also tended to increase the probability of return migration, although the effect was not strong. In predicting return migration, land ownership was a particularly powerful variable.

### Segmented Labor Market Theory

We found modest support for the view that Mexican migration is driven by labor demand in the United States. As the rate of U.S. job creation rises, so does the probability of first and subsequent undocumented trips, but the rate of employment growth is unrelated to the likelihood of documented migration or to the probability that either documented or undocumented migrants return home. Moreover, job growth has a relatively small effect on predicted probabilities of undocumented migration. While

technically correct, therefore, segmented labor market theory does not appear to account for much of the variance in undocumented movement.

Our indicator of labor demand is not very good, however, and a more theoretically appropriate measure might have produced more supportive results. Ideally our models would have included a measure of employment demand within those segments of the economy where Mexicans are disproportionately employed, not simply an indicator of overall job growth. Unfortunately, such data are not available on a year-to-year basis by state and thus could not be incorporated into our model.

### Social Capital Theory

Migration is very strongly encouraged by having social connections to U.S. migrants. During the initial stage of migration, ties to parents, siblings, and community members with U.S. experience are most important in raising the odds of taking a first U.S. trip, with or without documents. Although indicators of general social capital continue to be significant in predicting later trips, a more powerful role is played by migration-specific social capital—ties developed in the course of migration itself. The migration of wives and children and the birth of children in the United States strongly raise the odds of taking additional U.S. trips, documented or undocumented, and strongly lower the odds of returning to Mexico, especially among those with documents.

### World Systems Theory

We did not find much support for the leading hypothesis of world systems theory: that migration between Mexico and the United States stems from an ongoing process of capital penetration. Direct foreign investment had relatively small effects on predicted probabilities of initial, repeat, and return migration, and, to the extent that there was an effect, a high rate of growth in direct foreign investment produced a *lower* likelihood of initial undocumented migration and *reduced* probabilities of repeat migration among both legal and undocumented migrants. It also tended to raise the likelihood of return migration for those with documents.

We might have found more supportive results if we had been able to trace out the local effects of capital penetration. Indeed, when we examined the effect of economic development at the community level, we found patterns consistent with world systems theory, which sees development as a radical transformation that displaces people from traditional ways of life and creates a mobilized population prone to migrate. Thus, we found that economic development—as indicated by high wage rates and high proportions of women working in manufacturing—was central to

initiating the process of undocumented out-migration. As these factors increased, the odds of first undocumented migration rose sharply. As already noted, however, the same findings are consistent with the new economics of migration, so ultimately our analysis provides equivocal support for world systems theory.

### What Is Driving Mexico-U.S. Migration?

After systematically considering the effects of 41 variables and exploring the validity of five theories of international migration, we conclude that three fundamental forces are at work in promoting Mexican migration to the United States. The first is *social capital formation*, which occurs because people who are related to U.S. migrants are themselves more likely to migrate. As a result, each act of migration creates additional social capital capable of instigating and sustaining more migration. After more than 50 years of continuous development, this process of social capital formation is well advanced and largely self-sustaining. National surveys indicate that about half of adult Mexicans are related to someone living in the United States (Camp 1993), so that social capital—generally the most powerful factor predicting the odds of initial, repeat, and return migration—is very widely diffused throughout the Mexican population.

The second fundamental force is *human capital formation*. For undocumented migrants, the most important element of human capital is migration experience itself: crossing the border, living in the United States, working in the U.S. labor market, and negotiating U.S. housing markets. The more U.S. experience a migrant accumulates, the higher his likelihood of both documented and undocumented migration. Once a man migrates, the odds are quite high that he will migrate again, and, with each additional month of U.S. experience, the odds of taking another trip rise while the odds of returning to Mexico fall.

This process of self-reinforcing human capital formation intersects with and reinforces the process of social capital formation since added experience makes a person more valuable as a resource for gaining entry to the United States and finding a job. The more experience a person has, the more likely his friends and relatives are to begin migrating and to continue migrating themselves. National surveys reveal that one-third of all Mexicans have been to the United States at some point in their lives (Camp 1993). Once again, after 50 years of constant movement back and forth, the human capital necessary to support mass migration is also widely diffused throughout Mexico.

The final process underlying Mexico-U.S. migration is *market consolidation*. Over the past two decades, the economies of Mexico and the United States have become increasingly connected to each other and to

the global capitalist economy. Within rural Mexico, competitive markets have penetrated ever further into subsistence sectors and brought about the displacement of manual workers, the concentration of land, and the mechanization of production. Within urban Mexico, the ending of import substitution industrialization has brought about wrenching economic transformations that have displaced workers from parastate enterprises and public bureaucracies.

Growing economic insecurity coupled with a strong desire to participate in this new political economy have led Mexican households to search for ways to self insure against threats to family income and to gain access to scarce capital. Given ready access to human and social capital connecting them to the United States, household heads and other family members migrate internationally as part of a conscious strategy of risk diversification and capital accumulation. Thus, economic development goes hand in hand with international migration.

#### U.S. POLICIES RECONSIDERED

This theoretical understanding permits a clearer evaluation of policy options for the United States. Although the mechanisms hypothesized under neoclassical economics do not seem to play a very large role in promoting Mexico-U.S. migration, U.S. policy has been based almost exclusively on a cost-benefit approach. In recent years, for example, the U.S. Congress has reduced the number of visas available to Mexicans, devoted more resources to border enforcement, and enacted employer sanctions; it is now debating whether or not to bar legal immigrants from receiving certain social services, and California's Proposition 187 seeks to deny undocumented migrants public services in that state.

Our analysis suggests that these actions will not be very effective in reducing undocumented migration from Mexico to the United States. First, reducing the supply of legal visas simply channels a larger share of the migrant stream into undocumented status without really affecting the volume of the flow. Second, increasing apprehension probabilities and implementing employer sanctions raise the odds of initial undocumented migration and have no effect on the odds that undocumented migrants already in the United States will return to Mexico. Third, although raising apprehension probabilities and implementing employer sanctions have modest effects in lowering the odds of taking additional illegal trips, these are counterbalanced by positive effects on first trips to yield a net effect close to zero, consistent with Espenshade's (1994) analysis of aggregate apprehension statistics. Finally, whether positive or negative, the effects of border apprehensions and employer sanctions on predicted probabilities of initial, repeat, and return migration are rather small and are conse-

quently overwhelmed by the more powerful forces of social capital formation, human capital formation, and market consolidation.

Cutting off undocumented migrants from the receipt of social services will probably have contradictory and largely offsetting effects on the odds of initial and repeat migration and no effects whatsoever on the likelihood of return migration. The most likely effect of Proposition 187's implementation in California will not be a reduction in the size of the undocumented population in that state, but the creation of an undocumented population that is markedly poorer, less healthy, less educated, and more tenuously connected to the rest of society.

At the same time that the United States has implemented repressive policies to discourage immigration from Mexico, it has pursued other policies that have had unintended, but ultimately more powerful, effects that encourage the transnational flow. The U.S. policy of allocating visas on the basis of family ties, for example, reinforces the process of social capital accumulation that is one of the principal engines of Mexican immigration. Of the 656,000 legal immigrants admitted to the United States in 1990 (excluding those receiving amnesty), 77% were admitted because they were related to someone already admitted to the country (U.S. INS 1994).

The process of social capital formation was given additional impetus by the legalization program implemented under the Immigration Reform and Control Act. The legalization of 2.3 million former undocumented Mexican migrants promotes chain migration in two ways: first, it gives immediate family members in Mexico a new claim on legal entry (6% of all immigrants in 1993 were dependents of former undocumented migrants who had undergone legalization); second, legalization allows amnesty recipients to sponsor the undocumented migration of their friends and relatives. According to our analysis, being from a household where someone received amnesty very greatly increases the odds of first and subsequent migration by undocumented family members.

U.S. economic policies toward Mexico also have worked to exacerbate one of the most important factors promoting migration to the United States: high interest rates. As a condition of receiving additional funds and loan guarantees from the United States to deal with its successive economic crises over the past decade, Mexico has repeatedly been forced to raise interest rates to exceedingly high levels. Our analysis clearly shows that high interest rates are a powerful force promoting both documented and undocumented migration.

Finally, economic changes occurring in Mexico under the auspices of the North American Free Trade Agreement (NAFTA) can be expected to promote undocumented migration as well. The provisions of NAFTA reinforce the ongoing process of market consolidation in Mexico and help to bring about the social and economic transformations that generate mi-

grants. The integration of the North American market will also create new links of transportation, telecommunication, and interpersonal acquaintance, connections that are necessary for the efficient movement of goods, information, and capital but which also encourage and promote the movement of people—students, business executives, tourists, and, ultimately, undocumented workers.

In the final analysis, then, U.S. policies with respect to Mexican immigration are counterproductive. The repressive measures currently favored by voters and public officials, when combined with policy actions that unintentionally reinforce and nurture the forces driving Mexico-U.S. migration yield the worst of all possible worlds. The flow of migrants is not stopped, and all parties pay a high price for these continuing, misdirected efforts. The migrants pay the highest price, of course, because they bear the brunt of the repressive measures. The United States also pays a high price, however, through the creation of a new population of marginalized residents disconnected from the rest of society—unhealthy, poorly educated, with little stake in the future of the country, its government, or its way of life.

## APPENDIX

TABLE A1

SELECTIVITY-CORRECTED REGRESSIONS USED TO PREDICT EXPECTED HOURLY WAGES IN MEXICO AND THE UNITED STATES

VARIABLE	MIGRANT WITHIN MEXICO		LOG OF MEXICAN HOURLY WAGE		MIGRANT WITHIN U.S.		LOG OF U.S. HOURLY WAGE	
	B	SE	B	SE	B	SE	B	SE
Demographic background:								
Married .....	-.135*	.014	.165	.213	-.063*	.018	-.105*	.072
No. of milbors in household .....	-.007*	.002	.056	.057	.029*	.004	-.034*	.013
General human capital:								
Labor force experience .....	-.002*	.001	-.030*	.012	-.050*	.001	.011*	.003
Education .....	.040*	.001	.089*	.023	-.019*	.002	.021*	.006
Migration-specific human capital:								
Cumulative U.S. experience .....	-.005*	.001	.004	.006	.015*	.001	.0006*	.0003
No. of prior U.S. trips .....	-.050*	.004	.073	.122	.120*	.003	-.0006	.005
Documented .....	-.395*	.039	.211	.624	.529*	.028	.011	.063
General social capital:								
Wife a U.S. migrant .....	-.262*	.025	-1.433*	.289	.971*	.022	.152*	.071
No. of U.S. migrant children .....	.048*	.008	-.258	.257	-.012	.009	-.019	.017
U.S.-born children .....	-.385*	.047	.257	.910	.255*	.033	.140	.752
Period:								
1970-74 .....			-1.049*	.395			.104	.214
1975-79 .....			-.286	.413			-.017	.202
1980-84 .....			-.346	.401			-.306	.200
1985-89 .....			-1.471*	.413			-.112	.194
Selectivity:								
$\lambda$ .....			.509*	.165			-.032	.034
Constant .....	-.735*	.014	.141	.406	-.911*	.018	1.697*	.209
Log likelihood .....			40,686.000*				20,985.000*	
$\chi^2$ .....			4,560.100*				34,978.000*	
Adjusted $R^2$ .....				.237				.126
Person-years .....	89,737		16,596		89,737			13,775

NOTE.—Hourly wages in both Mexico and the United States are expressed in 1990 U.S. dollars



TABLE A2

BIVARIATE PROBIT ESTIMATES OF EQUATIONS USED TO PREDICT THE PROBABILITY OF USING SOCIAL SERVICES IN THE UNITED STATES

VARIABLE	USED AFDC OR FOOD STAMPS		USED UNREIMBURSED MEDICAL SERVICES		USED U.S. PUBLIC SCHOOLS	
	B	SE	B	SE	B	SE
Demographic background:						
Age .....	-.108*	.006	-.103*	.006	-.045*	.004
Age <sup>2</sup> .....	.001*	.0001	.001*	.0001	.0001*	.00001
No. of minors in household ..	.039*	.006	.062*	.006	.056*	.003
General human capital:						
Education .....	.015*	.002	.027*	.002	-.007*	.002
Migration-specific capital:						
Cumulative U.S. experience ..	.001*	.0001	.002*	.0002	.004*	.001
Documented .....	.343*	.022	-.121*	.035	.067*	.021
General social capital:						
Wife a U.S. migrant .....	.480*	.023	.563*	.033	1.139*	.019
No. of U.S. migrant children ..	.047*	.007	.072*	.011	.208*	.005
U.S.-born children .....	.365*	.027	.317*	.039	.849*	.024
Period:						
1970-74 .....					-.120*	.018
1975-79 .....					-.272*	.019
1980-84 .....					-.453*	.021
1985-89 .....					-.621*	.024
Constant .....	-.090	.085	-.428*	.092	-.118	.062
Log likelihood .....	15,187.000*		10,854.000		31,996.000	
$\rho$ .....	-.987		.292		.452	
Person-years .....	89,737		89,737		89,736	

NOTE.—Migration selection equations are not shown

\*  $P < .05$ .

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# World-Economic Trends in the Distribution of Income, 1965–1992<sup>1</sup>

Roberto Patricio Korzeniewicz and Timothy Patrick Moran

*University of Maryland at College Park*

This article assesses trends in world income inequality between 1965 and 1992. The study first decomposes Theil's *T* to show that between-country inequality is the most significant component of overall world income inequality, which facilitates further research, as between-country longitudinal data are more easily available than national income distribution data. Next, the study uses a larger set of nations and finds a significant increase in world income inequality as measured by both Gini and Theil coefficients, particularly during the 1980s. The findings are robust even after controlling for differential rates of population growth or using alternative sources of data. By identifying these trends, the article is able to explain past discrepancies and recent shifts in the relevant empirical and theoretical literature.

Empirical research on trends in the world distribution of income has attracted intermittent attention throughout the past two decades, particularly from the field of economics. But there are practically no such studies tracing the evolution of world income inequality during the 1980s, a decade of stagnant economic growth in many low- and middle-income nations. In particular, contributions from sociologists are virtually nonexistent. Despite the discipline's long-standing concern with identifying patterns of stratification and social inequality, studies of these trends on a global scale are rare (noteworthy exceptions are Robinson [1976], Breedlove and Nolan [1988], and Peacock, Hoover, and Killian [1988]), and there has been practically no published research on this topic during the 1990s. This shortage of empirical research is surprising because the topic at hand addresses both issues of intense policy debates at national and international levels (in areas such as economic strategy) and key theo-

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retical debates in the social sciences (in areas such as social stratification and development).

This study seeks to identify trends in world income inequality between 1965 and 1992. The first section of the article reviews the pertinent literature, highlighting a recent shift from analytical dispute to theoretical convergence and linking this shift to the current paucity of empirical research on the topic at hand. The second section is designed to establish whether data on the distribution of income between countries can provide an accurate assessment of trends in world income inequality; for this purpose the study decomposes world income inequality to assess the relative contribution of between- and within-country components. From these findings, the third section of the article provides a more detailed assessment of trends in world income inequality between 1965 and 1990 and evaluates whether these trends are explained primarily by differential rates in economic or population growth. Finally, we use our empirical findings to provide an explanation of the trends noted within the pertinent theoretical and empirical literature.

#### THEORETICAL AND EMPIRICAL BACKGROUND

While contending theoretical approaches have advanced quite different speculations about trends in the global distribution of income, there are few systematic comparisons of these interpretations. To be sure, efforts to synthesize the relevant literature are complicated by the sheer number of pertinent debates and interpretations (as the topic is addressed either directly or indirectly by virtually all theories and empirical researchers dealing with world capitalist development). What is more important, empirical researchers have pursued separate paths of inquiry, following the boundaries enforced within different academic disciplines with few cross-references between the various studies. The resulting literature is thus characterized by considerable heterogeneity in methods of inquiry, analytical assumptions, and theoretical concerns.

Despite these difficulties, there are rather clear trends within the literature. Overall, there is a general theoretical and empirical consensus that, until the 1950s, the development of capitalism was characterized by growing disparities in the distribution of income between poor and rich nations (although with some disagreements as to whether a significant gap existed before the 19th century; see Kuznets 1965; Bairoch 1962, 1981, 1993; Maddison 1983; Zimmerman 1962). This consensus broke down, however, when considering trends between the 1950s and 1970s, with contentious debates as to whether the world distribution of income subsequently moved toward convergence or growing inequality. Since the 1980s, finally,



the literature has undergone a gradual but perceptible shift from analytical contention toward theoretical consensus.

### The Argument for Convergence

A theoretical case for convergence has been made by authors across the social sciences. Some emphasize growing social similarities among all nations (Inkeles and Rossi 1956; Hoselitz 1960; Hoselitz and Moore 1963; Levy 1967; Inkeles 1969). Others, particularly within economics, argue that successful development itself generates institutional obstacles to continuing rapid growth among the wealthiest of nations, while "backward[ness] in level of productivity carries a *potential* for rapid advance" among poorer nations (Abramovitz 1986, p. 386, emphasis in the original; see also Veblen 1915; Rostow 1960; Kindleberger 1961; Gerschenkron 1962). For many proponents of this perspective, world markets serve to circulate new technologies and innovations, so "international product and factor markets unobstructed by either cartelization or governmental intervention will bring irrepressible and rapid growth to any poor country" (Olson 1982, p. 176; see also Rostow 1960; Baumol 1986).

Exploring the institutional forces that lead to convergence, Olson (1982, p. 65; see also 1995) argues that the rise of collective organizations and coalitions in wealthy nations eventually inhibits innovation, leading to "reduced efficiency and aggregate income in the societies in which they operate." Likewise, for Abramovitz (1986, p. 401), the rate of economic growth is likely to diminish in wealthier nations because "the capital stock of a country consists of an intricate web of interlocking elements. They are built to fit together, and it is difficult to replace one part of the complex with more modern and efficient elements without a costly rebuilding of other components." Given this uneven distribution of institutional constraints and technological opportunities between more and less advanced nations, "poorer and technologically less advanced areas can grow faster, as they catch up, than richer and technologically more advanced areas" (Olson 1982, p. 114).

Although often lacking fully developed theoretical frameworks, the results of several empirical studies are compatible with such a convergence hypothesis (see table 1). Ram (1989, pp. 81–83) argues that, although income inequality increased for 1960–80 as a whole, "increased inequality seems to have slowed down and may even have stopped" toward the end of the period, so that the "time-profile of intercountry inequality is likely to be a U-curve and not one of monotonic increase." As indicated in table 1, other studies claim to have found little change in world income distribution after 1950 (Andic and Peacock 1961; Beckerman and Bacon 1970; Berry, Bourguignon, and Morrisson 1983a; Summers, Kravis, and Heston

TABLE 1

## SUMMARY OF EMPIRICAL RESEARCH

Authors	Year of Study	Time Frame	Sample Size and Composition	Measurement of Income	Measurement of Inequality
<b>Convergence/no change:</b>					
Andic and Peacock	1961	1949-57	62, no socialist countries	"National income"; author mixes NDP, GDP, and GNP for missing data	Gini, Lorenz curves, growth rates
Beckerman and Bacon	1970	1954-63	74, China excluded	Constructed own "consumption" measure	Gini
Ram	1989	1960-80	115, no socialist countries	GDPPC	Thell, regression analysis
Summers et al.	1981	1950-75	106, no socialist countries*	GDPPC, GDY, with PPPs	Gini
Berry et al.	1983 <sup>a</sup>	1950-77	124, socialist countries, China, and India included	GNPPC with PPPs	Gini, Thell
<b>Divergence:</b>					
Breedlove and Nolan	1988	1960-80	69, socialist countries, China, and India included	GDPPC with PPPs	Gini
Jackman	1982	1960-78	102, no socialist countries†	GNPPC	Regression analysis
Kirman and Tomasin	1969	1951-66	44, no socialist countries	GNPPC	Spearson's Rho, other rank measures, Lorenz curves
Morawetz	1977	1950-75	132, no socialist countries	GNPPC	Growth rates, gap analysis
Passé-Smith	1993	1962-90	115, China excluded	GNPPC	Growth rates, gap analysis
Peacock et al.	1988	1950-80	53, no socialist countries	GDPPC with PPPs	Thell
Rock	1993	1975-89	Samples vary	GDPPC	Growth rates
Thell	1967	1949-76	54, no socialist countries	"Income" with PPPs	Thell

\* We were unable to determine whether China or India were included

† We were unable to determine whether India was included.

1981). A longitudinal study (Berry et al. 1983b) combines data on income distribution within and between countries and reports that, as measured by both Gini and Theil indexes, inequality increased between 1960 and 1970 but began to level off thereafter. Several authors argue that although disparities between rich and poor nations may be more pronounced over the postwar period, the share of middle-income countries has consistently increased (Atkinson 1983; Berry et al. 1983a; Summers et al. 1981). Firebaugh (1983, p. 268) reports finding evidence for convergence but warns that this evidence might reflect only short-term trends, as the world-economy might experience "cycles of scale economy and scale entropy, cycles occasioned by major technological change."

### The Argument for Divergence

There is an extensive theoretical literature that challenges the hypothesis of convergence. A long-standing line of Marxist interpretation portrays inequality as a structural component of capitalist accumulation in a world-economy (Lenin 1939; Luxemburg 1951; Baran 1957; Magdoff 1969). On a different track, policymakers working for the Economic Commission for Latin America (ECLA) of the United Nations after World War II contended that a deterioration of terms of trade was leading to growing inequality between wealthy and poor nations (Prebisch 1950 and 1964; ECLA 1969; Furtado 1971). Parallel arguments were developed by Singer (1950) and Myrdal (1957). Later, and often evolving as a critique of the previous interpretations, dependency studies argued that the very existence of a capitalist international economy, as embodied in global trade or an international division of labor, entailed a continual transfer of surplus from poor (or satellite) to wealthy (or core) areas (e.g., Cardoso 1974, 1977; Cardoso and Faletto 1969; Dos Santos 1970; Frank 1966, 1967, and 1978).<sup>2</sup> A similar emphasis on the persistence of world inequality also prevails within the original world-systems literature (e.g., Portes and Walton 1981; Wallerstein 1974, 1979, 1980).

Within these different approaches, the explanation of the mechanisms generating the persistence of inequality has varied, but all would agree that world income inequality becomes more pronounced over time. For example, some authors emphasize the importance of profit remittances and labor surpluses in peripheral areas (Amin 1976), while others highlight the role of unequal exchange (Emmanuel 1972) or the uneven distribution of productivity gains between enterprises and labor in core and peripheral

<sup>2</sup> For useful reviews of this literature see Fitzgerald (1981), Gereffi (1994), the essays in Orral et al. (1975), Palma (1978), Portes (1976), Roxborough (1979), Valenzuela and Valenzuela (1978).

countries (ECLA 1969; Prebisch 1959, 1964). Myrdal (1957, p. 6) indicates that inequality results from high rates of population growth and little innovation in underdeveloped countries, combined with a firm "pattern of continuing economic development" in wealthy nations. Singer and Ansari (1982, p. 37) argue that "the real source of the maldistribution of the gains from trade and investment lies in the nature of modern technology and the process of its development" in wealthy nations (for similar arguments, see Griffin [1978] and Singer and Roy [1993]). Frank (e.g., 1966, 1967) indicates that networks linking metropolises to satellites serve to transfer economic surplus from poor to wealthy areas; he expects world inequality to become less pronounced when these linkages are weakened by economic recession and/or disruptions in international trade. Wallerstein's (1974) initial contributions emphasize the competitive and political advantages derived by core enterprises from both the uneven spatial distribution of accumulated capital (including human capital and technologies) and the uneven development of states and different modes of labor control.

Providing empirical support for such interpretations, several quantitative studies indicate that the gap between poor and rich nations is extensive and growing. Kirman and Tomasini (1969) indicate that, regardless of the specific techniques used to evaluate this gap, the distance between the two groups grew during the 1950s and 1960s. While acknowledging that the *relative* gap narrowed slightly for a few regions experiencing high rates of economic growth (China, East Asia, the Middle East), Morawetz (1977) analyzes gross national product per capita (GNPPC) data to indicate that the *absolute* gap continued to widen during the 1950–75 period, and that even the relative gap continued to grow in many regions (Latin America, Africa, South Asia).<sup>3</sup> Passé-Smith (1993a) performs a similar exercise, showing a widening gap over the 1975–90 period in absolute as well as relative terms (except for a few countries, such as Italy and Japan, and for the East Asia/Pacific region). These conclusions are compatible with Jackman (1982), who argues that the overall gap between poor and wealthy nations increased in the 1970s, although with some convergence between the wealthiest nations and the most developed peripheral countries.

Breedlove and Nolan (1988) measure international inequality by calculating Gini coefficients of real gross domestic product per capita (GDPPC) for five-year intervals between 1960 and 1980; they conclude that there

<sup>3</sup> Morawetz points out that the growth of the absolute gap "is the result of the simple algebra of gaps. In brief, a poor country growing faster than a rich one will not even begin to reduce the absolute gap between them until the ratio of their per capita incomes is equal to the inverse ratio of their growth rates" (Morawetz 1977, p. 27; see also Rock 1993).

was a growing gap between the most and least developed countries, with different trends within each of these groups (convergence among the most developed countries, divergence among the least developed). Although the authors express reservations regarding the ability of world-systems theory to explain their findings, Peacock et al.'s (1988) estimates of Theil's  $T$  for between-group and within-group inequalities (calculated on the basis of GDPPC data) suggest a slight increase in world income inequality between 1960 and 1980 (but a decline in the 1950s). Chase-Dunn (1989, p. 269) reviews recent studies and provides rough data on changes in the distribution of GNPPC and other resources (such as energy consumption) to argue that "despite the undeniable industrialization of many peripheral and semiperipheral states, there is no evidence of a reduction in the magnitude of core/periphery inequalities." Arrighi (1991, p. 40) compares levels of GNPPC for different regions and countries and concludes that "after more than thirty years of developmental efforts of all kinds, the gaps that separate the incomes of the East and of the South from those of the West/North are today wider than ever."

The differences in the empirical findings within the convergence/divergence debate are striking. To what extent can these differences be explained as an outcome of the data or methodological procedures selected by these studies? A careful analysis of the studies in table 1 finds no clear pattern in either data sources or methodological procedures that might explain these differences, as the structure of these studies (i.e., data sources, measures of inequality, sample size, or time period) does not go far in explaining the direction of their findings. In fact, studies with very different conclusions use the exact same data set to provide their evidence (e.g., Breedlove and Nolan [1988] and Peacock et al. [1984] use the data of Summers and Heston [1984, 1988] to find evidence for divergence, but the same source is used by Ram [1989] to claim growing convergence). Further inquiry into these differences, however, has been hampered by subsequent developments in the literature during the 1980s and 1990s.

### Toward Theoretical Convergence?

Regardless of methodological disagreements, theoretical shifts since the 1980s have altered the terms of the convergence/divergence debate. Moving away from the convergence hypothesis, influential authors in the field of economics are acknowledging that growth in poor nations can continue to be hindered by persistent institutional constraints, while wealthy countries can experience unexpected pressures (e.g., domestic competition among regions or political transformations) that serve to overcome institutional constraints and to promote renewed growth (e.g., Abramovitz 1986; Adelman and Morris 1980; Barro and Sala-i-Martin 1992; Baumol 1986;

Baumol and Wolff 1988; Clark 1987; De Long 1988; Easterlin 1981; Zind 1991; and some of the essays in Baumol, Nelson, and Wolff [1994]).<sup>4</sup>

Similar caveats regarding the convergence hypothesis and emphasizing the importance of institutions in shaping both the rate and direction of economic growth have always been available elsewhere in the literature (e.g., Adelman and Morris 1967, 1980; Ames and Rosenberg 1963; Hirschman 1958; Horowitz 1966; Kuznets 1971; North 1981, 1989).<sup>5</sup> The new economic literature, however, does not treat technology as an exogenous variable, but emphasizes the endogenous processes (e.g., investments in human capital or patterns of government spending) involved in economic growth (Barro 1990, 1991; Benhabib and Jovanovic 1991; Helpman 1992; King and Rebelo 1990; Lucas 1988; Romer 1986, 1990a, 1990b; You 1994). This line of interpretation moves away from short-term predictions regarding trends in the world distribution of income, to evaluate instead endogenous growth models that attribute differences in growth rates to variables such as the amount of resources allocated to research and development.<sup>6</sup>

At the very least, then, the economic literature on technology and economic growth has moved toward much greater skepticism regarding convergence for the world-economy as a whole. As indicated more broadly for the field of economics by Sabel (1994, p. 140), "Many of the most mainstream economists now doubt that markets work to equalize growth rates in all economies. More to the point, they suspect that strength can breed strength and the strong can continue to grow faster than the weak." Within the field of economics, this theoretical shift might be partly responsible for the relative decline in the number of quantitative studies focusing on trends in world income inequality.

<sup>4</sup> For Abramovitz (1986, p. 389), "social capability . . . depends on more than the content of education and the organization of firms. Other aspects of economic systems count as well—their openness to competition, to the establishment and operation of new firms, and to the sale and purchase of new goods and services. Viewed from the other side, it is a question of the obstacles to change raised by vested interests, established positions, and customary relations among firms and between employers and employees."

<sup>5</sup> Even Baumol (1986, p. 1081) notes that much of the literature on the slowdown in the rates of growth among wealthy nations was produced in the midst of the economic changes experienced during the 1970s and early 1980s in the United States, a country that experienced a dramatic fall in the growth rate of labor productivity from a post-war peak: "But it is the peak which looks like the aberration, and the decline from it may well prove to be a return to historical growth rates in labor productivity."

<sup>6</sup> Endogenous growth models have been challenged by recent empirical studies suggesting that long-run rates of economic growth are "invariant to conventional government policies" (Jones 1995, p. 497), but no alternative overarching explanation of differences in rates of economic growth has yet emerged from the economics literature.

But a parallel theoretical shift has characterized what can be referred to as the "new" critical literature on the political economy of development (e.g., Evans 1995; Haggard 1990; Shafer 1994; Wade 1992). To a considerable extent, much of this literature emphasizes that different patterns of state policies have led to a growing heterogeneity in the relative economic performance of peripheral and semiperipheral countries after the 1970s. For example, many of these studies contrast the Latin American and East Asian patterns of growth, arguing that prevailing patterns of industrialization and state regulation led to economic stagnation in the former region, but to rapid economic growth in the latter. Drawing on such experiences, Shafer (1994, p. 1) introduces his recent study of state developmental strategies by noting that "for decades we focused on North-South issues: Today we must explain differentiation within the third world."

Likewise, while retaining an emphasis on the persistence of world inequality, the latest contributions to the world-systems perspective portray world-economic status as contingent upon an uneven ability of states and enterprises in rich and poor nations to engage in innovation (Arrighi and Drangel 1986; Arrighi 1990, 1991, 1994; Cumings 1984; Gereffi and Korzeniewicz 1994; Gereffi and Wyman 1990; Wallerstein 1983). Of course, this orientation is linked to Schumpeter (1934, 1942), for whom innovative processes were at the root of the creative destruction that characterizes capitalism as a system. From this perspective, the very implementation of innovative practices initiates their diffusion, their eventual routinization, and the creation of new technological, organizational, and institutional rigidities, all manifested in the relative intensity of competitive pressures.<sup>7</sup>

Combined, the shifts reviewed in these pages have produced a noteworthy theoretical convergence. As studies have moved to focus on the impact of technological change and institutional innovation on global patterns of competition and economic growth, considerable overlap has emerged both between disciplines (e.g., sociology and economics) and between theoretical perspectives (e.g., world-systems theory and the new institutional economics). In this sense, a theoretical convergence in the 1990s has displaced to a considerable degree the contentious debates of the 1970s and 1980s on the future of world income inequalities.

<sup>7</sup> Hence, there is nothing inherent about the character of certain production processes or commodities (e.g., iron vs. apples) that determines their yield of relative wealth. Instead, production processes and commodities "have had 'production cycles,' starting off as core products and eventually becoming peripheral products" (Wallerstein 1983, p. 36). Insofar as innovation constitutes the principal mechanism through which world income inequality has been reproduced, no specific or single strategy can be expected to provide a fully reliable lodestar of development.

With the advance of this theoretical convergence, however, empirical research on world income inequality has lagged behind. Replacing such inquiries, the social sciences in general have come to be guided in the 1990s by a series of general assumptions drawn from apparent patterns of growth in the contemporary world-economy. For example, the economic success of East Asia is often presumed to entail a substantial redistribution of world income from wealthy to poor nations. To be sure, the paucity of research on these trends suggests that such assumptions have gained ground through a selective assimilation of partial observations rather than through systematic empirical verification. These untested assumptions are legitimated primarily by their commonsense appeal, yet they have come to prevail in establishing the boundaries of reasonable empirical inquiry in fields such as economic development.

The main concern of this study is to address the current lack of empirical research by evaluating recent trends in world income inequality. Such an effort is designed to begin addressing the lag between theoretical development and empirical inquiry discussed above. As always, such empirical inquiries often facilitate the task of evaluating which of the existing theoretical approaches (e.g., among those reviewed in this section) provides the best fit to observed trends. As indicated in the concluding section of this article, however, an evaluation of trends in world income inequality can also prove to be useful in explaining the very patterns of *theoretical* development addressed in this section.

#### DISAGGREGATING THE WORLD DISTRIBUTION OF INCOME

Here, we assess the relative contribution of between- and within-country inequalities to overall trends in the world distribution of income between 1965 and 1992 to determine whether between-country income distribution data on their own provide a sufficiently close approximation to trends in the overall distribution of income. After coming to terms with this methodological issue, we examine, in greater detail, the evolution of trends in the global distribution of income over the 1965–90 period.

The most comprehensive procedure for evaluating changes in the global distribution of income would estimate the relative shares accruing to individuals or households by combining national income data within countries with an indicator of the relative distribution of income between nations, then to estimate longitudinal changes by calculating an appropriate coefficient that measures inequality for the income distribution (Berry et al. 1983a; Grosh and Nafziger 1986). Several problems, however, affect national data on income distribution, as most studies restrict their measurement of income to money income or wages and are often affected by incomplete measurements and infrequent observations, statistically inade-



quate samples, or a failure to properly control for change in the composition of family units (for further discussion of the problem, see Ahluwalia [1976, 1993], Berry [1985, 1987], Berry et al. [1983b], and Ram [1989]). The variety of procedures used to collect the data, particularly in less developed countries (LDCs), challenges the reliability of strict comparisons (Altimir 1987). Most important, there is generally an insufficient number of observations to carry out meaningful comparisons over time. As a result, most studies must either make the assumption that "national income distribution is fairly constant over time, at least for the short to medium terms" (Grosh and Nafziger 1986, p. 349; see also Berry et al. 1983a), or they must rely on the restricted data available in the 1960s (e.g., Nolan [1983] and even Robinson [1976]).

The recent appearance of new data on income distribution within low-income and middle-income nations provides an opportunity to establish whether data on between-country inequality can alone provide a sensible representation of overall trends in world income inequality. If so, such a finding would have important methodological consequences, as it would indicate that the scarcity of data on within-country income inequality does not represent an insurmountable obstacle to the study of trends in the global distribution of income. Our study hence evaluates changes in the global distribution of income by combining national income data within countries with an indicator of the relative distribution of income between nations. This information is then used to evaluate longitudinal changes by calculating and decomposing the appropriate coefficient measures of inequality for the income distribution.

### Data and Methods

The relevant longitudinal data include three variables: national income, population, and within-country income distribution. Until recently—or at least following the 1950s—data were easily available on national income and population, but not on income distribution within nations. However, new data on income distribution makes it possible to track changes between 1965 and 1992 in 46 countries (accounting for 68.0% of the world's population in 1992).<sup>8</sup>

<sup>8</sup> The study relies primarily on the income distribution data available from the World Bank (1994). This source was used for all our information ca. 1992, with the sole exception of Mauritius (for which we used the estimates in Jazairy, Alamgir, and Panuccio [1992]). For ca. 1965, most of the income distribution data was derived from Paukert (1973; 31 countries), with some use of Jazairy et al. (1992; 7 countries) and World Bank (1994, 6 countries). Our last choice for ca. 1965 was to use the information in World Bank (1993). In this case, the income shares of the third and fourth quintiles were collapsed together, so we were forced to divide the income share equally among

To evaluate changes in the distribution of income between countries, this study uses the GNPPC and population data available through the World Bank (1988 and various years). Many studies acknowledge GNPPC as the most acceptable relative indicator of income (e.g., Berry et al. 1983a; Kravis, Heston, and Summers 1978; Morawetz 1977; Whalley 1979). As a measure of relative command over world income, GNPPC is preferable to GDPPC (or similar indicators of output), because the latter indicator fails to capture changes in terms of trade or net receipts from abroad (both of which are relevant components of this study's variable).<sup>9</sup> Some object to the use of national incomes converted to U.S. dollars at official exchange rates (arguing that these are distorted indicators of income levels; see Berry et al. 1983a, p. 331) and propose instead that GNPPC be converted into an indicator of relative welfare (using, e.g., the purchasing power parities, or PPPs, reported by Summers, Kravis, and Heston [1980] and Summers and Heston [1984, 1988]).<sup>10</sup> While these efforts may provide a better approximation to *welfare conditions* by taking into account relative price differences in goods and services, GNPPC at market prices itself provides a better relational indicator of *command over income*, or the relative command that inhabitants of different countries have "over the human and natural resources" of each other (Arrighi 1991, p. 106).<sup>11</sup>

To combine the within- and between-country data, the study disaggregates each individual country into five population quintiles. Each of these quintiles is given as its income the proportion of the country's total gross national product (GNP) that is attributed to that particular quintile by the national income distribution. Hence, as opposed to the world income distribution patterns described by the between-country data (providing

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both quintiles (producing a rather conservative estimate of the degree of inequality). This last source was used for only two cases ca. 1965.

<sup>9</sup> Others have used measures of industrial production and/or exports, but Rau and Roncek (1987) persuasively argue that these might not be good indicators of command over wealth.

<sup>10</sup> The assumption is that wage and productivity differentials in high- and low-income countries result in distinct price structures, wherein the real GDP levels of low-income countries tend to be higher than their nominal levels (and vice versa for high-income nations; see Kravis et al. 1978). By weighting price structures independent of fluctuating exchange rates, so as to compensate for price distortions and informal economic activities, a better approximation can be reached of relative wealth levels among nations. Overall, the use of such measures as GDP adjusted by PPPs tend to lower the gap between rich and poor nations (see, e.g., Levy and Chowdhury 1994).

<sup>11</sup> To use an analogy, the analysis of changes over time in income distribution in the United States has a legitimacy of its own, and does not depend on whether observers find different degrees of monetarization of productive activities among rich and poor sectors of the population.

information for the relative income accruing to the population of 46 countries), this procedure allows us to further disaggregate the world population into the five population quintiles of each country in the sample (thereby producing observations for 230 country quintiles). The relative position of each of these country quintiles in 1965 and 1992 is listed in appendix table A1.

### Summary Measures of Inequality

This study relies on two of the most commonly used indicators of income distribution, the Gini coefficient and Theil's *T*. These measures have appropriate characteristics for the subject of this study (see Allison 1978), as they both include the property of scale invariance (so in this study, neither the units of measurement nor proportionate increases in the income of countries and/or quintiles affects the summary measure of inequality), and meet Dalton's principle of transfers (so transfers of income from poorer to richer countries and/or quintiles result in a higher measure of inequality, while transfers from richer to poorer countries and/or quintiles have the opposite effect). However, each of the measures differ in their sensitivity to transfers. While the Gini index is particularly sensitive to transfers in the middle range of the income distribution, Theil's *T* is more sensitive to transfers affecting low-income earners (a characteristic that is particularly useful under the assumption that income has a diminishing marginal utility). The popularity of the two measures also allows for a more systematic comparison of findings across studies.

The Gini coefficient (*G*), attributable to Corrado Gini (1912), is the oldest and most widely used index, due largely to its strong intuitive appeal. Based on the logic of the Lorenz (1905) curve, the Gini coefficient is the ratio of the area between the lines of absolute equality (the diagonal) and observed inequality in a Lorenz curve to the entire triangular region underneath the diagonal (Sen 1973). The coefficient can vary between zero (total equality) and one (total inequality). Larger Gini coefficients hence represent greater discrepancies between a given distribution and absolute equality. As a result, the Gini coefficient can be compared over time—distributions with larger Gini coefficients are more unequal, and this is true regardless of whether the Lorenz curves intersect.

After arraying the countries in ascending order with respect to GNPPC, the Gini coefficient for the given distribution was calculated using the following formula:

$$\frac{\sum_{j=1}^J \bar{X}_j p_j (q_j - r_j)}{\bar{X}},$$

"where  $q_j$  is the proportion that are in groups with means less than  $\bar{X}_j$ , and  $r_j$  is the proportion in groups with means greater than  $\bar{X}_j$ . Hence  $p_j + q_j + r_j = 1$  for all  $j$ " (Allison 1978, p. 876).

Theil (1967, 1972) developed his measure of inequality from information theory and the concept of entropy. Interpretation of this measure is less intuitive than in the case of the Gini coefficient, but one of the advantages of Theil's  $T$  is that the measure can be easily decomposed into between- and within-group components.<sup>12</sup> Theil's  $T$  varies between zero (when each country's population share equals its income share) and  $\log n$  (complete inequality). With the appropriate changes, the study follows here the usual procedure used to decompose Theil's  $T$  into the between-group and within-group components (Theil 1972; similar procedures are reported in Allison [1978], Bourguignon [1979], Coulter [1989], and Peacock et al. [1988]). This two-level aggregation procedure follows the following equation:

$$T = \sum_{c=1}^C Y_c \ln \frac{Y_c}{X_c} + \sum_{c=1}^C Y_c \left( \sum_{q=1}^Q \frac{Y_q}{X_c} \ln \frac{Y_q/Y_c}{X_q/X_c} \right),$$

where  $X_c$  is the proportion of the total world population in each country  $c$ , and  $X_q$  is the proportion of world population represented by quintile  $q$ . Likewise,  $Y_c$  is the proportion of total world GNP accruing to country  $c$ ,  $Y_q$  is the proportion of total world GNP accruing to quintile  $q$ , and  $\ln$  is the natural log.

This equation yields two relevant terms, each moving closer to zero when a country's (or quintile's) proportion of world GNP is equivalent to its share of world population. The left-most term on the right-hand side of the equation provides a measure of the relative contribution of between-country inequality to overall world income inequality. The right-most term on the right hand-side of the equation provides a measure of the relative contribution of within-country inequality to overall world income inequality. Thus, this procedure identifies the relative contribution of between- and within-country inequalities to overall inequality. The sum of these two components, then, measures the total world income inequality.

### The Contribution of Between- and Within-Country Components

Figures 1 and 2 below show the Lorenz curves for the world income distribution in 1965 and 1992. The curves indicate that inequality became more

<sup>12</sup> For a debate over the most appropriate procedures for decomposing measures of inequality, see Adelman and Levy (1984, 1986), Bourguignon (1979), Cowell (1985, 1988), Das and Parikh (1982), Pyatt (1976), and Silber (1989).

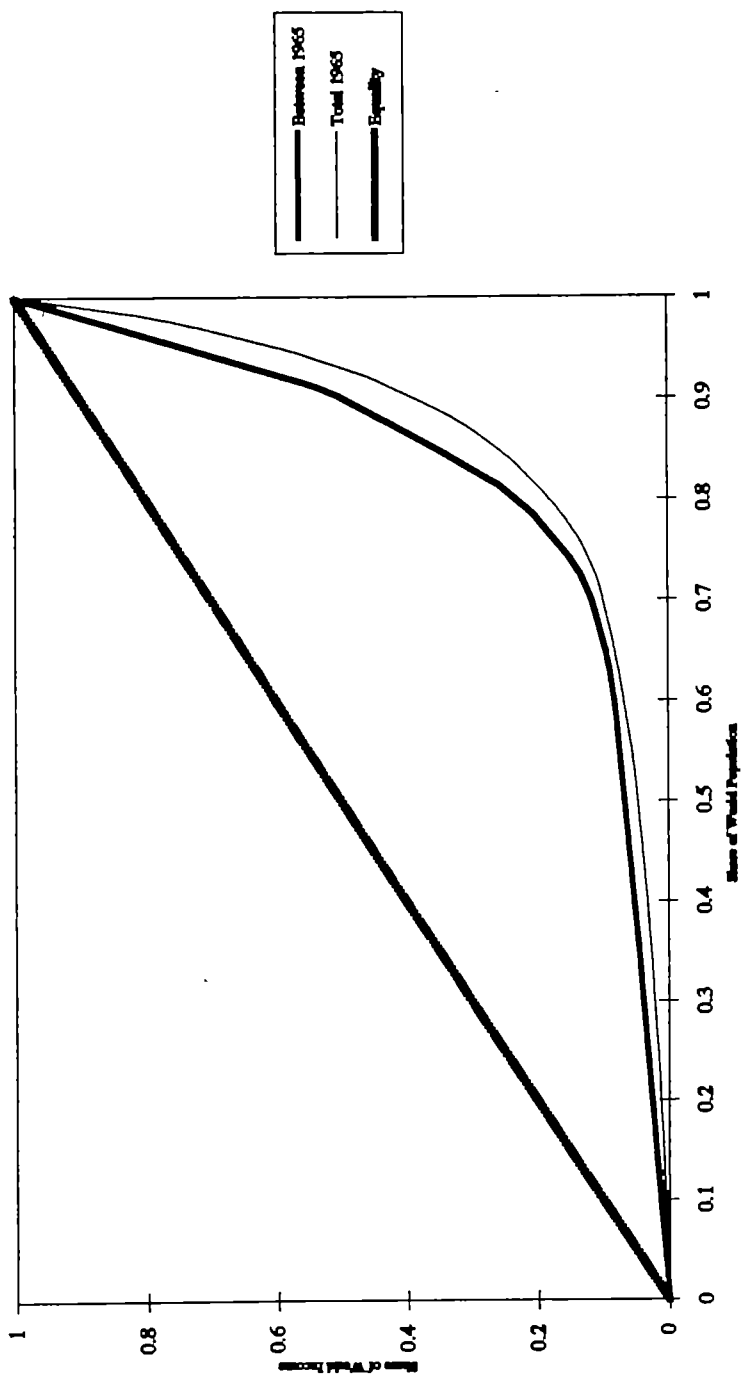


FIG. 1.—World distribution of income, 1965

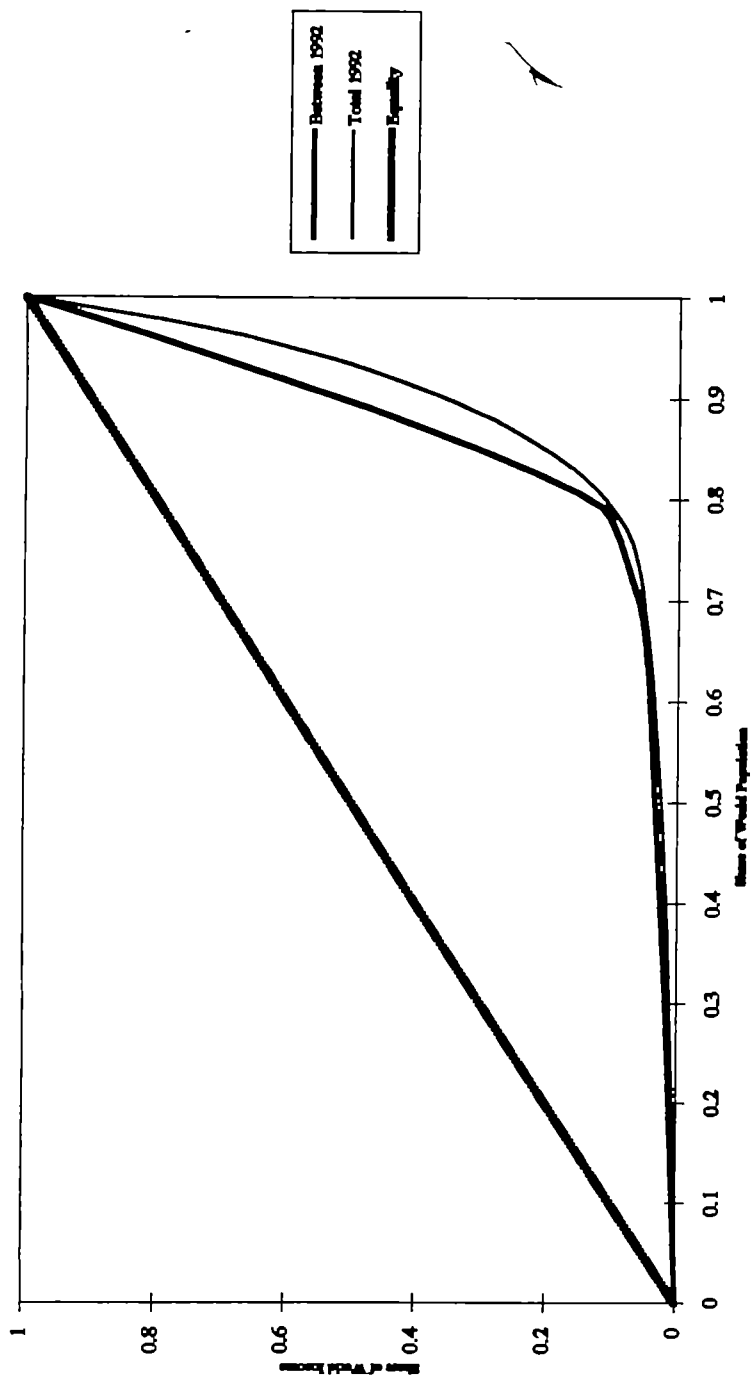


FIG. 2.—World distribution of income, 1992

TABLE 2  
SUMMARY MEASURES OF WORLD INCOME INEQUALITY

Measure	1965	1992	% Change
Gini coefficient:			
Between-country + within-country inequality .....	.749	.796	6.28
Between-country inequality .....	.682	.738	8.21
Proportion of inequality accounted for by between-country inequality .....	.91	.93	
Theil's <i>T</i> .....	1.145	1.321	15.37
Between-country inequalities .....	.902	1.131	25.39
	(78.8)	(85.6)	
Within-country inequalities .....	.243	.190	-21.81
	(21.2)	(14.4)	
Assumption A-*			
Gini coefficient .....	.749	.804	7.34
Theil's <i>T</i> .....	1.145	1.367	19.40
Between-country inequalities .....	.902	1.131	25.39
	(78.8)	(82.7)	
Within-country inequalities .....	.243	.237	-2.47
	(21.2)	(17.3)	

NOTE.—Nos. in parentheses are % of the total inequality (% of *T*)

\* Assumption A = no changes in income distribution within nations

pronounced between 1965 and 1992. The level of inequality depicted here for 1992 is so high that no individual nation in the contemporary world reaches similar levels of inequality in the distribution of income within their borders.<sup>13</sup>

Confirming what the Lorenz curves suggest, the calculated Gini coefficients indeed show a considerable increase between 1965 and 1992. As indicated in table 2, the Gini coefficient for between-country inequality stood at .682 in 1965 and grew to reach .738 (for an overall increase of 8.2%) by 1992. Following a similar trend, the Gini coefficient combining between- and within-country inequalities stood at .749 in 1965 and reached .796 (for an overall increase of 6.3%) by 1992. An even larger increase results from the calculation of Theil's *T*, as the latter rose from 1.145 in 1965 to 1.321 by 1992, for an overall increase of 15.4%. In short, the data suggest quite unequivocally that world income inequality became increasingly pronounced over the 1965–92 period as a whole.

<sup>13</sup> The country with the greatest reported income inequality in the 1990s was Brazil, with a Gini coefficient of .570 (World Bank 1994). This figure is considerably lower than the Gini coefficients derived from between-country inequalities (.739), or the combination of between- and within-country (.796) distributions, for our 1992 world data.

The decomposition of Theil's  $T$  allows an evaluation of the relative contribution of between- and within-country inequalities to overall inequality. As indicated in table 2, in 1965 inequality between countries already accounted for a much larger share of total world inequality (78.8%) than within-country inequality (21.2%). This is also illustrated in a different manner by figure 1 above, as it suggests that the Lorenz curve of the world distribution of income produced by using between-country data is similar to the curve produced by taking into account both between- and within-country data. The decomposition of Theil's  $T$  suggests that by 1992, between-country inequality acquired even greater weight (increasing from 78.8% to 85.6%, as compared to a decline from 21.2% to 14.4% for within-country inequality) in explaining overall inequality. Finally, assumption A in table 2, indicates that even with no change in within-country inequality over the period, similar overall trends would be experienced between 1965 and 1992.

These findings suggest that data on the between-country distribution of world income can indeed be used as appropriate indicators of inequality. Since data on the between-country distribution of income are more forthcoming, such a finding opens the door to more detailed (e.g., over time) and complete (e.g., including large countries such as China) evaluations of trends in world income inequality. More complex measures of the world distribution of income that include both between- and within-country distributions may be more accurate, but they are not likely to alter trends, nor are they likely to significantly affect the magnitude of inequality. This finding is consistent with similar decomposition analyses carried in the past (e.g., Berry et al. 1993b), and in fact is the conclusion reached by Theil (1967) himself in the initial study that developed his summary measure of inequality.

#### THE WORLD DISTRIBUTION OF INCOME, 1965-92

The findings discussed above simplify the task of probing deeper into longitudinal changes in the world distribution of income, as series of between-country data are far more readily available than within-country data. This allows us to expand both the share of the world population included in the sample and the number of years for which the trends at hand can be observed.

#### Data Sources

We rely on the GNPPC and population data available through the World Bank (1988 and various years). Since our study is no longer constrained by the spotty availability of data on within-country income distributions,



the sample now includes the population of 121 countries for which observations were available in 1965 and the subsequent years presented in this article (a sample that accounts for 93.6% of the world population in 1990).<sup>14</sup>

### Summary Measures of Inequality

We use the same procedure to calculate the Gini coefficient as the one reported in the previous section. However, the characteristics of the new data require some modifications in the formula used to calculate Theil. Following Theil (1967),  $T$  is calculated using the following formula:

$$T = \sum_{i=1}^n Y_i \ln \frac{Y_i}{X_i},$$

where  $n$  is the number of countries, where  $X_i$  is the population share of country  $i$ , and  $Y_i$  is its income share. Theil's coefficient, calculated in this manner, weights the pertinent distribution by income.

For the population of the 121 countries that had data available for the entire 1965–90 period, Gini and Theil coefficients were calculated at every five-year interval.<sup>15</sup> To further assess trends in the world distribution of income, we assessed population quintiles by separating the arrayed countries into five groups, each representing 20% of the combined population of the 121 nations in the data set. When a country rested on the border of a quintile (e.g., the cumulative population at that country equaled .234 and the cumulative frequency preceding the country was .198), a simple interpolation was conducted whereby the bordering country's population was divided. Cumulative GNP at these quintile borders could then be calculated. For any given year, dividing the absolute GNP of each quintile by the total GNP gave us the percentage of world GNP controlled by that quintile.

### Trends

Figure 3 shows the Lorenz curves for world income distribution in 1965 and 1990. As in the previous section, both curves indicate a prevalence

<sup>14</sup> The figure for world population in 1990 is from Jazayry et al. (1992, p. 77). As opposed to most of the studies presented earlier in table 1, our data include China and the countries constituting the former Soviet Union. For the latter, we relied on the estimates used in Korzeniewicz and Martin (1994).

<sup>15</sup> We also calculated Gini coefficients for subsequent years in which more countries had data available. Although the total sample was considerably larger (up to 150 countries in 1985), the Gini coefficients were all within .004 of each other and showed the same trends as those later reported in this article.

of profound inequality. Indeed, the summary measures show a constant increase throughout the period considered in this article, with the most pronounced increase taking place in the 1980s. As indicated in table 3 and figure 4 below, the Gini coefficient stood at .658 in 1965, and thereafter grew over every five-year period to reach .739 by 1990 (for an overall increase of 12.3%). Theil, with its greater sensitivity to transfers affecting low income earners, indicates an even larger increase, rising from .802 in 1965 to 1.107 by 1990 (for an overall increase of 38.0%).

There were significant differences in trends among each of the five-year intervals. Most important, income inequality changed relatively less over the 1965-70 and 1975-80 subperiods but became greatly accentuated between 1980 and 1990. For example, whereas the Gini coefficient increased by 3.5% in the 15 years between 1965 and 1980, the coefficient rose by 8.7% in the 10-year period between 1980 and 1990. Likewise, Theil increased by 8.5% between 1965 and 1980, but rose 27.2% in the subsequent decade. To make the same point in a different manner, the rise of inequality in the 1980s accounted for roughly three-quarters of the overall rise in global income inequality during the 1965-90 period (or 71.9% in the case of Gini; 77.7% in the case of Theil).

The character of these trends can be further specified by disaggregating the between-country income data into population quintiles (see table 3). The share of world income accruing to the poorest 40% of the world's population diminished over the 1965-90 period from 5.1% to 3.2%, for an overall decline of 37.3% (after increasing slightly between 1980 and 1985, the relative income of these two quintiles fell precipitously between 1985 and 1990). For the third quintile of the world's population, the share of world income diminished in every five-year period, for an overall decline of 49.2% between 1965 and 1990, with much of this decline taking place after 1985. Over the 1965-90 period as a whole, the three bottom quintiles of the world population experienced a noticeable convergence in their relative share of income (e.g., whereas the ratio of the average GNPPC of the third quintile to the poorest quintile was 1.8 in 1965, by 1990 it had declined slightly to 1.5).

The trends during the 1965-90 period as a whole were more unstable for the fourth population quintile. This quintile was characterized by considerable fluctuations, with its relative share of world income increasing during the 1965-70 and 1975-80 subperiods and declining over the other five-year periods (particularly during the 1980s). For the period as a whole, this quintile experienced a decline of 46.8% in its share of world income.

The richest 20% of the world's population was the only quintile to experience a sustained increase in its share of world income. Between 1965

TABLE 3

WORLD INCOME DISTRIBUTION, 1965-90 (N = 121)

	1965	1970	1975	1980	1985	1990
<b>Poorest 20%</b>						
Quintile 5	GNPPC boundaries	30-90	60-110	120-240	110-290	80-350
	% world GNPPC	2.25	2.17	1.65	1.82	1.38
	GNPPC	74	98	219	253	283
<b>Quintile 4</b>	GNPPC boundaries	90-100	110-130	240-300	290-320	350-370
	% world GNPPC	2.87	2.77	2.21	2.26	1.78
	GNPPC	94	125	293	314	363
<b>Quintile 3</b>	GNPPC boundaries	100-270	130-280	300-1,030	320-810	370-730
	% world GNPPC	4.15	3.81	3.52	3.25	2.11
	GNPPC	136	172	467	451	432
<b>Quintile 2</b>	GNPPC boundaries	270-1,110	280-1,790	1,030-4,443	810-4,123	760-4,060
	% world GNPPC	21.23	21.29	18.28	15.39	11.29
	GNPPC	697	965	2,426	2,136	2,308
<b>Richest 20%</b>						
Quintile 1	GNPPC boundaries	1,110-3,650	1,790-4,980	4,443-17,800	4,123-16,770	4,060-32,790
	% world GNPPC	69.50	69.97	75.38	77.27	83.44
	GNPPC	2,281	3,169	10,006	10,723	17,056
<b>Richest 10%</b>						
Decile 1	% world GNPPC	47.26	46.86	46.27	52.19	56.09
		.658	.662	.682	.703	.740
Gini coefficient		(.658)	(.655)	(.664)	(.683)	(.712)
Thiel coefficient		.802	.814	.870	.945	1.108
		(.802)	(.791)	(.813)	(.868)	(.990)

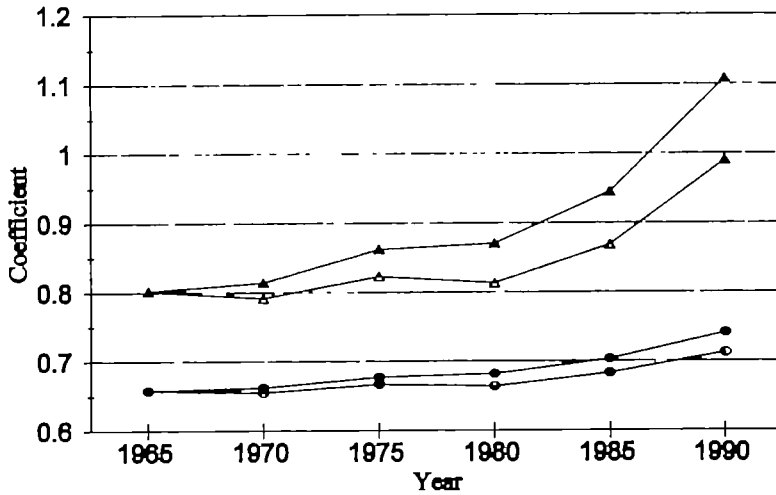


FIG. 4.—Summary measures of inequality, 1964–90; dark triangle = Theil; open triangle = Theilpop; dark circle = Gini; open circle = Ginipop.

and 1990, this quintile's relative share of world income increased from 69.5% to 83.4%, a change of 20.1%. The increase in its share of income rose more rapidly during the 1970–75 and 1980–90 subperiods. Given these trends, the overall distance between the poorest four quintiles and the richest quintile increased significantly over the 1965–90 period as a whole. Thus, whereas the ratio of the average GNPPC of the first quintile to the poorest quintile was 30.9 in 1965, by 1990 it had increased to 60.5. All quintiles lost relative ground to the wealthiest quintile between 1965 and 1990.

The trends that characterized the wealthiest quintile of the population, however, were not evenly distributed within the quintile. As suggested by the data in table 3, while both top deciles increased their relative share of world income over the 1965–90 period, each of the two deciles was characterized by some fluctuations. For the period as a whole, the second decile experienced a relatively greater increase in income (23.0%) than the first decile (18.7%). By 1990, however, the top decile had reached its highest relative share of world income (56.1%) for the period as a whole, and all the increase in its relative income accrued during the 1980s (when its income rose by 21.2%). The second decile, on the other hand, experienced most of the growth in its relative share of world income prior to the 1980s (as its share rose 30.9% between 1965 and 1980), and then underwent an initial decline between 1980 and 1985 before slightly recovering by 1990.

TABLE 4  
WORLD INCOME DISTRIBUTION, 1965 AND 1992 (*N* = 46)

Decile		1965	1992	% Change
Poorest 10%:				
1	GNPPC boundaries	8-39	13-150	
	% world GNPPC	.51	.24	-52.94
	Weighted average GNPPC	35.40	134.00	
2	GNPPC boundaries	39-59	150-239	
	% world GNPPC	.70	.33	-52.86
	Weighted average GNPPC	48.38	182.23	
3	GNPPC boundaries	59-72	239-259	
	% world GNPPC	.92	.46	-50.00
	Weighted average GNPPC	63.61	254.29	
4	GNPPC boundaries	72-91	259-385	
	% world GNPPC	1.10	.58	-47.27
	Weighted average GNPPC	76.05	316.36	
5	GNPPC boundaries	91-100	385-573	
	% world GNPPC	1.43	.80	-44.06
	Weighted average GNPPC	99.02	437.39	
6	GNPPC boundaries	100-176	573-642	
	% world GNPPC	2.13	1.10	-48.36
	Weighted average GNPPC	147.64	605.13	
7	GNPPC boundaries	176-246	642-998	
	% world GNPPC	3.12	1.65	-47.12
	Weighted average GNPPC	216.21	903.87	
8	GNPPC boundaries	246-949	998-6,956	
	% world GNPPC	8.14	5.93	-27.15
	Weighted average GNPPC	564.79	3,248.61	
9	GNPPC boundaries	949-2,245	6,959-21,615	
	% world GNPPC	20.73	24.13	16.40
	Weighted average GNPPC	1,437.83	13,219.30	
Richest 10%:				
10	GNPPC boundaries	2,245-7,501	21,615-54,093	
	% world GNPPC	61.23	64.77	5.78
	Weighted average GNPPC	4,246.10	35,481.06	

These trends become even more stark if the disaggregation of income data by population shares is carried out with data that include both between- and within-country distributions of income. As can be observed in table 4, between 1965 and 1992, the eight poorest world population deciles lost income shares relative to the top two population deciles. The decline was most pronounced for the poorest deciles: the world income share of the poorest 30% of the world population declined from 2.1% to 1.0%, for an overall decline of 51.6% during the 1965-92 period.

For the four intermediate deciles (deciles 4–7 in table 4), the world income share declined from 7.8% to 4.1%, for an overall decline of 46.9% during the same period. The decline was less pronounced in decile 8, but only the top 20% of the world population experienced an increase during the period under consideration, increasing their overall share of world income from 82.0% in 1965 to 88.9% by 1992. In either case, if these population deciles were represented as 10 runners in the midst of a race, with the lead runner already considerably ahead of the slower nine in 1965, by 1992 runner 9 (decile 9) has broken away from the pack and is the only one to gain ground relative to the leader of the race. Meanwhile, the other eight runners (deciles 1–8) have dropped further and further behind.

Some may argue that this striking growth in income inequality could be primarily an outcome of differential rates of population growth in low- and high-income nations. To evaluate this possibility, the study calculated  $T^*$ , which measures inequality in the case of unchanged population shares (Theil 1967, p. 110). Considering 1965 the fixed base year in which the population shares are  $X_1, \dots, X_n$ , new income shares can be calculated for each country in current year  $t$  as follows:

$$Y_i^* = \frac{X_i Z_i}{\sum_{j=1}^n X_j Z_j}, \quad i = 1, \dots, n,$$

where  $Z$  equals the per capita income of country  $i$  and  $Y_i^*$ , then, represents the income share in any current year  $t$  if the population shares in  $t$  were held constant at 1965 levels and if the per capita incomes were actually observed in the current year. Hence,

$$T^* = \sum_{i=1}^n Y_i^* \ln \frac{Y_i^*}{X_i},$$

is the level of inequality that exists when controlling for population.

As indicated by table 3, even if population shares are held constant to their 1965 dimensions, world income inequality as measured by Gini would still have grown by 8.2% between 1965 and 1992 (as compared to the actual growth of 12.3% reported earlier in this section). Likewise, under the same assumption, Theil still shows an increase of 23.4% (as compared to the actual growth of 38.0% previously reported). Furthermore, the overall direction of the trends in world income inequality for every five-year period are essentially similar (with 1965–70 and 1975–80 showing an attenuation of these inequalities, and the 1980s dramatically erasing

all gains made in previous years). In short, if population growth rates were identical among all areas of the world, income inequality would have increased slightly less, but the overall direction of the trends would not be significantly different.

We also evaluated whether the observed trends would be altered by the use of GDPPC rather than GNPPC data, and no significant differences were found. Using available World Bank (1995) data on GDPPC at market prices in current U.S. dollars for the 1965–90 period ( $N = 112$  countries, including China and India), the respective Gini coefficient rose from .671 in 1965, to .701 in 1980 and .738 by 1990. The Gini coefficient obtained for 1990 with the GDPPC data is virtually identical to the Gini coefficient reported for the GNPPC data (.739). The overall increase in inequality obtained through the GDPPC data (10.0%) was slightly less pronounced than observed in the GNPPC data (12.3%), but the direction of trends in each set of data coincided both for each five-year subperiod, and for the 1965–90 period as a whole.

### Mobility

Given rising inequality, to what extent were the populations of particular nations able to challenge the general tendencies and increase their relative control over income? As indicated earlier in the first section, such an evaluation is of considerable importance, as common sense in the field has come to represent the economic success of East Asia as involving a substantial redistribution of world income from wealthy to poor nations.

Focusing on the between-country data ( $N = 121$ ), there was considerable stability over time in the distribution of world population among the income quintiles (see table 5). A chi-square test was applied to the relationship between each country's quintile location in 1965 and in 1990, and this relationship was found to be statistically significant (overall,  $\chi^2 [16; N = 124] = 132.23, P < .0001$ ; as indexed by Cramer's statistic, the strength of the relationship was 0.52). The population of 60.5% of the countries in our sample fell into the same quintile in both 1965 and 1990, and there were only 13 countries that were not classified in the same or adjacent quintiles in both years (upward mobility was most strongly featured in the cases of Botswana, Central African Republic, the Comoros, Indonesia, Lesotho, Oman, South Korea, and Taiwan; downward mobility was most evident for Niger, Nigeria, Madagascar, Mozambique, and Zaire). There were no cases of countries rising from the two poorest quintiles to the wealthiest quintile.

Similar results are suggested by the data that combine between- and within-country information on income distribution. The appendix table at the end of this article shows the 1965 and 1992 ranking and decile

TABLE 5  
POSITION OF NATIONS IN WORLD INCOME DISTRIBUTION, 1965 AND 1990

1990	1965				
	Quintile 5 (17)	Quintile 4 (5)	Quintile 3 (40)	Quintile 2 (31)	Quintile 1 (31)
Quintile 5 (18)	Afghanistan Bangladesh Burundi Chad Ethiopia Malawi China1	Myanmar Nepal Rwanda Tanzania Upper Volta Gambia India1	Madagascar Mozambique Niger Nigeria Zaire India2		
Quintile 4 (19)		Haiti			
Quintile 3 (23)	Central African Republic Comoros Indonesia Lesotho	Benin China2	Angola Bolivia Ivory Coast Egypt Ghana Honduras Kenya Liberia Mauritius	Pakistan Senegal Sri Lanka Sudan Togo Zambia Zimbabwe	Nicaragua



**Quintile 2 Botswana**  
**(42)**

location of the country population quintiles in the sample.<sup>16</sup> For descriptive purposes, "high upward mobility" involved those country quintiles that moved up either two or three deciles. For the period under consideration, 6.4% of the world population was characterized by such mobility.<sup>17</sup> Overall, this group of country quintiles increased its share of world GNP from 1.9% in 1965 to 6.5% by 1992.

If "very high upward mobility" involved a shift of four deciles or more between 1965 and 1992, 0.8% of the world population in 1965 (and 1.0% of the same population in 1992) underwent such mobility. These country quintiles were all related to six countries: Botswana (all four bottom quintiles), Lesotho (fourth quintile), Malaysia (poorest quintile), South Korea (two bottom quintiles), Thailand (second quintile), and Tunisia (second quintile).<sup>18</sup> Overall, the group of country quintiles characterized by "very high upward mobility" increased its share of world GNP from 0.1% in 1965 to 0.4% by 1992.

Other country quintiles underwent opposite trends. Following equivalent criteria, "high downward mobility" involved country quintiles that moved down either two or three deciles. For the period under consideration, 1.0% of the world population in 1965 (and 1.3% of the same population in 1992) was characterized by such mobility. This mobility involved most clearly population quintiles from countries from Africa (e.g., the two

<sup>16</sup> An additional observation is warranted by the information presented in appendix table A1. While the results reported here clearly suggest that data on the distribution of income between countries are likely to provide a sufficiently accurate description of trends in world income inequalities on their own, the use of more disaggregated data (i.e., including information on the distribution of income between countries) provides a more nuanced account of the changes under consideration. For example, according to the data on the distribution of income between nations, the United States fell from being ranked first in our sample for 1965, to number 4 (below Switzerland, Japan, and Denmark) by 1992. However, the more disaggregated data also tell us that the wealthiest quintile of the United States retained its number 1 ranking for both 1965 and 1992. Both pieces of information are relevant: the first serves to identify changes in the relative resources accruing to a nation (a variable that is of crucial importance in explaining overall world income inequalities, as we have shown above); the second helps explain how different populations are actually affected by gains and losses in national income.

<sup>17</sup> This mobility involved most clearly population quintiles from countries from Asia (the poorest quintile of Sri Lanka and various quintiles of Japan, Malaysia, South Korea, and Thailand) and to a lesser extent from Latin America (e.g., the poorest quintiles of Jamaica and Mexico, and the poorest and middle quintiles of Brazil) and Africa (e.g., the wealthiest quintile of Botswana, the poorest and middle quintiles of Tunisia, and assorted quintiles from Lesotho, Morocco, Mauritius).

<sup>18</sup> While some of the Asian cases have received considerable attention in the development literature, this has not been the case of countries such as Botswana, a country characterized by some of the highest rates of GNP and GDP growth in the world during the 1980s and 1990s.

wealthiest quintiles of Tanzania and the middle quintile of Zimbabwe) and Latin America (e.g., the three poorest quintiles of Guatemala, the poorest quintile of Peru and Panama, and the fourth quintile in Honduras), and to a lesser extent from Asia (e.g., the fourth quintile of Bangladesh). Overall, this group of country quintiles decreased its share of world GNP from 0.15% in 1965 to 0.06% by 1992.<sup>19</sup>

In general terms, however, the main characteristic of the period was a striking stability in the relative standing of the population of poor and wealthy nations. "Stability" can be defined as involving country quintiles that remained in the same income decile or moved only to an adjacent income decile for the 1965–92 period as a whole. Hence defined, approximately 91.7% of the world population in 1965 (and 91.4% of the same population in 1992) experienced relative stability over the period as a whole. Overall, this share of the world population accounted for 93.1% of the 1992 world GNP in the sample under consideration (and 97.9% in 1965). Such stability further corroborates the few authors who have highlighted the growing inequalities of the 1980s (e.g., Arrighi 1991; Chase-Dunn 1989; Passé-Smith 1993a, 1993b), while challenging the hypothesis (e.g., as raised in Ram 1989) that world inequality in the distribution of income has been abating.

## DISCUSSION

The world distribution of income has become more unequal over the 1965–90 period, and while the gap between the populations of wealthy and poor nations has grown steadily since 1965, it intensified during the worldwide recession of the 1980s. These findings are robust even when controlling for population growth or using alternative sources of data. By decomposing world income inequality into between- and within-country components, we also found strong evidence that between-country inequalities are of significantly greater importance in shaping the trends in question. Overall, while between-country inequality has become more pronounced over the period under consideration, the opposite was the case of within-country inequality. However, the attenuation of income inequality within nations was not nearly sufficient to compensate for the accentuation of between-country inequality. Inequality in the distribution of in-

<sup>19</sup> According to our criteria, there were no cases of "very high downward mobility" in our sample for the period under consideration. Of course, it might be the case that the type of data analyzed in this study are less likely to be reported by state authorities and public officials in countries experiencing slow economic growth, high levels of poverty, and/or a deterioration of standards of living. In this sense, our results (particularly in regard to downward mobility) must be interpreted with caution.

come between-countries continues to be of essential importance to global social stratification.

The first section of this article notes that empirical studies on world income inequality are characterized by ambivalent findings. The trends in the distribution of world income found in this study, however, can be used to identify possible sources of these discrepant results. Most of these empirical studies were conducted with data from the 1960s and 1970s, a period in which trends of inequality were not as pronounced as they became in the 1980s. Given these conditions, minor differences in methodological procedures (e.g., in the percentage of the world population included in a sample, or in the indicator used to measure the distribution of income among nations) were likely to sway results in different directions. World income inequality grew at a more rapid pace during the 1980s, a decade when empirical research on this issue was lacking.<sup>20</sup> Empirical studies that include this particular decade in their analysis, rather than restrict their investigation to the 1960s and 1970s, are likely to inevitably produce results that corroborate the main findings of this article.

The trends identified here also help understand the recent theoretical convergence discussed in the first section. On the one hand, indications of the persistence and deepening of world income inequality in the 1980s (slow growth in areas such as Latin America and Africa and high growth among core nations) are likely to have influenced scholars within the field of economics toward greater emphasis on the importance of institutional development and endogenous variables shaping growth. At the very least, such trends provided strong intuitive ammunition to analysts seeking to challenge the notion that exposure to markets alone would serve to reduce the income gap between wealthy and poor nations. On the other hand, such trends were accompanied by noteworthy exceptions. For the population of some poor and middle-income nations (such as those in East Asia), development strategies appeared to play an important role in generating sufficient economic growth to escape the polarization of income. These exceptions were particularly influential in shaping critical studies of the political economy of development within other disciplines in the social sciences.

While advancing our understanding of the current theoretical conver-

<sup>20</sup> Our findings are compatible with the argument that periods of global economic downturn are likely to *accentuate* the gap between peripheral countries and the rest of the world-economy (core and semiperipheral nations), while producing mixed results for semiperipheral countries (with upward mobility for some and relative stagnation for others; see Wallerstein 1980, p. 241). Other authors within the same perspective have suggested that the outcome of global economic downturns varies over time, with no clear pattern characterizing the trajectory of the different groups of nations (Arrighi, Korzeniewicz, and Martin 1986; Arrighi 1994).

gence that characterizes this field of inquiry, the findings reported in this article are insufficient to conclusively support one theoretical approach above all others in areas of continuing contention. Adherents of a world-systems perspective might claim that the persistence of inequality reflects the constraints inherent in the world-economy for the vast majority of the population. Other theoretical approaches might argue that such inequalities merely reflect the incomplete adoption of market-oriented strategies of growth in countries bounded by conflicts and negotiations involving rent-seeking interests, and that as countries abandon state-centered strategies of growth in favor of markets, a shift toward greater growth and social equality might be expected sometime in the future.

In this respect, however, the coming decade is likely to provide fundamental evidence as to whether a shift toward market-centered strategies of growth will deliver greater inequality or a more proportionate distribution of economic resources in the world-economy. This article joins others (e.g., Robinson 1976; Breedlove and Nolan 1988; Peacock et al. 1988) to suggest easily implemented procedures that can be followed in the near future to continue tracing trends in world income inequality. By endorsing the use of easily available between-country data in such inquiries, the findings presented in this study will make it easier to closely track the future impact of market-centered strategies of growth on world income inequality and to further evaluate the relevance of alternative theoretical approaches.

Our findings indicate that efforts to account for patterns of development in the world-economy must include polarization as a crucial component of recent trends. Findings in this respect are rather clear. The world distribution of income became more unequal over the 1965–90 period, and inequality accelerated during the 1980s. Some populations (such as in East Asia) experienced upward mobility in a “world hierarchy of income,” but such mobility is limited when compared to polarizing tendencies in the world distribution of income as a whole. These results highlight the continuing need for more detailed inquiries into the processes that generate growing inequality in the world distribution of income. In challenging current commonsense expectations regarding the economic opportunities easily available to the majority of the world population, such findings call at the very least for pause and critical reflection.

TABLE A1  
RANK AND DECILE POSITION OF COUNTRY QUINTILES

Rank (1992)	Country Quintile	Rank (1965)	Decile Position (65,92)	Rank (1992)	Country Quintile	Rank (1965)	Decile Position (65,92)
1	Tanzania1	4	(D1,D1)	58	Honduras4	97	(D7,D5)
2	Tanzania2	11	(D1,D1)	59	Guatemala3	100	(D7,D5)
3	Tanzania3	22	(D2,D1)	60	Mauritius1	39	(D3,D5)
4	Honduras1	6	(D1,D1)	61	Morocco2	52	(D4,D5)
5	Lesotho1	2	(D1,D1)	62	Senegal3	62	(D6,D5)
6	Guatemala1	47	(D4,D1)	63	Ivory Coast3	67	(D6,D5)
7	Tanzania4	35	(D3,D1)	64	Philippines3	64	(D6,D5)
8	Bangladesh1	9	(D1,D1)	65	Thailand1	21	(D2,D5)
9	Zimbabwe1	14	(D1,D1)	66	China4	61	(D5-6,D5-6)
10	India1	13	(D1,D1)	67	Lesotho4	8	(D1,D6)
11	Senegal1	10	(D1,D1)	68	Sri Lanka4	91	(D6,D6)
12	Bangladesh2	18	(D1,D1)	69	India5	112	(D7,D6)
13	China1	19	(D1-2,D1-2)	70	Malaysia1	27	(D2,D6-7)
14	Honduras2	25	(D2,D2)	71	Peru3	90	(D6,D7)
15	Pakistan1	16	(D1,D2)	72	Jamaica2	92	(D6,D7)
16	Zimbabwe2	28	(D2,D2)	73	Brazil2	76	(D6,D7)
17	Bangladesh3	24	(D2,D2)	74	Venezuela1	113	(D7-8,D7)
18	Lesotho2	3	(D1,D2)	75	Mexico1	53	(D4,D7)
19	Bolivia1	12	(D1,D2)	76	Ivory Coast4	82	(D6,D7)
20	India2	26	(D2,D2)	77	Bolivia4	81	(D6,D7)
21	Peru1	51	(D4,D2)	78	Senegal4	88	(D6,D7)
22	Bangladesh4	44	(D4,D2)	79	Panama2	108	(D7,D7)
23	Colombia1	15	(D1,D2-3)	80	Morocco3	56	(D4,D7)
24	Sri Lanka1	17	(D1,D3)	81	Philippines4	96	(D7,D7)
25	Panama1	72	(D6,D3)	82	Pakistan5	114	(D8,D7)
26	Ivory Coast1	48	(D4,D3)	83	Colombia3	83	(D6,D7)
27	Philippines1	20	(D2,D3)	84	Thailand2	34	(D3,D7)
28	India3	42	(D3-4,D3)	85	Costa Rica2	89	(D6,D7)
29	China2	30	(D2-3,D3-4)	86	Tunisia2	38	(D3,D7)
30	Pakistan2	33	(D3,D4)	87	Guatemala4	101	(D7,D7)
31	Senegal2	40	(D3,D4)	88	Chile2	121	(D8,D7)
32	Guatemala2	71	(D6,D4)	89	Botswana2	5	(D1,D7)
33	Zimbabwe3	77	(D6,D4)	90	Jamaica3	120	(D8,D7)
34	Brazil1	23	(D2,D4)	91	China5	95	(D6-7,D7)
35	Honduras3	54	(D4,D4)	92	Peru4	119	(D8,D7-8)
36	Bolivia2	41	(D3,D4)	93	Mauritius2	84	(D6,D8)
37	India4	60	(D5,D4)	94	Sri Lanka5	132	(D8,D8)
38	Lesotho3	7	(D1,D4)	95	Morocco4	79	(D6,D8)
39	Morocco1	50	(D4,D4)	96	Malaysia2	69	(D6,D8)
40	Tanzania5	111	(D7,D4)	97	Brazil3	80	(D6,D8)
41	Sri Lanka2	43	(D4,D4)	98	Thailand3	73	(D6,D8)
42	Pakistan3	57	(D4,D4)	99	Tunisia3	68	(D6,D8)
43	China3	45	(D4,D4-5)	100	Mexico2	86	(D6,D8)
44	Philippines2	49	(D4,D5)	101	Colombia4	115	(D8,D8)
45	Costa Rica1	65	(D6,D5)	102	Venezuela2	138	(D8,D8)
46	Ivory Coast2	59	(D5,D5)	103	Costa Rica3	107	(D7,D8)
47	Jamaica1	32	(D3,D5)	104	Panama3	125	(D8,D8)
48	Bangladesh5	87	(D6,D5)	105	Chile3	129	(D8,D8)
49	Peru2	55	(D4,D5)	106	Ivory Coast5	141	(D8,D8)
50	Sri Lanka3	66	(D6,D5)	107	Jamaica4	142	(D8,D8)
51	Pakistan4	75	(D6,D5)	108	Mauritius3	117	(D8,D8)
52	Bolivia3	63	(D6,D5)	109	Botswana3	36	(D3,D8)
53	Zimbabwe4	78	(D6,D5)	110	Bolivia5	143	(D8,D8)
54	Botswana1	1	(D1,D5)	111	Lesotho5	106	(D7,D8)
55	Chile1	94	(D6,D5)	112	Zimbabwe5	159	(D8,D8)
56	Colombia2	46	(D4,D5)	113	Malaysia3	99	(D7,D8)
57	Tunisia1	29	(D2,D5)	114	Philippines5	137	(D8,D8)

TABLE A1 (Continued)

Rank (1992)	Country Quintile	Rank (1965)	Decile Position (65,92)	Rank (1992)	Country Quintile	Rank (1965)	Decile Position (65,92)
115	Honduras5	151	(D8,D8)	173	Japan1	104	(D7,D9)
116	Thailand4	74	(D6,D8)	174	Italy2	152	(D8,D9)
117	Tunisia4	93	(D6,D8)	175	Spain3	144	(D8,D9)
118	Venezuela3	164	(D8,D8)	176	France2	157	(D8,D9)
119	Mexico3	116	(D8,D8)	177	Finland2	155	(D8,D9)
120	Costa Rica4	122	(D8,D8)	178	Netherlands2	156	(D8,D9)
121	Chile4	147	(D8,D8)	179	Germany2	168	(D9,D9)
122	Senegal5	146	(D8,D8)	180	USA2	204	(D9-10,D9)
123	Brazil4	105	(D7,D8)	181	New Zealand4	206	(D10,D9)
124	Mauritius4	118	(D8,D8)	182	South Korea5	109	(D7,D9)
125	Morocco5	153	(D8,D8)	183	Great Britain3	191	(D9,D9)
126	Peru5	181	(D9,D8)	184	Australia3	198	(D9,D9)
127	Panama4	128	(D8,D8)	185	Denmark2	173	(D9,D9)
128	South Korea1	31	(D3,D8)	186	Israel4	187	(D9,D9)
129	Botswana4	37	(D3,D8)	187	Spain4	161	(D8,D9)
130	Malaysia4	124	(D8,D8)	188	Norway2	174	(D9,D9)
131	Guatemala5	165	(D8,D8)	189	Italy3	172	(D9,D9)
132	New Zealand1	160	(D8,D8)	190	Sweden2	180	(D9,D9)
133	Venezuela4	178	(D9,D8)	191	Canada3	209	(D10,D9)
134	Jamaica5	195	(D9,D8)	192	Netherlands3	177	(D9,D9)
135	Mexico4	134	(D8,D8)	193	Japan2	136	(D8,D9)
136	Colombia5	171	(D9,D8)	194	France3	188	(D9,D9)
137	Australia1	148	(D8,D8)	195	Germany3	182	(D9,D9)
138	Israel1	133	(D8,D8)	196	Finland3	184	(D9,D9)
139	Tunisia5	154	(D8,D8)	197	Australia4	208	(D10,D9)
140	Great Britain1	135	(D8,D8)	198	Great Britain4	202	(D9,D9-10)
141	South Korea2	58	(D4-D5,D8)	199	USA3	216	(D10,D10)
142	Thailand5	127	(D8,D8)	200	Sweden3	210	(D10,D10)
143	Costa Rica5	175	(D9,D8)	201	Denmark3	199	(D9,D10)
144	South Korea3	70	(D6,D8)	202	Italy4	189	(D9,D10)
145	Spain1	103	(D7,D8)	203	Netherlands4	194	(D9,D10)
146	Canada1	149	(D8,D8)	204	Norway3	196	(D9,D10)
147	USA1	169	(D9,D8)	205	Japan3	150	(D8,D10)
148	France1	98	(D7,D8)	206	Canada4	215	(D10,D10)
149	New Zealand2	179	(D9,D8)	207	Spain5	190	(D9,D10)
150	Finland1	102	(D7,D8)	208	France4	207	(D10,D10)
151	Italy1	126	(D8,D8-9)	209	Israel5	212	(D10,D10)
152	Denmark1	140	(D8,D9)	210	New Zealand5	221	(D10,D10)
153	Venezuela5	213	(D10,D9)	211	Germany4	197	(D9,D10)
154	Panama5	185	(D9,D9)	212	Finland4	201	(D9,D10)
155	South Korea4	85	(D6,D9)	213	USA4	223	(D10,D10)
156	Malaysia5	166	(D8,D9)	214	Japan4	170	(D9,D10)
157	Israel2	163	(D8,D9)	215	Norway4	203	(D9,D10)
158	Norway1	131	(D8,D9)	216	Sweden4	217	(D10,D10)
159	Germany1	139	(D8,D9)	217	Denmark4	211	(D10,D10)
160	Mauritius5	158	(D8,D9)	218	Australia5	220	(D10,D10)
161	Botswana5	110	(D7,D9)	219	Netherlands5	219	(D10,D10)
162	Netherlands1	123	(D8,D9)	220	Great Britain5	222	(D10,D10)
163	Chile5	193	(D9,D9)	221	Finland5	224	(D10,D10)
164	Great Britain2	167	(D8-9,D9)	222	Canada5	227	(D10,D10)
165	Brazil5	162	(D8,D9)	223	Italy5	214	(D10,D10)
166	Spain2	130	(D8,D9)	224	Germany5	226	(D10,D10)
167	Australia2	183	(D9,D9)	225	France5	228	(D10,D10)
168	Mexico5	186	(D9,D9)	226	Norway5	218	(D10,D10)
169	New Zealand3	205	(D10,D9)	227	Sweden5	229	(D10,D10)
170	Sweden1	145	(D8,D9)	228	Denmark5	225	(D10,D10)
171	Israel3	176	(D9,D9)	229	Japan5	200	(D9,D10)
172	Canada2	192	(D9,D9)	230	USA5	230	(D10,D10)

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# Escaping Distressed Neighborhoods: Individual, Community, and Metropolitan Influences<sup>1</sup>

Scott J. South and Kyle D. Crowder  
*State University of New York at Albany*

This article links longitudinal data from the Panel Study of Income Dynamics with information on respondents' census tracts to examine patterns of annual residential mobility between poor and non-poor neighborhoods. Education and marriage increase the likelihood of leaving poor tracts, while age, home ownership, and receiving public assistance reduce it. Blacks are substantially less likely than whites to escape poor tracts and substantially more likely to move into them, even after socioeconomic status is controlled. Residential segregation by race and poverty status and the supply of new housing in the metropolis also influence the likelihood of moving between distressed and nondistressed neighborhoods.

Growing concern over the development of an urban underclass and the apparently increasing geographic concentration of poverty has kindled interest in the ability of persons living in poor or distressed neighborhoods to escape these areas for better environs (Kasarda 1989; Massey, Gross, and Shibuya 1994; Wilson 1987). Residential mobility out of poor neighborhoods and into better ones is believed to enhance employment and educational prospects, to reduce exposure to crime, and to increase access to a variety of valued services and facilities, especially for African-Americans (Alba, Logan, and Bellair 1994; Massey, Condran, and Denton 1987; Rosenbaum and Popkin 1991; Wilson 1979). More generally, even after controlling for the socioeconomic characteristics of individuals and families, neighborhood characteristics have been shown to exert important effects on a variety of social, psychological, and economic outcomes,

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including educational attainment (Crane 1991), marital and nonmarital fertility (Billy and Moore 1992; Hogan and Kitagawa 1985), sexual activity (Brewster, Billy, and Grady 1993; Mosher and McNally 1991), criminal victimization (Miethe and McDowall 1993; Smith and Jarjoura 1989), life satisfaction (Fernandez and Kulik 1981), and children's cognitive development (Brooks-Gunn et al. 1993; Entwisle, Alexander, and Olson 1994). By and large, problematic behaviors and experiences appear to be more common in socioeconomically distressed neighborhoods; thus, it is frequently assumed that migration out of these neighborhoods and into more advantaged environments will prove beneficial for those who can escape poor areas (Jencks and Mayer 1990; Rosenbaum and Popkin 1991; Rosenbaum et al. 1991).

It is surprising, however, that few studies have directly examined factors that impede or facilitate the residential mobility or the locational choices of persons initially residing in impoverished neighborhoods. Perhaps the most popular model of local residential mobility is Speare's (1974; Speare, Goldstein, and Frey 1975) residential satisfaction perspective and the modifications and extensions of that framework (Bach and Smith 1977; Heaton et al. 1979; Landale and Guest 1985; Lee, Oropesa, and Kanan 1994; Newman and Duncan 1979; Rossi [1955] 1980). This model identifies a set of personal and life-cycle factors that influence the decision to move either directly or, by affecting satisfaction with one's current residence, indirectly. Housing characteristics (Deane 1990; McHugh, Gober, and Reid 1990) and neighborhood characteristics (Boehm and Ihlandfeldt 1986; Lee et al. 1994; South and Deane 1993) are also considered to be important initiators of residential mobility. Yet, these studies of intraurban residential mobility rarely disaggregate geographic moves by characteristics of the neighborhood of origin or destination (South and Deane 1993; Speare et al. 1975; cf. Lee et al. 1994; Nelson and Edwards 1993), concentrating instead on factors influencing the decision to move *per se* without regard to the qualities of either the old or new neighborhood (Long 1988). Many of the residential moves studied by these investigations are likely to be within the same neighborhood or to neighborhoods of similar socioeconomic standing. Thus, these studies shed only limited light on factors influencing residential mobility out of or into poor neighborhoods. Moreover, these studies of local residential mobility rarely focus specifically on the mobility experiences of African-Americans, despite considerable theoretical and policy debate over the apparent inability of poor blacks to escape underclass neighborhoods (Fairchild and Tucker 1982; Galster 1992).

Another area of research that bears on the study of residential mobility out of poor neighborhoods is the vast literature on residential segregation

between minority and majority groups and between different social classes (Farley 1991; Farley and Frey 1994; Lieberman and Carter 1982; Massey and Denton 1993; Massey and Eggers 1990; Simkus 1978). The high levels of segregation between racial and ethnic minorities and the white, non-Hispanic majority have been documented extensively (Massey and Denton 1987, 1988), as have the moderate changes that have occurred at the metropolitan level (Farley and Frey 1994) and within individual census tracts (Lee and Wood 1991; Wood and Lee 1991). High levels of segregation and housing discrimination imply that minorities, especially blacks, are spatially constrained to neighborhoods of relatively low socioeconomic status (Logan and Alba 1993). Implicit in these studies of the aggregate spatial distributions of racial and ethnic groups is that the underlying dynamic shaping patterns of—and changes in—neighborhood residential differentiation is residential mobility. Yet, rarely do these studies examine the characteristics of individuals and households that move from a neighborhood with a known socioeconomic status and/or racial composition to a different neighborhood having different socioeconomic and/or racial characteristics (see Gramlich, Laren, and Sealander 1992; Massey et al. 1994; Nelson and Edwards 1993). As Massey and Denton (1985, p. 104) note, cross-sectional studies of the geographic distributions of racial and economic groups “cannot capture the full dynamics of the mobility process.”

The primary purpose of this article is to identify the characteristics of individuals and households as well as neighborhoods and metropolitan areas that impede or facilitate residential mobility between poor and non-poor neighborhoods. We attach information on the census tract and metropolitan area of residence at each annual interview for respondents in the 1979 to 1985 waves of the Panel Study of Income Dynamics (PSID) to trace the mobility experiences of persons initially residing in poor and nonpoor areas. We estimate binary and multinomial logistic regression models to address four broad questions regarding residential mobility between poor and nonpoor neighborhoods. First, what social and demographic characteristics of individuals and their communities enable some people to escape poor neighborhoods for better areas? Second, how do these same characteristics influence the risk of moving from nonpoor areas into poor ones? Third, to what extent are racial differences in mobility between poor and nonpoor neighborhoods attributable to differences in sociodemographic attributes and locale characteristics? And, fourth, in light of substantial barriers to residential mobility among African-Americans (Massey and Denton 1993), do the factors that influence mobility between poor and nonpoor neighborhoods operate differently for blacks and whites?



## THEORETICAL FRAMEWORK

Studies of the geographic location of social and economic groups in the United States have generally been informed by two broad theoretical perspectives. One perspective, with roots in the early ecological studies of the Chicago school (Park, Burgess, and McKenzie 1925), argues in part that cities serve as vast sorting machines that channel persons of similar socioeconomic status, ethnicity, and life-cycle stage into relatively homogeneous (or "natural") areas, often circumscribed by physical boundaries. Patterns of residential differentiation by social class emerge as persons "match" their own socioeconomic status with that of their neighborhood, using, to the extent possible, their human capital and other endowments to purchase residences in the most desirable neighborhood. Residential mobility from less desirable to more desirable neighborhoods is considered to be a natural consequence of social and occupational mobility and, to a somewhat lesser extent, the locational preferences associated with various stages in the life course (e.g., the presence of children). This perspective has most often been used to explain the geographic dispersal of minority and immigrant groups concomitant with their assimilation and acculturation into American society (Massey 1985), as socioeconomic progress made by these groups is converted into residential integration with the white majority and "upgrading from central-city slums to working-class neighborhoods to suburbs" (Logan and Alba 1993, p. 244). But the spatial assimilation of minority-group members is, in large measure, a special case of a more general process in which social and economic advancement by initially disadvantaged persons, including members of the white majority, translates into residential mobility into desirable neighborhoods. Consistent with this perspective, Logan and Alba (1993) show that the median income of suburban communities is positively associated with individual human capital characteristics such as income and education. When applied to the experiences of socioeconomically disadvantaged populations (especially minority or immigrant groups), this perspective is frequently referred to as the spatial assimilation model (Alba and Logan 1991; Guest 1980; Massey 1985). Because in this article we are primarily concerned with the residential mobility patterns of different socioeconomic groups (without regard to ethnic origins), we refer to this perspective as the *human capital/life-cycle model* of mobility.

Neighborhood residential differentiation results largely, of course, from the movements of many individual households from one neighborhood to another as well as from life-course changes among the individuals in those households (White 1987). While research on intraurban residential mobility tends to emphasize life-cycle factors as initiators of mobility (Lee et

al. 1994; Rossi 1980) and to ignore the specific characteristics of the neighborhoods of origin and destination, it, too, suggests that economically mobile persons seek the subjective and concrete advantages conferred by residence in richer neighborhoods. Rossi (1980, pp. 226–27), for example, argues that “families moving up the ‘occupational ladder’ are particularly sensitive to location and use residential mobility to bring their residences into line with their prestige needs.”

Taken together, the human capital/life-cycle mobility model and micro-level studies of residential mobility suggest several salient influences on the probability of moving between poor and nonpoor neighborhoods. First, among residents of poor neighborhoods, *socioeconomic differences* should distinguish those who move to nonpoor areas and those who either remain behind or move to other poor areas. High income and employment stability and the prospects for attaining these, as indicated by high levels of educational attainment, should increase the desire and/or capacity to leave poor neighborhoods. Similarly, among persons initially residing in nonpoor neighborhoods, socioeconomic advantages should buffer the risk of moving into a poor area. In addition to the current level of financial resources, residential mobility between poor and nonpoor neighborhoods might also be caused by recent changes in economic conditions. Sharp increases in income and becoming employed are likely to promote mobility out of poor areas, while reductions in income and job loss are apt to promote mobility from nonpoor into poor neighborhoods. Prior studies have shown that various dimensions of socioeconomic status are only weakly related to thoughts of moving (McHugh et al. 1990) and to the actual probability of local residential mobility without regard to the qualities of the neighborhoods of origin or destination (Deane 1990; Lee et al. 1994). But ignoring these neighborhood characteristics might mask important socioeconomic effects on the probability of moving between poor and nonpoor areas. For example, if high-income or highly educated residents are more likely than others to vacate poor neighborhoods but less likely to move within such areas, or if socioeconomically advantaged persons are more likely to leave poor areas but less likely to leave rich ones, then socioeconomic influences on mobility would not be detected by these studies. It has been shown that income is positively related to migration across states and regions (Long 1988).

Apart from the level of income, the source of income might also influence the likelihood of escaping poor neighborhoods. Kasarda (1988; see also Clark 1990), for example, argues that the receipt of public assistance and public housing mires poor people in poor neighborhoods. By partially substituting for gainful employment, welfare dependence serves to impede residential mobility, as well as long-distance migration, to areas with superior job opportunities. Nelson and Edwards (1993) report that the receipt

of public housing assistance retards mobility in at least some metropolitan areas; unfortunately, because they measure housing assistance at the end rather than at the beginning of a mobility period, their findings are subject to alternative interpretations.

A second set of explanatory variables relevant to this perspective on residential mobility out of poor neighborhoods emphasizes *life-cycle characteristics*. However, while these variables have been linked to the overall probability of moving, it is generally less certain how they might affect the choice of a new location. In the aggregate, age is inversely related to residential mobility (Lee et al. 1994; Long 1988; South and Deane 1993). Rates of mobility tend to peak in the young adult years, as these persons leave the parental home, get jobs, marry, attend college, and experience other life-course transitions that necessitate a change in residence. It seems likely that, for many young people, these moves will be to a neighborhood of different economic status than the neighborhood of origin. The age profile of migration begins to decline sharply at about age 30, generally flattening out or declining only modestly above age 50 (Castro and Rogers 1983; Long 1988). Married persons tend to move less frequently than the never married (South and Deane 1993), but recent changes in marital status might be more relevant than the current marital status to moving between poor and nonpoor areas. Given the higher poverty rates of single than married persons, marriage (or remarriage) is likely a route out of poor neighborhoods, while the deteriorating economic circumstances precipitated by marital disruption may cause the newly divorced or widowed to experience downward neighborhood mobility. While the presence of children in the household increases thoughts of mobility (McHugh et al. 1990), on balance children impede residential mobility, perhaps because they increase families' social ties to—and investments in—the neighborhood. Yet, it seems reasonable to propose that when families with children do move, they will be more likely to choose a nonpoor rather than a poor neighborhood. Neighborhood characteristics such as safety and school quality, which are likely to be correlated with a local community's socioeconomic status, should be particularly salient for families with children. Thus, we anticipate that the presence of children in the household will increase the likelihood of moving from a poor to a nonpoor neighborhood, and to reduce the probability of moving in the reverse direction.

Two features of the housing unit—whether it is owned or rented and how crowded it is—have also been shown to influence the likelihood of moving. Because of greater financial investments in the current dwelling and the greater costs of moving, homeowners exhibit substantially lower rates of residential mobility than do renters (Deane 1990; Lee et al. 1994; Speare et al. 1975). However, for those who are moving from a poor neighborhood, it seems likely that homeowners will be more likely than renters

to select a nonpoor rather than an equally poor location. Home equity provides homeowners greater financial resources to purchase residences in wealthier areas. Given their barriers to mobility, homeowners may be pulled away from their current neighborhood only by attractive alternatives in nonpoor communities; absent these alternatives, they are more likely than renters to remain in their current home. Residential crowding is often given as a reason for moving and is believed to predispose potential movers to seek a new residence (Rossi 1980). The empirical evidence linking crowding to residential mobility is somewhat equivocal, however, with some studies (e.g., South and Deane 1993) but not others (e.g., Deane 1990) observing the expected positive association. We anticipate that residential crowding will influence not only the decision to move but also the choice of a new location. Crowding is likely to differentiate movers to nonpoor neighborhoods from those who do not move or who move to poor areas; persons who move to alleviate household crowding should be especially likely to choose large residences in neighborhoods of low density—characteristics associated with comparatively wealthy neighborhoods.

Finally, with its emphasis on educational and occupational mobility as causes of geographic dispersal, the human capital/life-cycle mobility model implies that differences among racial (and ethnic) groups in their residential patterns are, to a considerable degree, reflections of compositional differences among these groups (Alba and Logan 1991; Logan and Alba 1993). In the context of this study, this feature of the model suggests that differences between blacks and whites in mobility rates between poor and nonpoor neighborhoods are largely attributable to racial differences in socioeconomic and life-cycle characteristics. Although blacks appear to be less likely than whites to move from poor to nonpoor neighborhoods and more likely than whites to move from nonpoor to poor areas (Gramlich et al. 1992; Massey et al. 1994), these differences should, theoretically, disappear after socioeconomic and life-cycle characteristics are held constant. Moreover, there appears to be little, if anything, in this model to suggest that the effects of human capital and life-cycle variables on the probability of moving between poor and nonpoor neighborhoods will differ for blacks and whites.

While the human capital/life-cycle mobility perspective is largely consistent with the cross-sectional locational patterns and residential mobility experiences of whites and nonblack minorities, on several counts it appears incapable of explaining the situation among blacks. Blacks appear less likely than other minority groups and non-Hispanic whites to convert human capital characteristics into desirable neighborhood amenities, such as relative freedom from exposure to crime (Alba et al. 1994), suburban location (Alba and Logan 1991), community wealth (Logan and Alba

1993), and associated areal resources (Massey and Denton 1985; Villemez 1980). Blacks also appear less able than other racial and ethnic groups to translate economic achievements into contact with the white majority (Alba and Logan 1993; Massey and Denton 1987; Massey and Mullan 1984). Moreover, substantial racial differences in residential mobility have also been documented, with blacks less likely than whites to change residences (when income and home ownership are controlled), less likely to convert neighborhood dissatisfaction into a move, and more constrained than whites by the level of residential segregation in the metropolitan area (South and Deane 1993). Blacks are believed to be less able than whites to escape poor neighborhoods, and blacks who move out of nonpoor areas are more likely than their white counterparts to move to poor rather than to other nonpoor neighborhoods (Gramlich et al. 1992; Massey et al. 1994; Nelson and Edwards 1993). Among movers from cities to suburbs, twice as many blacks as whites relocate in low-income neighborhoods (Spain and Long 1981). These discrepancies in the distributional patterns of blacks and whites have led to the development of an alternative theoretical perspective on urban locational attainment—the *place stratification model* (Logan and Molotch 1987). This model supplements the human capital/life-cycle and spatial assimilation perspectives by drawing attention to the barriers to residential mobility faced by black residents, especially in the form of housing discrimination (Farley and Allen 1987; Foley 1973; Galster and Keeney 1988; Massey and Denton 1993). The discriminatory practices of real estate agents (Pearce 1979; Yinger 1995), local governments (Shlay and Rossi 1981), and mortgage lenders (Leahy 1985; Shlay 1988; Squires and Kim 1995) create a racially segmented housing market that obstructs the mobility aspirations of African-Americans, especially for those wishing to move to racially integrated and/or middle-class neighborhoods. White stereotyping of black residents may also impede blacks' residential mobility (Farley et al. 1994). The place stratification model does not deny that human capital and life-cycle factors are important instigators of residential mobility but rather emphasizes the structural constraints under which these mechanisms operate for certain racial groups—blacks, in particular.

Applying the place stratification model to patterns of residential mobility between poor and nonpoor neighborhoods generates several hypotheses. First, in contrast to the human capital/life-cycle model described above, this model implies that, even net of socioeconomic differences, blacks will be significantly less likely than whites to escape poor areas and more likely than whites to move into them. Second, the stratification model suggests that a contextual variable—the degree of housing segregation in the metropolitan area—will impede black mobility out of poor neighborhoods (South and Deane 1993; Massey et al. 1994). High levels

of racial residential segregation are considered by many to be indicative of severe housing discrimination against blacks that limits their housing choices; as such, we hypothesize that the level of segregation in the metropolitan area is inversely related to the probability that black inhabitants of poor neighborhoods will move into nonpoor areas but positively associated with the probability of moving in the opposite direction (i.e., from nonpoor to poor neighborhoods). Conversely, by constraining blacks to relatively few neighborhoods, racial residential segregation may open up more neighborhoods to which whites may desire to move (South and Deane 1993). Whites prefer not to move into areas that are racially mixed or predominantly minority (Clark 1992; Farley and Frey 1994). Because racial segregation is likely to result in a substantial number of predominantly white—and thus racially attractive—neighborhoods for white movers, segregation should facilitate white residential mobility. Hence, we hypothesize that racial segregation is positively associated with the probability that white residents of poor neighborhoods will leave them for wealthier areas.

Finally, the place stratification model implies that the impact of human capital characteristics on the likelihood of leaving poor neighborhoods will vary by race, although the direction of that difference is subject to alternative interpretations. In what Logan and Alba (1993, pp. 244–45) call the “strong version” of the place stratification model, the effects of individual economic resources on locational attainments are weaker for minority groups than for majority groups; housing discrimination reduces the locational “return” minority-group members receive for their human capital, forcing even comparatively advantaged minority-group members to reside in impoverished or otherwise undesirable neighborhoods (Massey 1990). Support for this version of the model is found in the lower levels of social class segregation among blacks than among whites (Massey and Denton 1993). This strong version of the stratification model implies that the effects of human capital factors on out-migration from impoverished neighborhoods will be weaker for blacks than for whites and that, compared to whites, greater human capital endowments are required for blacks to remain in nonpoor neighborhoods. Logan and Alba (1993) also posit a “weak version” of the place stratification model in which valued locational resources “cost” less for minority-group members than for comparable members of the majority group but in which the locational attainments of even the most successful minority-group members barely exceeds that of the least successful members of the majority. This version of the model therefore implies that the influence of human capital factors on the probability of leaving impoverished neighborhoods will be greater for blacks than for whites.

## Other Contextual Effects on Escaping Distressed Neighborhoods

Both the human capital/life-cycle model and, to a slightly lesser extent, the place stratification model emphasize the characteristics of individuals and families that impede or facilitate residential mobility. Although the place stratification model implies that the level of housing discrimination in the local area inhibits the residential mobility of African-Americans, neither model stresses the importance of community or metropolitan area features as determinants of residential mobility. Indeed, only recently have studies of locational attainment (Alba and Logan 1991; Logan and Alba 1993), housing turnover (Rosenbaum 1992), and local residential mobility (Lee et al. 1994; South and Deane 1993) begun to consider seriously qualities of the larger social and economic context as determinants of microlevel spatial distribution processes. Such an omission is particularly striking in light of the considerable variation across metropolitan areas in overall rates of residential mobility (Long 1988) and, it appears, in the ability of residents of distressed areas to leave them for preferable localities (Nelson and Edwards 1993).

Studies of local residential mobility recognize two sources of contextual influence: characteristics of the neighborhood of origin that may propel (or retain) potential movers (Lee et al. 1994) and features of the metropolitan area that may attract (or repel) potential movers from their current neighborhood (South and Deane 1993). Several of these contextual factors are likely to impact differentially the likelihood that blacks and whites will move between poor and nonpoor neighborhoods.

At the neighborhood level, both an area's level of poverty and racial composition are likely to affect out-migration. Residents of the very poorest neighborhoods have farther to travel (in an economic, as well as a spatial, sense) in order to move to a nonpoor neighborhood. For residents of the poorest neighborhoods, moving into a nonpoor area requires an even greater upgrading in neighborhood quality. Although the neighborhood poverty rate (among neighborhoods classified as poor) is not apt to influence the probability of moving *per se*, it is likely to reduce the probability that out-movers will relocate to a nonpoor district rather than another poor district. By the same logic, among residents of nonpoor areas, the neighborhood poverty rate should increase the risk of moving into a poor area.

The racial composition of neighborhoods might also influence the prospect of moving between poor and nonpoor areas, but this effect is likely to differ for whites and blacks. A large minority population is likely to promote the out-migration of the white population. According to classical ecological models of invasion and succession (Frey 1979; Hawley 1950),

whites will attempt to avoid areas with large and growing minority concentrations. Moreover, these residential moves by whites out of racially mixed or predominantly minority areas for predominantly (or exclusively) white neighborhoods are also apt to be moves into nonpoor neighborhoods, given the strong association between neighborhoods' socioeconomic status and their racial composition.

The effect of neighborhood racial composition on the movement of blacks out of impoverished areas is more difficult to anticipate. More so than whites, blacks express a verbal preference for racially integrated neighborhoods (although relatively few black movers actually realize those preferences; see Clark 1992). On the one hand, then, blacks initially residing in largely black (and poor) neighborhoods may be more likely than blacks residing in racially mixed or largely white areas (but also poor) to move out of the neighborhood. To the extent that black movers are able to fulfill their preferences for racially mixed neighborhoods, they would also be more likely to choose nonpoor areas. On the other hand, blacks who reside in predominantly black neighborhoods may live there precisely because they prefer same-race neighbors (Clark 1992); if so, they might be equally (or less) likely to leave their neighborhood than black residents of more integrated areas. Moreover, given racial homogeneity in kin and friendship networks, blacks in predominantly black neighborhoods may experience more extensive social ties to the neighborhood of origin and, therefore, be less likely to move (Madigan and Hogan 1991). In addition, the fusion of high poverty and racial isolation may create "concentration effects" that immobilize minority residents of underclass areas (Wilson 1987).

In addition to neighborhood-of-origin effects, characteristics of the metropolitan area are also likely to influence the probability of moving between poor and nonpoor neighborhoods. As noted above, the place stratification model suggests that housing segregation and discrimination in the metropolis impairs mobility opportunities for blacks. But other properties of metropolitan areas might also inhibit (or expedite) movement out of or into poor neighborhoods, for whites as well as blacks. Because these properties generally reflect the supply of housing available to persons who might desire to leave poor neighborhoods, we refer to this perspective as the *housing availability model*. Perhaps most important, residents of poor neighborhoods will be apt to leave them for wealthier areas when opportunities exist to obtain low-cost housing in nonpoor areas. By this reasoning, the degree to which neighborhoods in the metropolitan area contain a mixture of both poor and nonpoor residents should positively influence the probability of moving from a poor to a nonpoor area. Put another way, we hypothesize a positive association between the extent of residential segregation by social class (specifically, poor vs. nonpoor) in the metropoli-



tan area and the likelihood of leaving an impoverished neighborhood. Opportunities to seek housing in nonpoor areas should also be enhanced by the sheer availability of housing in the metropolis, as indicated by high vacancy rates and new housing construction. The supply of new housing may be particularly important for explaining the mobility patterns of African-Americans because recent housing has operated under antidiscrimination legislation (Farley and Frey 1994). Farley and Frey (1994) show that, across metropolitan areas, new housing construction is inversely related to both levels of and changes in racial residential segregation. This finding suggests that, in rapidly growing urban areas, blacks can more readily find housing in racially mixed (and, presumably, higher-income) neighborhoods, thereby increasing the likelihood of moving from poor into nonpoor areas and reducing the likelihood of moving from nonpoor areas into poor ones. Because discrimination against minorities is believed to increase along with the size of the minority group (Blalock 1967; Lieberman 1980), it also seems likely that the ability of blacks to move into nonpoor neighborhoods will be lower in metropolitan areas having relatively large black populations. Minority group size is likely to exacerbate the racial structuring of the housing market (Stearns and Logan 1986).

Finally, regional differences in the ecological structure and local governmental configuration of metropolitan areas should also influence patterns of residential mobility out of poor areas. Suburban communities in the older metropolitan areas of the Northeast and Midwest regions were historically quick to incorporate, often employing restrictive covenants, land use regulations, and zoning ordinances to impede the in-migration of poor and, especially, black residents. Consequently, people who live in poor neighborhoods in the metropolitan areas of these regions face comparatively restricted housing options. In contrast, political and historical conditions enabled cities in the South and newer cities in the West to annex outlying areas (Farley and Frey 1994), resulting in fewer and smaller suburban enclaves and a correspondingly larger central city population as a proportion of the entire metropolitan population. This relative paucity of economically and racially restrictive suburban communities in metropolitan areas in the South and West, we reason, should expand the housing options for residents of their poor neighborhoods and, hence, should raise the probability that they will leave impoverished neighborhoods for more advantaged areas.

#### DATA AND METHODS

The Panel Study of Income Dynamics (PSID) is a nationally representative, longitudinal survey of noninstitutionalized U.S. residents and their families (Hill 1992; PSID 1987). Beginning in 1968 with approximately

respondents are also younger than their white counterparts and have more children in the household, fewer years of school completed, and lower incomes. Even comparing *within* poor and nonpoor tracts, these blacks and whites reside in different types of neighborhoods; blacks live in tracts with higher poverty rates and much larger concentrations of blacks. Among those in poor tracts, the typical black respondent's neighborhood has a population that is 84% black; the corresponding figure for whites in poor tracts is 24%. Substantial differences in tract racial composition are also observed for blacks and whites residing in nonpoor tracts.

Differences in the characteristics of the metropolitan areas inhabited by the blacks and whites in the study are comparatively modest, but, compared to whites, blacks tend to reside in areas having larger black populations and higher levels of residential segregation by poverty status and by race. The black respondents are also more concentrated in metropolitan areas in the South. Also apparent is the much higher levels of residential segregation by race than by poverty status in U.S. metropolitan areas. How these life-cycle, human capital, and geographic variables influence residential mobility between poor and nonpoor neighborhoods and the degree to which racial differences in these variables account for racial differences in mobility are the questions we now address.

Table 4 presents the results of several logistic regression equations examining the determinants of residential mobility out of poor census tracts. In the first two columns are the bivariate logistic coefficients and their standard errors for equations in which the dependent variable contrasts persons who did not leave a poor tract (including those who either remained in the poor tract of origin or moved to another poor tract) with persons who moved to a nonpoor tract.<sup>12</sup> At the bivariate level, hypotheses derived from all three of the theoretical models receive considerable support. Consistent with the life-cycle model of mobility, number of children and home ownership are inversely associated with the (log) odds of leaving a poor neighborhood for a nonpoor area, while marrying increases that risk. Mobility declines with age but at a decreasing rate. Women who begin or end the mobility interval as heads of household are less likely than men to move from a poor to a nonpoor tract. In addition—consistent with the human capital model of mobility—education, income, and employment as well as increases in income and becoming employed all increase the risk of moving to a nonpoor tract, while receiving public assistance reduces that probability. The coefficient for race implies that the

<sup>12</sup> Because person-year observations for the same respondent are not independent, the standard errors of the coefficients may not be accurate (Bye and Riley 1989). For these analyses, however, adjusting the standard errors for nonindependence of observations had no appreciable effect on our substantive conclusions.

odds of blacks moving from a poor to a nonpoor tract are only 36% ( $e^{-1.023} = .36$ ) of the corresponding odds for whites. This difference, which was also apparent in table 1, is consistent with the place stratification model of residential mobility.

Both of the characteristics of the tract of origin—the percentage that is poor and the percentage that is black—significantly reduce the likelihood of moving from a poor to a nonpoor tract. Persons residing in the poorest of the poor tracts and those in tracts with substantial concentrations of blacks are least likely to move to nonpoor neighborhoods.

Characteristics of the metropolitan area are also significantly related at the bivariate level to the probability of moving from a poor to a nonpoor neighborhood. Consistent with the housing availability model, high levels of residential segregation by both poverty status and by race appear to impede mobility into nonpoor tracts. As suggested above, high levels of segregation are likely to indicate a shortage of dwellings in nonpoor areas that are available to residents of poor neighborhoods. The percentage of the metropolitan area's housing stock that was built recently is positively related to the probability of moving to a nonpoor tract, while the percentage of the area's population that is black is inversely associated with this type of mobility. Finally, compared to residents of metropolitan areas in the West, residents of the other three regions are less likely to move from poor to nonpoor neighborhoods, presumably a function of regional differences in ecological and political structures.

The next two columns of table 4 present the coefficients and standard errors for the multiple logistic regression equation. In contrast to the bivariate effects, the partial regression coefficients suggest a more guarded assessment of the hypotheses. Many of the significant bivariate coefficients become nonsignificant in the multivariate model. Of the life-cycle variables, the effects of age and home ownership remain significant, as does the dummy variable for those who marry during the mobility interval. Of the human capital variables, years of school completed, income, and receipt of public assistance remain statistically significant. Controlling for the other independent variables reduces, but by no means eliminates, the effect of race on the probability of escaping poor tracts; net of other variables in the model, the odds of blacks moving to a nonpoor tract increase to 66% ( $e^{-.418} = .66$ ) of the odds for whites. The reduction in the effect of race when other variables are controlled is consistent with the human capital/life-cycle model, but the significant and substantial difference that remains supports the place stratification perspective.

The most pronounced difference between the bivariate and multivariate equations, however, is for the metropolitan area independent variables. In the multivariate model, only the coefficient for residence in the Northeast remains significant. While some of the reduction in the strength

TABLE 4  
LOGISTIC COEFFICIENTS FOR THE REGRESSION OF RESIDENTIAL MOBILITY OUT OF POOR CENSUS TRACTS, 1979-85

INDEPENDENT VARIABLE	MULTINOMIAL REGRESSION												
	BINARY REGRESSION				Move to Poor Tract vs. Remain in Poor Tract			Move to Nonpoor Tract vs. Remain in Poor Tract			Move to Nonpoor Tract vs. Move to Poor Tract		
	Bivariate		Multivariate		b	SE	b	SE	b	SE	b	SE	
	b	SE	b	SE									b
Individual-level characteristics													
Race (1 = black)	-1.023***	.094	-418***	.151	.674***	.180	-.337**	.153	-1.011***	.218			
Sex (1 = female)	-.181**	.018	.089	.114	.181*	.098	.134	.116	-.048	.139			
Age	-.094***	.004	-.086***	.020	-.134***	.014	-.112***	.020	.022	.023			
Age <sup>2</sup>	.001**	.000	.001**	.000	.001***	.000	.001***	.000	-.000	.000			
Marital status (1 = married)	-.127	.086	-.050	.126	.022	.108	-.051	.127	-.073	.154			
Became married	1.163***	.144	.629***	.160	.094	.168	.644***	.164	.550***	.208			
No. of children	-.053*	.030	.019	.044	-.050	.033	.007	.045	.057	.052			
Homeowner	-.865***	.116	-.710***	.131	-.837***	.123	-.766***	.131	.071	.173			
Persons per room	-.051	.098	-.145	.137	.363***	.090	-.045	.140	-.408***	.153			
Years of school completed	.215***	.017	.092***	.022	-.012	.019	.090***	.023	.102***	.028			
Income (\$1,000s)	.019***	.004	.013*	.007	-.034***	.009	.011	.007	.045***	.011			
Change in income (\$1,000s)	.026***	.008	-.003	.009	.014	.009	-.003	.009	-.016	.012			

Currently employed	.498***	.085	-.012	.119	.012	.105	-.039	.120	-.051	.148
Became employed	.580***	.119	.187	.142	-.088	.132	.185	.144	.274	.179
Receiving public assistance	-.424***	.132	-.299**	.151	-.107	.103	-.306**	.153	-.200	.172
Tract-level characteristics:										
%poor	-.032***	.004	-.015***	.005	.000	.003	-.015***	.005	-.015***	.005
%black	-.014***	.001	-.007***	.002	-.001	.002	-.007***	.002	-.006**	.003
MSA-level characteristics:										
Residential segregation by poverty status ( <i>D</i> )	-.051***	.007	-.020	.013	.040***	.013	-.014	.013	-.054***	.017
Residential segregation by race ( <i>D</i> )	-.019***	.004	.008	.007	-.004	.007	.008	.007	.012	.009
%housing units vacant	.012	.019	.009	.030	-.004	.030	.010	.031	.013	.040
%new housing	.014***	.004	.009	.009	.015*	.009	.011	.009	-.004	.012
%black	-.036***	.005	-.007	.007	-.002	.006	-.007	.007	-.005	.009
Region: <sup>a</sup>										
Northeast	-.932***	.165	-.510**	.208	.173	.197	-.494**	.210	-.667**	.268
Midwest	-.603***	.127	-.130	.173	.166	.178	-.108	.176	-.275	.231
South	-.678***	.114	-.138	.183	-.119	.193	-.165	.185	-.046	.247
Constant			.312	.702	-.588	.640	.770	.708	1.358	.889
$\chi^2$			574				1,295			
df			25				50			

<sup>a</sup> West serves as the reference category.

\*  $P < .10$  (two-tailed test).

\*\*  $P < .05$  (two-tailed test).

\*\*\*  $P < .01$  (two-tailed test).

and significance of these effects may be due to spuriousness or the mediating effects of other independent variables, we suspect that our ability to estimate these net effects is hampered somewhat by the very high correlations among the metropolitan area variables. As examples, the zero-order correlation between the two measures of residential segregation is .71, the correlation between the vacancy rate and new housing construction is .66, and the multiple correlation between the percentage of new housing and the regional dummies is .73. A problem of multicollinearity is also suggested by sharp increases in the standard errors of some of the coefficients (doubling in the cases of residential segregation by poverty status and the percentage of new housing) and the sign reversal for the coefficient for residential segregation by race.

Although our inferences regarding the net effects of these variables must therefore remain tentative, the potential for multicollinearity led us to explore different model specifications by omitting various explanatory variables having high correlations with the other variables. Three findings were worth noting. First, omitting either one of the segregation indices has little effect on the coefficient for the remaining index, implying that the high correlation between these two variables is not a cause of the sharp reduction from their bivariate effects.<sup>13</sup> Second, omitting the regional dummy variables causes the coefficient for new housing construction to become positive and significant. Finally, the diminution in the significance of the metropolitan area variables is, with one exception, *not* a result of their covariation with the individual-level or tract-level independent variables. The coefficients for each of the metropolitan area variables remain significant (in the case of the vacancy rate, *becomes* significant) when the individual and tract variables are controlled. The exception is residential segregation by race, which drops to nonsignificance as a function of its correlations with respondent's race and the percentage of the tract population that is black.

The remainder of table 4 presents the coefficients from a multinomial logistic regression in which persons who did not move to a nonpoor tract are subdivided into those who remained in the (poor) tract of origin and those who moved to another poor tract. This analysis elaborates the binary logistic regressions by isolating the effect of the explanatory variables on residential mobility per se (as reflected in the contrasts between remaining in a poor tract vs. moving to a different poor tract and between remaining in a poor tract vs. moving to a nonpoor tract) and the destina-

<sup>13</sup> In the multivariate regressions examining mobility out of underclass tracts, the coefficient for segregation by poverty status is negative and significant at the .05 level. Moreover, the coefficients for both of the segregation variables are negative and significant at the .01 level when the other segregation index is omitted from the equation.

tion of movers (as reflected in the contrast between moving to a poor tract vs. moving to a nonpoor tract). Several findings from this analysis are worth highlighting. First, as suggested by table 1, blacks differ significantly from whites on all three contrasts: they are more likely than whites to move to another poor tract than to remain in the (poor) tract of origin; they are less likely than whites to move to a nonpoor tract than to remain in the tract of origin; and, conditional upon moving, they are less likely than whites to move to a nonpoor tract than to another poor tract. Second, age and home ownership decrease the rate of moving from a poor to a nonpoor tract because they decrease the overall risk of moving. Both variables significantly distinguish nonmovers from those who move into poor and nonpoor tracts, but neither variable significantly differentiates those who move to a poor tract from those who move to a nonpoor tract. Contrary to expectations, mobile home owners and households with children are no more likely than renters and childless households to select a nonpoor than a poor neighborhood. Third, although in the multivariate binary logistic regressions household crowding is not significantly related to the risk of moving from a poor to a nonpoor tract, crowding significantly increases the risk of moving to another poor tract (relative to not moving), and, conditional upon moving, significantly decreases the rate of moving to a nonpoor rather than to a poor tract. Fourth, relative to their low segregation counterparts, respondents in metropolitan areas characterized by high levels of residential segregation by poverty status are significantly more likely to move to a nonpoor tract than to remain in the tract of origin. In addition, among movers, residential segregation by poverty significantly decreases the likelihood of moving to a nonpoor than to a poor tract. Hence, the lack of available, low-cost housing in nonpoor areas does not appear to inhibit residential mobility among those residing in poor tracts, but it does reduce the likelihood of choosing a nonpoor neighborhood as a destination. Fifth, originating in a very high poverty or predominantly black tract does not influence the likelihood of moving to another poor tract (relative to not moving), but these tract-of-origin characteristics significantly reduce the probability of moving to a nonpoor tract, relative to both remaining in the origin tract and moving to another poor tract.

Table 5 presents the results of parallel binary and multinomial logistic regression equations for respondents who begin the mobility interval in a nonpoor tract. The binary regressions contrast respondents who either remain in or move to another nonpoor tract with those who move to a poor tract. At the bivariate level, almost all of the individual-level independent variables exhibit significant associations with the risk of moving from a nonpoor to a poor tract. Blacks, female household heads, older persons, those in crowded households and households with children, respondents who recently became unmarried or nonemployed, and those receiving





Currently employed .....	.091	.228	.147	-.070	.095	.213	.149	.283*	.162
Became nonemployed .....	.131	.594***	.178	.167	.127	.648***	.183	.480***	.199
Receiving public assistance .....	.152	.017	.181	-.171	.156	-.030	.185	.141	.217
Tract-level characteristics:									
% poor .....	.008	.017	.011	-.022***	.006	.010	.011	.033***	.012
% black .....	.001	.002	.002	-.001	.002	.002	.002	.003	.002
MSA-level characteristics:									
Residential segregation by poverty status ( <i>D</i> ) .....	.006	-.013	.013	.005	.006	-.011	.013	-.016	.014
Residential segregation by race ( <i>D</i> ) .....	.004	.004	.008	.011***	.004	.007	.008	-.004	.008
% housing units vacant .....	.015	.037**	.028	-.022	.016	.052*	.028	.073**	.030
% new housing .....	.004	-.021**	.009	.020***	.005	-.015*	.009	-.035***	.010
% black .....	.005	-.009	.009	-.007	.005	-.011	.009	-.004	.010
Region: <sup>a</sup>									
Northeast .....	.166	-.184	.208	-.380***	.107	-.301	.210	.080	.222
Midwest .....	.114	.070	.177	-.196**	.089	-.003	.179	.193	.188
South .....	.119	.149	.185	-.196**	.096	.076	.187	.272	.197
Constant .....		-.210	.710	1.100***	.388	.826	.716	-.274	.763
$\chi^2$ .....		1,145				3,355			
<i>df</i> .....		25				50			

<sup>a</sup> West serves as the reference category

\*  $P < .10$  (two-tailed test)

\*\*  $P < .05$  (two-tailed test)

\*\*\*  $P < .01$  (two-tailed test)

public assistance are more likely than others to move to a poor tract. In addition, homeowners, married persons, the employed, and persons with higher levels of education and income are significantly less likely to move in this direction. Thus, consistent with the human capital model of mobility, higher SES individuals are both more likely to move from poor to nonpoor neighborhoods and less likely to move from nonpoor to poor neighborhoods. Respondents who begin the interval in tracts with comparatively high poverty rates and large black populations are more likely to move to a poor tract.

The risk of moving from a nonpoor to a poor neighborhood also varies significantly with several of the metropolitan area characteristics. High levels of residential segregation by both poverty status and race, which were inversely related to the probability of escaping poor tracts, are significantly and positively associated with the likelihood of moving into poor tracts. This risk is also higher in MSAs with high vacancy rates and large black populations and, compared to western respondents, lower for northeastern but higher for southern residents.

The multivariate regressions tell a generally similar story, although again several of the bivariate effects become nonsignificant. Of the individual-level variables, the coefficients for number of children in the household, persons per room, current employment status, and public assistance receipt drop to nonsignificance. Once other variables are controlled, female household heads are significantly less likely than their male counterparts to move from a nonpoor to a poor tract. As with movement out of poor tracts, controlling for the other explanatory variables reduces but does not eliminate the racial difference in the odds of moving from nonpoor to poor tracts. Absent controls, the odds of blacks moving from nonpoor to poor tracts are 8.5 times the odds for whites ( $e^{2.142} = 8.5$ ); with controls this ratio drops to 4.2 ( $e^{1.436} = 4.2$ ).

In contrast to their bivariate associations, neither the poverty rate nor the racial composition of the origin tract exhibits significant effects in the multivariate regression. In addition, of the metropolitan-level variables, only the vacancy rate and new housing construction exhibit significant effects.<sup>14</sup> While the lower rate of mobility from nonpoor to poor tracts in areas having substantial new housing stock is consistent with the housing availability model, the positive effect of the vacancy rate runs counter to our hypothesis. One possible explanation for this effect is that urban disinvestment and the depopulation of inner-city neighborhoods increases their vacancy rates, thus driving down the cost of housing in relatively

<sup>14</sup> In the regressions examining mobility from nonunderclass to underclass tracts, the coefficient for segregation by race is also positive and statistically significant at the .05 level.

poor tracts and increasing the incentive for moving from nonpoor into poor neighborhoods. Such a scenario is broadly consistent with observed patterns of gentrification in U.S. cities (Nelson 1988). Further research that identifies the precise location of vacant housing within metropolitan areas may help to establish the validity of this explanation.

Several results from the multinomial regression analyses of mobility out of nonpoor tracts are worth noting. As in table 4, blacks are significantly different from whites on all three contrasts, while age and home ownership significantly decrease the overall risk of moving but not the poverty status of the destination tract among those who move. Household crowding increases the risk of moving to a different nonpoor tract relative to not moving but does not affect the risk of moving to a poor tract. Consistent with the life-cycle model, becoming divorced or widowed increases the overall likelihood of moving and, conditional upon moving, also increases the risk of moving to a poor rather than to a nonpoor neighborhood. Becoming nonemployed, in contrast, does not influence the probability of moving to a different nonpoor tract but does significantly increase the risk of moving to a poor tract. Finally, consistent with regional differences in residential mobility (Gober 1993), mobility into other nonpoor tracts is significantly higher in the West than in other regions.

The equations in table 6 disaggregate by race of respondent the multivariate binary logistic regressions in tables 4 and 5.<sup>15</sup> We show here the coefficients for the race-specific equations, as well as the difference between those coefficients.<sup>16</sup> Of particular importance for the theories under consideration are racial differences in the effects of human capital factors, residential segregation by race, and the racial composition of the tract of origin and the metropolitan area. Consistent with the "weak version" of the place stratification model, years of school completed has a significantly more positive effect on the probability of moving from a poor to a nonpoor tract for blacks than for whites. This differential effect suggests that, compared to whites, it "costs" blacks less in years of education to escape poor neighborhoods. Yet, even at comparatively high levels of education, blacks are less likely than whites to leave poor neighborhoods. For example, using the race-specific coefficients from table 6 and assuming the race-specific means for the other explanatory variables, the predicted probability that a black respondent with 16 years of education will move from a poor to a nonpoor tract is .086; the probability for a white with 16 years

<sup>15</sup> Coefficients for race-specific, bivariate logistic equations and for race-specific multinomial logistic regressions are available from the authors upon request.

<sup>16</sup> For both poor and nonpoor origin groups, the improvement to chi-square from adding all race-by-independent variable interactions to the equations containing main effects only (tables 4 and 5) is significant at the .05 level.

TABLE 6

LOGISTIC COEFFICIENTS FOR THE REGRESSION OF RESIDENTIAL MOBILITY BETWEEN POOR AND NONPOOR CENSUS TRACTS, BY RACE, 1979-85

INDEPENDENT VARIABLE	MOVE FROM POOR TO NONPOOR TRACT				MOVE FROM NONPOOR TO POOR TRACT			
	Blacks		Whites		Blacks		Whites	
	b	SE	b	SE	b	SE	b	SE
Individual-level characteristics								
Sex (1 = female)	.065	.132	.026	.241	.039	.145	-.326*	.210
Age	-.085***	.025	-.081***	.039	-.004	.027	-.073***	.036
Age <sup>2</sup>	.001*	.000	.000	.000	.001	.000	.000	.000
Marital status (1 = married)	-.194	.148	.297	.255	-.491*	.180	-.579***	.231
Became married	.622***	.185	.652**	.327	-.030			
Became unmarried								
No. of children	.065*	.048	-.333***	.118	.418***	.1785***	1.069***	.289
Homeowner	-.609***	.153	-1.012***	.252	.403	.022	.116	.093
Persons per room	-.140	.153	-.255	.340	.115	-.946***	-.816***	.208
Years of school completed	.123***	.029	.011	.039	.112**	.058	.166	.282
Income (\$1,000s)	.017*	.009	.016	.013	.001	-.080***	-.018	.042
Change in income (\$1,000s)	-.003	.011	-.012	.018	.009	-.025**	-.067***	.014
Currently employed	.023	.137	-.314	.260	.337	.015	.009	.015
Became employed	.180	.164	.280	.303	-.100	.299*	.177	.267
								.370

Became nonemployed	-.267	.164	-.408	.431	.141	.760***	.213	.149	.336	.611*
Receiving public assistance						.159	.201	-.228	.461	.387
Tract-level characteristics										
% poor	-.022***	.005	.024**	.012	-.046***	.010	.014	.023	.019	-.013
% black	-.004*	.002	-.011***	.004	.007	.002	.002	.003	.010	-.001
MSA-level characteristics										
Residential segregation by poverty status ( <i>D</i> )	-.025	.017	-.005	.021	.020	-.032*	.020	-.007	.019	-.025*
Residential segregation by race ( <i>D</i> )	-.005	.010	.025**	.012	-.030***	.015	.011	-.002	.011	.017
% housing units vacant	-.004	.038	.038	.058	-.042	.032	.037	.106***	.042	-.074
% new housing	.009	.011	.017	.018	-.008	-.022*	.012	-.025*	.015	.003
% black	-.016*	.009	.015	.017	-.031*	.004	.012	-.019	.017	.023
Region: <sup>a</sup>										
Northeast	-.290	.266	-.909**	.382	.619	.348	.285	-.599*	.335	.947**
Midwest	.092	.208	-.801**	.337	.893***	.134	.247	-.005	.273	.139
South	.007	.234	-.846***	.328	.853**	.137	.251	.248	.286	-.111
Constant	.652	.848	-1.041	1.379		1.404	.936	-.224	1.156	
$\chi^2$	349		173			341		277		
<i>df</i>	24		24			24		24		
<i>N</i>	7,105		1,103			3,577		11,437		

<sup>a</sup> West serves as the reference category.

\*  $P < .10$  (two-tailed test)

\*\*  $P < .05$  (two-tailed test)

\*\*\*  $P < .01$  (two-tailed test)

of education is .121. In fact, a black with 16 years of schooling is less likely than a white with only eight years of schooling (predicted  $P = .112$ ) to leave a poor for a nonpoor neighborhood.

As anticipated by the place stratification and housing availability models, the effect of residential segregation by race in the metropolitan area is significantly different for blacks and whites. Among whites, residential segregation enhances the probability of mobility out of poor tracts, while among blacks the coefficient is negative but not significant. These findings imply that racial segregation in the housing market opens up opportunities for whites to move from distressed neighborhoods to less disadvantaged areas.<sup>17</sup>

As expected, the percentage of the metropolitan area's population that is black tends to lower the probability that blacks will escape poor tracts, presumably as a consequence of increased discrimination attendant to increases in minority group size. The effect of MSA %black on white mobility is not significant. We also find no evidence, however, that sizable black populations in the tract of origin raise the probability that whites will move from poor to nonpoor tracts; the coefficient is negative for both whites and blacks, and the difference between them is not significant. Perhaps the inverse association between the percentage of the population that is black and the probability that whites will leave a distressed neighborhood results, in part, from a selection effect; whites with strong preferences for avoiding living near blacks may have already left tracts with large black populations, leaving behind whites with less averse preferences.

Another unanticipated finding in table 6 is the positive effect among whites of the percentage of the tract-of-origin population that is in poverty on the likelihood of moving to nonpoor tracts. Among blacks, those living in less poor tracts are more likely than those in the poorest neighborhoods to move to nonpoor tracts, as we hypothesized. But among whites, the likelihood of escaping a distressed tract is higher for those in the poorest of these poor tracts. Thus, not only do whites experience an overall higher probability than blacks of escaping distressed neighborhoods, this racial differential is greatest for residents of the poorest areas.

Finally, regional differences in the ability to escape poor neighborhoods are significantly more marked among whites than among blacks. Among

<sup>17</sup> Among blacks, the coefficient for segregation by poverty status in the regression predicting mobility from poor to nonpoor tracts becomes negative and statistically significant at the .05 level when segregation by race is omitted. Also among blacks, in the underclass equations that include all of the independent variables, the coefficients for both segregation variables are inverse and significant at the .10 level. The coefficients for both variables become significant at the .05 level when the other is deleted.

whites, residents of metropolitan areas in the West are significantly more likely than others to move from poor to nonpoor areas, a pattern predicted (although imperfectly, given the similarity of the South to the Northeast and Midwest regions) from regional differences in the development of economically restrictive suburban communities. Among blacks, however, regional differences are small. We suspect that this difference reflects, in large measure, regional differences in suburbanization patterns that facilitate the movement of whites, but not blacks, out of poor neighborhoods. In particular, the low levels of black suburbanization during the 1980s in the West relative to other regions (Schneider and Phelan 1993) may have reduced the likelihood that blacks would move to a nonpoor neighborhood and thus otherwise offset a tendency for higher escape rates for blacks in this region.

Several significant racial differences in the effects of the explanatory variables on the risk of moving from nonpoor to poor tracts are also observed in table 6. The inverse effect of income on downward residential mobility is significantly stronger for whites than for blacks, implying that a comparatively higher income is needed for blacks to remain in nonpoor neighborhoods. Relatedly, blacks appear more vulnerable than whites to adverse changes in life circumstance; becoming nonemployed and unmarried is more likely to engender a move from a nonpoor to a poor tract for blacks than for whites. Among blacks, becoming nonemployed more than doubles the odds of moving to a poor neighborhood ( $e^{.760} = 2.14$ ), while marital dissolution increases those odds almost sixfold ( $e^{1.785} = 5.96$ ). Overall, then, blacks' residency in nonpoor neighborhoods appears more tenuous than that of whites.

To further illustrate the effects of the explanatory variables, table 7 shows the estimated annual probabilities of moving between poor and nonpoor tracts, by race, for various values of selected independent variables.<sup>18</sup> These probabilities are derived from the equations in table 6, assuming the race- and origin-specific mean values for all but the indicated variable. Two reasonable values are chosen for the continuous, individual-level independent variables (age, education, and income), and the minimum and maximum values are chosen for the continuous, metropolitan-level independent variables. One stark implication of these simulations is the importance of life-cycle factors—especially age—for these mobility flows. For both blacks and whites, the probability of moving either from or to poor tracts declines precipitously from ages 20 to 60. Home owner-

<sup>18</sup> We focus here on those variables that appear most consequential for the theories under consideration; predicted probabilities for other independent variables or for other values of the selected variables can be computed from the information in prior tables (see, e.g., Liao 1994).

TABLE 7  
ESTIMATED PROBABILITIES OF MOVING BETWEEN POOR AND NONPOOR CENSUS TRACTS

INDEPENDENT VARIABLE	PROBABILITY OF MOVING FROM POOR TO NONPOOR TRACT		PROBABILITY OF MOVING FROM NONPOOR TO POOR TRACT	
	Blacks	Whites	Blacks	Whites
Sex:				
Male .....	.0444	.1147	.0871	.0055
Female .....	.0472	.1174	.0538	.0040
Age:				
20 .....	.2039	.4553	.1999	.0249
60 .....	.0086	.0322	.0598	.0014
Married:				
No .....	.0488	.1022	.1154	.0075
Yes .....	.0405	.1329	.0432	.0042
Became married:				
No .....	.0447	.1118		
Yes .....	.0802	.1947		
Became unmarried:				
No .....			.0651	.0048
Yes .....			.2934	.0139
Homeowner:				
No .....	.0529	.1599	.1017	.0085
Yes .....	.0295	.0647	.0421	.0038
Education:				
8 years .....	.0342	.1118	.0931	.0055
16 years .....	.0865	.1209	.0515	.0048
Income:				
\$5,000 .....	.0446	.1095	.0823	.0131
\$30,000 .....	.0659	.1538	.0462	.0025
Became nonemployed:				
No .....			.0685	.0050
Yes .....			.1359	.0058
Receiving public assistance:				
No .....	.0477	.1192	.0711	.0051
Yes .....	.0369	.0825	.0823	.0041
Residential segregation by poverty status:				
D = 15 .....	.0813	.1267	.1398	.0058
D = 49 .....	.0366	.1084	.0516	.0046
Residential segregation by race:				
D = 33 .....	.0556	.0522	.0421	.0055
D = 89 .....	.0426	.1826	.0910	.0049
% housing units vacant:				
2% .....	.0466	.1001	.0625	.0033
25% .....	.0425	.2097	.1224	.0365
% new housing:				
8% .....	.0394	.0883	.1065	.0078
60% .....	.0611	.1883	.0368	.0022
Region:				
Northeast .....	.0348	.0929	.0880	.0030
Midwest .....	.0502	.1024	.0723	.0055
South .....	.0463	.0984	.0725	.0070
West .....	.0460	.2027	.0638	.0055

NOTE.—Predicted probabilities derived from equations in table 6, assuming race-specific and origin-specific means for all but the indicated variable.



ship and marital status (including changes in marital status) also appear to influence substantially the likelihood of escaping or drifting into poor neighborhoods, although for whites the predicted probability of moving from nonpoor to poor tracts is quite low for all values of the independent variables. Marrying almost doubles the annual probability of moving from a poor to a nonpoor tract, while the dissolution of a marriage more than doubles the probability of moving from a nonpoor to a poor tract. Clearly, future research on the determinants of residential mobility between neighborhoods of varying socioeconomic status will need to attend closely to life-cycle and related factors that engender a change of residence.

Several of the effects of the metropolitan-level variables are also noteworthy. For example, under the assumptions used for these simulations, whites in metropolitan areas with the highest observed level of racial residential segregation ( $D = 89$ ) are over three times as likely as whites in the least segregated areas ( $D = 33$ ) to move from a poor to a nonpoor tract in a given year (.1826 vs. .0522). Whites in the West are about twice as likely as those in other regions to escape poor tracts. And, both blacks and whites are about three times as likely to move from a nonpoor to a poor tract in metropolitan areas that have the lowest, compared to the highest, rate of new housing construction. Thus, consistent with the housing availability model, several characteristics of metropolitan areas at least moderately influence the probability of moving between poor and nonpoor neighborhoods.

## DISCUSSION AND CONCLUSION

Despite the salience of this topic for both sociological theory and social policy, few studies have systematically examined the determinants of residential mobility between poor and nonpoor neighborhoods. We explore this issue here by attaching information on the census tracts of residence to a large, longitudinal, and nationally representative sample of American adults living in metropolitan areas. We derive and test hypotheses from three broad theoretical perspectives on residential mobility—one that emphasizes human capital and life-course determinants of mobility, one that underscores the barriers to mobility faced by racial minorities, and one that stresses the supply of housing in the local metropolitan area that is available to residents of distressed neighborhoods. In general, our analyses reveal at least some support for all three of these theoretical models.

The human capital/life-cycle model of residential mobility stipulates that families and individuals attempt to purchase or rent homes in the most desirable neighborhood available, under current financial constraints, and decide whether and where to move based on the needs and

desires associated with their life-course position. Consistent with this model, most indicators of socioeconomic status and life-course stage evince significant bivariate associations with the likelihood of moving between poor and nonpoor neighborhoods. Of these effects, education, age, home ownership, marriage, and the receipt of public assistance all influence the likelihood of leaving distressed neighborhoods for better environs, net of other influences on residential mobility. Socioeconomic resources also serve to retain those who already reside in nonpoor neighborhoods, while two life events—the disruption of a marriage and the loss of a job—substantially increase the risk of moving from a nonpoor into a poor area.

Yet, our results clearly demonstrate that the likelihood of escaping distressed neighborhoods is not solely a function of these individual attributes. As suggested by the place stratification model, racial differences in mobility are pronounced, with blacks substantially less likely than whites to leave poor areas and substantially more likely to move into them. Moreover, and seemingly contrary to the human capital/life-cycle model, these racial differences persist, although in muted form, even after adjusting for racial differences in socioeconomic and life-course variables. The place stratification model anticipates these net effects of race on the basis of barriers to black mobility in the form of racially segmented and discriminatory housing markets. Consistent with what has been called the “weak version” of this model (Logan and Alba 1993), it “costs” blacks less than whites in educational attainment to leave poor for nonpoor tracts, but even the most educated blacks remain substantially less likely than the least educated whites to escape distressed neighborhoods. Further, downward neighborhood mobility among blacks is more susceptible than that of whites to disruptive life-course events, including marital dissolution and unemployment. That residential segregation between blacks and whites in the local metropolitan area tends to increase the probability that whites will leave distressed neighborhoods and appears to diminish the chance that blacks will leave underclass neighborhoods is also consistent with the place stratification perspective and points to the influence of racially segmented housing markets on residential mobility patterns.

Neither the human capital/life-cycle model nor the place stratification model emphasizes characteristics of local metropolitan areas as determinants of escaping distressed neighborhoods. Our findings relevant to the housing availability model suggest that this neglect may be unwarranted. Although high correlations among the metropolitan-level explanatory variables render our inferences tentative, these results suggest that the likelihood of moving into and out of distressed neighborhoods depends on the supply of housing available in nondistressed areas. Escape rates tend to be higher in metropolitan areas having relatively numerous neigh-

borhoods that contain a mix of poor and nonpoor households and, especially for whites, metropolitan areas in the West. We also find suggestive evidence that residential integration by poverty reduces the risk of moving from poor to nonpoor neighborhoods. Residential integration by poverty status, we argue, raises escape rates by enlarging the supply of housing opportunities in nonpoor neighborhoods that are available to residents of poor areas. We suspect that escape rates are higher in the West because metropolitan areas of this region have comparatively small and few suburban communities that, through land-use regulations and other growth management policies, tend to restrict the in-migration of poor residents. Ample supplies of recently constructed housing help to retain people in nonpoor neighborhoods. Future research on factors affecting the ability to escape or evade distressed neighborhoods might benefit from exploring further other contextual influences on residential mobility.

Beyond these theoretical concerns, our results speak on a general level to several issues in urban policy. One such issue involves how best to facilitate the geographic dispersal of minority residents of low-income and underclass neighborhoods (Hughes 1993; Wilson 1987). Some argue that improving the human capital of underclass families and individuals is the most profitable strategy, since, as Wilson (1987, p. 158) puts it, "social mobility leads to geographic mobility." Others suggest that efforts to combat racial discrimination in the housing market are necessary to expand minority mobility prospects (e.g., Massey and Denton 1993). This view, which provides the impetus for initiatives such as the Gautreaux program (Rosenbaum and Popkin 1991), argues that integrating middle-class neighborhoods by race will improve housing opportunities and enhance residential mobility for underclass residents. A third view also focuses on the housing market but argues for a universalistic rather than a race-based, strategy. This approach implies that integrating neighborhood housing opportunities by social class would best facilitate the movement of minority residents out of distressed neighborhoods. That is, creating a more diverse mixture of low- and middle-income housing units within neighborhoods will, given the intersection of race and class, produce a movement of low-income minorities out of poor areas. Our findings, which admittedly speak somewhat obliquely to these policy concerns, nonetheless suggest that there is merit to all three approaches. High levels of education and income both increase the probability that black residents of poor neighborhoods will leave them for better ones and reduce the probability that they will move from nonpoor to poor areas. At the same time, however, we observe suggestive evidence that black mobility out of distressed (especially underclass) neighborhoods is also facilitated by higher levels of housing integration both by race and by poverty status. Our findings also suggest, however, that integrating neighborhoods by race may

diminish the likelihood that *white* residents of poor neighborhoods will leave them for more advantaged areas.

A second policy issue concerns efforts to empower residents of low-income neighborhoods by increasing their stake in community outcomes. Perhaps the most frequently suggested mechanism for this empowerment involves increasing the rate of home ownership in low-income neighborhoods (Cisneros 1995). While increases in home ownership may very well improve the quality of life and community integration of low-income neighborhoods, our findings reveal that such increases will almost surely have the unintended consequence of reducing the likelihood that residents of these areas will leave them for better neighborhoods. In this sense, policies that aim to disperse minority residents of low-income neighborhoods and policies that attempt to empower them through home ownership may prove to be at cross-purposes.

Finally, our results should serve to encourage further efforts to integrate microlevel research on residential mobility with studies of spatially circumscribed housing opportunities in metropolitan areas. While families and individuals may decide whether and where to move primarily on the basis of their personal characteristics, for blacks as well as whites, the ability to actuate these preferences depends on the distribution of opportunities embedded in the local social structure. Moreover, further research on *microlevel* mobility processes should provide valuable insight into the sources of change in *macrolevel* spatial distributions. In any event, a comprehensive explanation for why some people can—and others cannot—escape distressed neighborhoods will surely require reference both to individual attributes and to the external constraints generated by the social context.

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# Information Gaps and Unintended Outcomes of Social Movements: The 1989 Chinese Student Movement<sup>1</sup>

Fang Deng  
*North Central College*

Under what conditions will threats made by a state hinder social movements? And under what conditions will intended or unintended outcomes occur as a result? This article addresses these questions by applying a dynamic model that depicts the 1989 Chinese student movement as a three-iteration game with incomplete information. In this model, the Chinese government is willing ultimately to suppress the student resistance by force, but since it is playing a two-level game, it conceals its preferences as private information while initially choosing not to use force. In the end, many demonstrators died believing that the People's Liberation Army would never harm the Chinese people. This model suggests how an information gap can lead to unintended and undesirable outcomes, even when actors behave rationally.

The study of social movements has done little to illuminate movement development and outcome. Coleman observed that "social scientists have been less concerned with predicting the outcomes of a revolution or even describing the course it takes than with understanding its emergence" (1990, p. 469). Lichbach argued that "those who have applied CA [collective action] theories to conflict have focused almost exclusively on the initial problem of whether anyone who is rational will actually participate in protest and rebellion. The result of such a preoccupation is, firstly, that almost no CA theorists have gone on to study the many substantive problems arising in revolts and protests" (1994, p. 9). This article focuses on a social movement's development and outcomes. It is based on a case study of the 1989 Chinese student movement.

Recent publications on rationality and rebellious collective actions (Lichbach 1995; Moore 1995; Goldstone and Opp 1994; Marwell and Oli-

<sup>1</sup> I would like to thank the *AJS* reviewers for their valuable comments on a previous draft. Address correspondence to Fang Deng, Department of Sociology, North Central College, P.O. Box 3063, Naperville, Illinois 60566.

ver 1993; Muller, Dietz, and Finkel 1991; Coleman 1990) reflect a growing interest in using rational choice theory to explain protest and rebellion. In considering the development and outcomes of a social movement, rational choice theorists have proposed an explanation that focuses on the relationship between a social movement and the state (Koopmans 1993; McAdam 1982; Perrow 1979; Tilly 1978). According to this explanation, "The state generates many of the issues with which social movements wrestle; as well, the state facilitates or hinders movements, lowering or raising the costs of collective action, operating in coalition with the movement or opposing it" (Zald 1992, p. 339). Morris and Herring contended that "movement participants and their actions are rational; social movements pursue interests; movement mobilization occurs through an infrastructure or power base; outcomes of collective action are central, and they are products of strategic choices made by participants; either support or repression by elite groups can affect the outcomes of movements" (1987, p. 157). The core postulate of this explanation is that the state plays an important role in determining a movement's development and outcomes and that high levels of suppression and limited access to power can force challengers to withdraw from collective action.

There are some puzzling aspects of the 1989 Chinese student movement that challenge the adequacy of this explanation.<sup>1</sup> On April 17, 1989, the Chinese student movement began when about 3,000 university students from Beijing demonstrated against the Chinese government for political reform. When the students requested a meeting with high-level leaders of the Communist Party, the government responded with threats. The students, however, resisted the threats and the government backed down. In 1989, the government threatened the demonstrators three times, but none of the threats influenced the students. Why did the threats fail?

The first threat occurred on April 26, 1989, when an official editorial characterized the student movement as "a planned conspiracy," the essence of which was "to repudiate the leadership of the Chinese Communist Party and to repudiate the socialist institutions." The editorial stated that the movement should be suppressed immediately and harshly. But the threat failed to discourage the students as the number of participants rose from 60,000 on April 24 to 250,000 on April 27. The second threat occurred on May 19, when martial law was declared in parts of Beijing. Thousands of troops with tanks were deployed around the city. This plan failed to return the students to their campuses and to "restore order." For two days, more than a million people protested the enforcement of martial law. The

<sup>1</sup> My analysis is based on newspaper accounts and interview data. All of those interviewed were eyewitnesses of the movement. Quotations in this article without citations appear in the *People's Daily* (from April 15 to June 4, 1989).

third threat occurred on June 3, when the demonstrators were warned that if they persisted, they would "bear responsibility for their own fates." Hundreds of thousands of demonstrators refused to comply and the army opened fire.<sup>3</sup>

Why did these three threats fail to deter the students? If the demonstrators were rational, why did they persist?

Another question is, Why did the military confrontation occur when both sides hoped to avoid it? After the first threat, for example, the government backed down when the demonstrators resisted. When martial law was declared, several high-level leaders of the Communist Party asserted in public speeches that the troops were expected "to restore order, and to protect civilians" and that the leaders intended "to avoid a conflict between the demonstrators and the troops." On May 22, some tanks were withdrawn because the government expected that the demonstrators would back down. After a major victory, most of the demonstrators withdrew from Tiananmen Square by May 27, 1989. On May 31, most of the student leaders decided, contrary to their earlier position, to pressure the government by occupying the square until June 20, when the standing committee of the National People's Congress was expected to convene. Those remaining at Tiananmen Square believed that the longer they stayed, the greater the likelihood that the government would accept their requirements. Why was the outcome so different from that expected by either side?

Sometimes high levels of suppression and limited access to power can force demonstrators to withdraw, and a movement's outcome is as the state intended. At other times the state cannot deter the movement, and unintended outcomes occur. Therefore, any adequate theory of a movement's development and outcomes should specify the conditions under which a state can or cannot hinder movements and the conditions under which intended or unintended outcomes occur. The explanation given by the rational choice theorists (e.g., Koopmans 1993; McAdam 1982; Perrow 1979; Tilly 1978) is inadequate because it suggests that a power imbalance in favor of the state can always force demonstrators to withdraw, thus resulting in intended outcomes for the movement. As an alternative to this theory, this article applies game theory to the study of a movement's development and outcomes.

Blaug suggests that "the introduction of a game-theoretical approach to economics has brought with it a new 'understanding' of what is meant by rationality and interdependence and equilibrium" (1992, p. 240). The

<sup>3</sup> There are no official statistics on the number of people who died in the military crackdown. There are many unofficial estimates, but there is a great diversity among them.

understanding was new because "game theorists seek to determine whether a given game (i.e., pay-off structure) can explain social phenomena" (Moore 1995, p. 423). When a social movement challenges a state, a game describes the connection between the choices of one side and the options and outcomes for the other side. Over the past 20 years, noncooperative game theory has "made serious progress in two crucial areas: dynamics and asymmetric information" (Tirole 1988, p. 205). Dynamics become particularly important in the movement's development in which there are many time periods and intertemporal dependency of action sets. Asymmetric information is also an important issue because some studies have shown that demonstrators sometimes fail to obtain essential information when they choose strategies (Zald 1992). By studying the equilibrium in certain payoff structures, game theory can predict the choices made by the state and demonstrators and the corresponding outcomes of the movement.

This article is based on the following assumptions:

1. "People can be modeled as if they have Von Neumann and Morgenstern utility functions" (Moore 1995, p. 423).
2. "People seek to maximize their expected utility" (Moore 1995, p. 423).
3. There is a single leader of the state who makes decisions on that side, and all of the demonstrators are wholly unified as a single individual who decides his strategies on the other side (Coleman 1990).<sup>4</sup>

<sup>4</sup> The single player assumption is very important here because game theorists are interested in explaining social phenomena by payoff structures. When we discuss the payoff, the player in each side in the game has to be a unitary actor, otherwise, no one will understand whose payoff we are discussing. There is no doubt that almost all unitary actors in game theoretical models are internally divided. But the fragmentation of each side is not an issue in the study of the payoff structure. In other words, the single player and the internal division are two separate issues at different levels of analysis. Is the single player assumption plausible? I will answer this by reflecting on some specific development prior to the 1989 movement. Although the Chinese government was internally divided during the 1989 movement, Deng Xiaoping was the only decision maker. The Central Committee of the Chinese Communist Party made a formal decision in 1987 to grant him this power. Zhao Ziyang, the central secretary of the Chinese Communist Party and a leader of the faction disagreeing with Deng Xiaoping in 1989, failed in his efforts to challenge Deng's authority and resigned on May 19, 1989. The 1989 movement started as a student movement in April, and more than one million people spontaneously participated in May and June. The demonstrators were homogeneous, and all of them struggled for political reform. Although among the students there was internal disagreement, their number was less than 10% of the total number of participants. There was no organizational connection between student and nonstudent demonstrators. Most nonstudent demonstrators at that time never even heard any student leaders' names, and they knew nothing about the fragmentation of the students. The internal disagreement among students had no substantive impact on the strategic choices of 90% of the demonstrators.

4. The payoff structure induces or constrains people's choices in a situation in which "two or more individuals make decisions that will influence one another's welfare" (Myerson 1991, p. 5).

This article presents a dynamic model describing the 1989 movement as a game that is based on incomplete information and repeated three times. The first two sections focus on the first round of the repeated game, the next section on the second round, and the last section on the last round.

#### THREAT AND RESISTANCE: A GAME WITH INCOMPLETE INFORMATION

A basic model describing the 1989 movement can be constructed to reflect the sequence of choices made by each side and the options available to each as the confrontation developed. Although the model is much simpler than the actual course of events, it captures key aspects of the movement's development (see fig. 1).

In the first stage, the student movement began when the demonstrators pressured the government for political reform. In the second stage, after the emergence of the student movement, the Chinese government had various options, but for now it will be convenient to reduce them to three: (1) it could accept the demonstrators' demands and the game would end; (2) it could suppress the movement through might and force the demonstrators to withdraw; (3) it could threaten the demonstrators, if they did not back down, to take some action that would have worse consequences than backing down. The government chose to threaten the demonstrators. In the third stage, the demonstrators responded to the government's threat. They could "resist" or "back down," and they did not back down. In the fourth stage, after the demonstrators resisted the threat, the government had to choose whether or not to act on its initial threat as stated in option 2—that is, to use force. If the government had chosen to use force, the demonstrators would have been forced to withdraw and the game would have ended. But the government decided not to use force, and therefore, the game began again, when the demonstrators took new actions.

The model in figure 1 is a game composed of a four-step sequence. When does it end? Many possibilities have been mentioned above, which means that the game could end at any step or continue endlessly. In 1989, the game was repeated three times because the government used force after the demonstrators resisted for the third time. In game theory, this four-step sequence might be called a "stage game," and a game in which the stage game is repeated many times is called a "repeated game." This section and the next will assess the first round of the repeated game. In the first round, there are two questions: (1) Why did the demonstrators choose

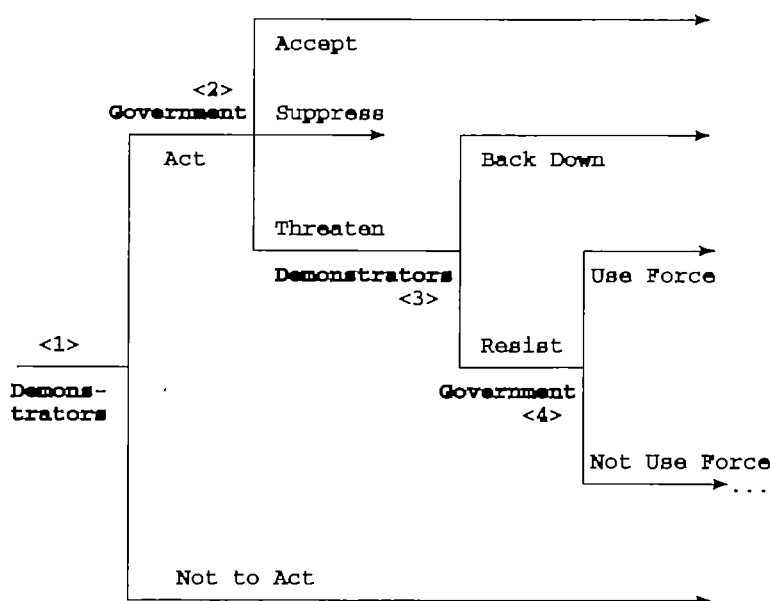


FIG. 1.—A basic model of the 1989 movement

to resist after the government threatened them? (2) Why did the government choose a list of strategies, that is, to threaten and not to use force? This section will focus on the demonstrators' choice, and the next will discuss the government's strategies.

After the government threatened the movement by publishing the official editorial on April 26, the students were angry. They expected to organize a full-scale resistance, but they were also concerned about the possibility of military intervention. Their uncertainty was based on private information held by the government. According to Fearon, "A person who knows something that others do not know about his preferences, intentions, or any other variables is said to have private information" (1992, p. 113). On April 26, 1989, only the government knew its preferences for the use of force. The students did not know. An overwhelming majority of students believed that there was a 50–50 chance that the government would suppress by force. Their beliefs were based on two conflicting facts: (1) the serious warning in the editorial that indicated that the government would use force and (2) the students' knowledge that the Chinese government had always tried to avoid military intervention during any demonstrations and had maintained this policy in all student movements since Liberation in 1949.

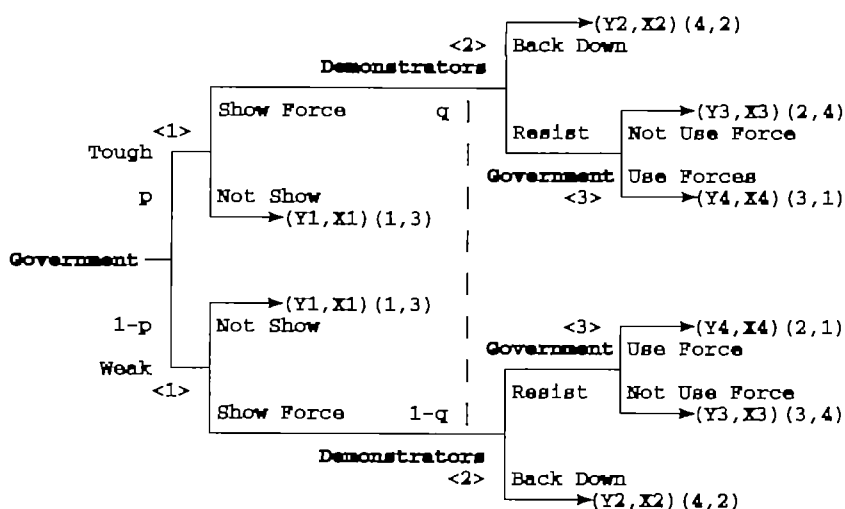


FIG. 2.—A main model of the movement's development

On April 27, the government sent mixed signals before the full-scale demonstration began. It signaled first by showing force. Seven blockade lines were erected between a western suburb of Beijing, where most of the universities are located, and the center of the city at Tiananmen Square. Each blockade line was occupied by hundreds of military police. It seemed that the government would act on its initial threat. But there was another signal. None of the police carried a gun or, strictly speaking, none of them was seen carrying a gun. The mixed signals confirmed the students' beliefs that there was a 50% probability that the government would suppress the movement through the use of force.

Given this uncertainty, why did the students decide to resist? Was their decision rational or impulsive? Were there other factors in their decision? The model in figure 2 addresses these questions.

Unlike the model in figure 1, the new model is a game with incomplete information (i.e., a game in which at least one player is uncertain about another player's payoff). In the new model, the demonstrators are confronted with the element of uncertainty. Another difference between the models in figures 1 and 2 is that the former is a game with a four-step sequence and the latter is a subgame within that sequence. The subgame begins at one of the government's decision nodes at the second step of the four-step sequence. First, the subgame must be analyzed. By definition, the government in figure 2 is tough if it prefers to use force and weak if it does not.

In the first step of the main model (i.e., fig. 2), suppose the government

is tough or that the demonstrators initially believe it is tough with the probability  $p$  and weak with the probability  $1 - p$ . The game is initiated by a choice the government makes between two alternatives: either to show or not to show force after it threatens the demonstrators, that is, to be tough or to be weak. The government recognizes the uncertainty of the demonstrators and, therefore, either the tough or the weak government can take advantage of the situation to further threaten the movement by showing force. In the second step, a dotted line denotes an information set. The demonstrators have the information set with two nodes, which reflects their inability to verify the node where they are located when they must make a decision. In other words, although the demonstrators have observed that the government *showed* force after the threat (seven blockade lines were erected), they do not know if the government intends to *use* force. Suppose the demonstrators are located on the tough side with probability  $q$  and on the weak side with probability  $1 - q$ . The demonstrators on either side have a choice between resisting or backing down. If they back down, the game ends. In the third step, the government must decide whether to use force after the demonstrators choose to resist.

In figure 2,  $X_1$ ,  $X_2$ ,  $X_3$ , and  $X_4$  are the demonstrators' payoffs, and  $Y_1$ ,  $Y_2$ ,  $Y_3$ , and  $Y_4$  are the government's payoffs. The first number in parentheses denotes the government's preferences. The second denotes the demonstrators' preferences. The greater the number, the stronger the preferences of both sides. The payoffs and preferences for each side are summarized in table 1, which reveals the demonstrators' payoffs and preferences as follows:

$X_1 = \sqrt{H}$	$H$ denotes honor to the demonstrators. Usually, resisting the threat brings honor to the demonstrators, and here the demonstrators get smaller $\sqrt{H}$ because they are strong and the government is afraid to show force after the threat.
$X_2 = -F$	$-F$ denotes a general cost for the demonstrators ( $F > 0$ ). The demonstrators will lose face whenever they back down, and this cost can be called a general cost.
$X_3 = H + G$	The demonstrators resist the threat that will bring honor, $H$ , to them; $G$ denotes an expected gain for the demonstrators, that is, their demands will be accepted when the government backs down ( $G > H$ ).
$X_4 = -L + H$	When the government suppresses the student movement, there is an expected loss, $-L$ ( $L > 0$ ).



TABLE 1  
PAYOFFS AND PREFERENCES IN THE MAIN MODEL

STRATEGY	PAYOFF			UTILITY	
	Demonstrations	Government	Demonstrations	Tough Government	Weak Government
Government does not show force . . . . .	$X_1 = \sqrt{H}$	$Y_1 = -B$	$U(X_1) = 3$	$U(Y_1) = 1$	$U(Y_1) = 1$
Government shows force, demonstrators back down	$X_2 = -F$	$Y_2 = B$	$U(X_2) = 2$	$U(Y_2) = 4$	$U(Y_2) = 4$
Government shows force, demonstrators resist, and government does not use force . . . . .	$X_3 = H + G$	$Y_3 = E - C$	$U(X_3) = 4$	$U(Y_3) = 2$	$U(Y_3) = 3$
Government shows force, demonstrators resist, and government uses force . . . . .	$X_4 = -L + H$	$Y_4 = C - E$	$U(X_4) = 1$	$U(Y_4) = 3$	$U(Y_4) = 2$

Because the demonstrators resist the threat first that brings  $H$  to them,  $-L + H < -F$ .

$$\therefore H + G > \sqrt{H} > -F > -L + H,$$

$$\therefore U(X3) > U(X1) > U(X2) > U(X4).$$

In table 1, the government's payoffs and preferences are as follows:

$Y1 = -B$	$-B$ denotes the greatest loss for the government because it backs down immediately after it threatens ( $B > 0$ ).
$Y2 = B$	$B$ denotes the greatest gain for the government when the demonstrators withdraw after the government showed force.
$Y3 = E - C$	$E$ denotes a good record on human rights ( $E > 0$ ); $-C$ denotes a loss of credibility ( $C > 0$ ). The government loses credibility when it fails to act on its initial threat, but its record on human rights will be good.
$Y4 = C - E$	$C$ denotes a gain of credibility; $-E$ denotes a bad record on human rights. The government will get a bad record on human rights whenever it acts on its initial threat.

There is a difference in the preferences between the tough government and the weak one. For the tough government,  $C > E > 0$ , therefore,

$$\therefore B > C - E > E - C > -B,$$

$$\therefore U(Y2) > U(Y4) > U(Y3) > U(Y1).$$

For the weak government,  $E > C > 0$ , therefore,

$$\therefore B > E - C > C - E > -B,$$

$$\therefore U(Y2) > U(Y3) > U(Y4) > U(Y1).$$

As mentioned before, the main model in figure 2 is a dynamic game with incomplete information. A very important methodological innovation in game theory is to invent a new equilibrium concept—perfect Bayesian equilibrium—for this kind of game. Gibbons introduces the new solution concept: "The crucial new feature of this equilibrium concept [perfect Bayesian equilibrium] is due to Kreps and Wilson (1982): beliefs are elevated to the level of importance of strategies in the definition of equilibrium. Formally, an equilibrium no longer consists of just a strategy for each player but now also includes a belief for each player at each information set at which the player has the move" (Gibbons 1992, p. 179). An

appendix shows how to find perfect Bayesian equilibria in the game shown in figure 2.<sup>5</sup> From the perfect Bayesian equilibria,

$$A = \begin{cases} 1, & \text{if } p < p^*, \\ 0, & \text{if } p > p^*, \\ 1/2, & \text{otherwise.} \end{cases} \quad (1)$$

$$p^* = \frac{X3 - X2}{X3 - X4} \quad (X3 > X2 > X4, \quad 1 > p^* > 0), \quad (2)$$

where  $A$  denotes the probability that the demonstrators resist the threat,  $p$  represents the demonstrators' beliefs about the government's preferences,  $p^*$  is a critical condition, and  $X2$ ,  $X3$ , and  $X4$  are the demonstrators' payoffs.

In (1), if the demonstrators' belief,  $p$ , that the government is tough is smaller than the critical condition,  $p^*$ , they will resist. If their beliefs about  $p$  are greater than  $p^*$ , they will back down. As mentioned before, students in the 1989 movement believed that there was a 50-50 chance that the government would suppress their movement by force, which means that, in figure 2, the demonstrators'  $p = 50\%$ . Is  $p$  greater or smaller than  $p^*$ , when  $p = 50\%$ ?

$$p(p = 50\%) \begin{cases} < p^*, & \text{if } (X3 - X2) > (X2 - X4), \\ > p^*, & \text{if } (X3 - X2) < (X2 - X4), \\ = p^*, & \text{otherwise.} \end{cases} \quad (3)$$

In (3),  $p(50\%) < p^*$ , if  $(X3 - X2) > (X2 - X4)$ ; and  $p(50\%) > p^*$ , if  $(X3 - X2) < (X2 - X4)$ . It is obvious that the demonstrators' beliefs, that the probability for the government to suppress by force is less than 50%, is smaller than  $p^*$  when  $p(50\%) < p^*$ .

In (3),  $X3$  is the demonstrators' payoff when they resist and the government does not use force ( $X3 = H + G$ );  $X2$  is their payoff when they back down ( $X2 = -F$ );  $X4$  is their payoff when they resist, and the government uses force ( $X4 = -L + H$ ). Then

$$(X3 - X2) \begin{cases} > (X2 - X4), & \text{if } |G| - |-L| > -2(F + H), \\ < (X2 - X4), & \text{if } |G| - |-L| < -2(F + H), \\ = (X2 - X4), & \text{otherwise.} \end{cases} \quad (4)$$

According to (4), if  $|G| - |-L| > -2(F + H)$ ,  $(X3 - X2) > (X2 - X4)$ , then  $p(50\%) < p^*$ ; if  $|G| - |-L| < -2(F + H)$ ,  $(X3 - X2) < (X2 - X4)$ ,

<sup>5</sup> An appendix, including all proofs of the equations used in this article, is available on request.

then  $p(50\%) > p^*$ . As mentioned before, for the demonstrators  $G$  denotes an expected gain,  $-L$  is an expected loss,  $-F$  denotes a general cost, and  $H$  is honor. Suppose the demonstrators think that the general cost and honor are not very important when compared with the expected gain and loss, which means that  $F + H$  is close to zero. But  $F + H$  is always greater than zero and  $-2(F + H)$  is always smaller than zero. Therefore, the condition for  $(X3 - X2) > (X2 - X4)$  can be relaxed to  $|G| - |-L| \geq 0$ . In other words, the condition for  $p(p = 50\%) < p^*$  in the main model is  $|G| \geq |-L|$ , that is, the expected gain is not smaller than the expected loss for the demonstrators. Here, the expected gain (or loss) is equal to multiplying a substantive gain (or loss) and the probability associated with the success (or failure) of the demonstrators' resistance.

According to the perfect Bayesian equilibrium in the main model, when the government threatens after the emergence of a movement, the demonstrators will resist if they believe that the chance is 50–50 or less that the government will use force and if their expected gain is equal to or greater than their expected loss. In 1989, after the government's first threat, the students realized that their substantive gain (the government would accept their demands for political reform) was not smaller than their substantive loss (the police would attack them). They perceived no difference between the substantive gain (or loss) and the expected gain (or loss) because they believed that the probability associated with their success or failure was 50–50. Therefore, they decided to resist, even though they were uncertain of the government's preferences.

#### SUBOPTIMAL STRATEGIES: A TWO-LEVEL GAME

In the last section, the main theoretical claim is that interaction between the government and the demonstrators in the first round resulted in a payoff structure, in a game with incomplete information. Owing to the effect of the payoff structure, the demonstrators resisted the government's threat regardless of its military strength. A key element in the game with incomplete information is that the government concealed its preference for the use of force as private information. Why did this occur? In 1989, China's leading strategist, Deng Xiaoping, decided that the Communist Party would never relinquish any of its power. This meant that the government would eventually suppress the movement by force. Why did the government fail to send a clear signal to the demonstrators after it threatened them? On April 27, 1989, when the students demonstrated, the military police withdrew and the seven blockade lines broke one by one. After showing force, why did the government choose not to use it?

It is probable that the government was engaged in a two-level game. This concept was first used by Putnam (1988) as a metaphor for domestic-

international interactions. Putnam observed: "The politics of many international negotiations can usefully be conceived as a two-level game. At the national level, domestic groups pursue their interests by pressuring the government to adopt favorable policies, and politicians seek power by constructing coalitions among those groups. At the international level, national governments seek to maximize their own ability to satisfy domestic pressures, while minimizing the adverse consequences of foreign developments. Neither of the two games can be ignored by central decision-makers, so long as their countries remain interdependent, yet sovereign" (Putnam 1988, p. 434).

A two-level game is not just a combination of any two games played by a government simultaneously. One important aspect of a two-level game is that a particular player plays two games with two different opponents at the same time, and the player's options in one game are limited by his strategies in another. In Putnam's two-level game, for example, a national government plays two games against distinct opponents: domestic constituents and foreign trading partners, who have opposing interests.

When the Chinese government confronted the student movement in 1989, it was engaged in several other games. Some of these were not a part of a two-level game because the government's options in these games were not limited by its strategies in the game with the demonstrators. One of these games was to control regional leaders who were ordering units in their province to lend too much money and to start too many projects. The most important economic task in 1989, then, was to restrict the money supply, which created a major constraint on regional leaders and propelled China into a recession. To suppress the students would signal a strong central government, one that could effectively constrain the money supply and inflation.

But in other games not directly related to the student movement, the government had interests in pursuing strategies that would have been damaged by the overt suppression of the students. For example, one of these games was to please the international community in order to attract foreign investors. If we take this game as an example, it is obvious that the Chinese government was playing a two-level game in 1989.<sup>6</sup> At the

<sup>6</sup> There are significant differences between Putnam's two-level game (1988) and the game played by the Chinese government in 1989. For Putnam, there is an interest conflict between domestic constituents and foreign trading partners, and the government is neutral in this conflict. In 1989, the Chinese government pursued opposing strategies in the two games, but there was no interest conflict between the demonstrators and the international community. Putnam explains the consistency between the two games by a concept, "win-sets," and by recognizing a difference between involuntary and voluntary defection. I explain the consistency by recognizing a difference between suboptimal and optimal strategies.

TABLE 2  
THE GOVERNMENT'S DILEMMA

STRATEGY	EQUILIBRIUM	
	Game with Demonstrators	Game with International Community
Threaten and suppress .....	Government wins	Government loses
Do not threaten or suppress .....	Government loses	Government wins
Threaten but do not suppress .....	Government wins if demonstrators back down; loses if demonstrators resist	Government does not lose

national level, it tried to force the student movement to back down in order to keep the core of the Communist government system intact. At the international level, it tried to avoid a poor record on human rights by avoiding military intervention.

According to Putnam (1988), the unusual complexity of a two-level game is that a player's rational moves in one game may be irrational in another game. Due to the unusual complexity of a two-level game, the Chinese government faced a dilemma in 1989 that is outlined in table 2.

If the government is only concerned about losing political power, its best strategy should be "threaten and suppress." But it did not choose this strategy because it perceived that a military crackdown would result in economic sanctions by other nations. It decided to avoid these sanctions because it was engaged in economic reform, and in 1989 it urgently needed foreign investment. If it was only concerned about economic development, its best strategy would be "do not threaten or suppress." But any tolerance of the movement would result in a loss of power for the Communist Party. This was also an adverse consequence that the government tried to avoid. Because of this dilemma it decided not to disclose its preference to use force, even though it had strong intentions to suppress the movement. This resulted in a game with incomplete information during the first round (see rows 1 and 2 of table 2).

Putnam stated that, while there is the unusual complexity of a two-level game, "there are powerful incentives for consistency between the two games" (1988, p. 434). Where do these incentives originate? How can consistency be achieved? As mentioned before, in the two-level game a given action for one player is simultaneously a move in two different games, and the player does not want to lose either game. Therefore, he must balance his interests by maximizing the sum of his payoffs in the two games.

TABLE 3  
DEALING WITH DEMONSTRATORS IN LIGHT OF MULTIPLE GAMES

Government's Strategy Set	Game with Demonstrators	Game with International Community
Optimal .....	Threaten and suppress	Do not threaten or suppress
Suboptimal .....	Threaten but do not suppress	Threaten but do not suppress

In 1989, the Chinese government had one set of strategies for the demonstrators, and another for other nations. Each set is composed of optimal strategies and suboptimal strategies, which are illustrated in table 3.

Facing a dilemma after the emergence of the 1989 student movement, the Chinese government's only choice was to abandon the optimal strategies in either game and to follow the suboptimal strategies that might mitigate the contradiction between the two games. As illustrated in the third row of table 2, there are two possible outcomes when the government chooses the suboptimal strategies. First, while the government avoids a poor record on human rights, it will win in the game with the demonstrators if they back down. The second possible outcome is that, although the government does not lose in the international game, it will lose in the game with the demonstrators if they resist. It is very obvious that the first outcome is much better than not only the second one but the ones shown in the first two rows of table 2. Therefore, the government must choose the suboptimal strategies if it expects to maximize its utility in the two games.

In short, the two-level game presents the government with a dilemma because it may lose either political power or foreign investment. To maximize the sum of its utility in the two games, it must not disclose its preferences in either game, and its only options are the suboptimal strategies.

#### INTERTEMPORAL DEPENDENCY: A REPEATED GAME

As mentioned before, the 1989 movement is a repeated game in which the four-step game is iterated three times. In the second round, the demonstrators adopted a new strategy. Beginning on May 13, more than 300 students staged a hunger strike at Tiananmen Square, and by May 16, almost 3,000 students were involved. They demanded a meeting with top party leaders and a fair reappraisal of the student movement. On May 20, the government responded by declaring martial law and deploying thousands of troops with tanks around Beijing. While the demonstrators still did not know the government's preferences for the use of force, the

hunger strike stopped. Still, the demonstrators did not back down, and about one million people protested against martial law on May 20 and 21. Local residents in Beijing tried to stop the tanks with barricades and with their own bodies. The Chinese government chose not to use force for the second time.

It is not difficult to explain the government's strategies in the second round by a framework of a two-level game. It still chose the suboptimal strategies: to show force, to conceal its preferences as private information, and to refrain from using force. But it is difficult to explain the demonstrators' actions in the second round. If they were anxious about their confrontation with the police on April 27, how could they lie fearlessly in front of tanks on May 20? Why did thousands of tanks fail to deter the unarmed demonstrators? The first section of this article specified two conditions under which the demonstrators would resist the threat: first, they must believe that the chance is 50-50 or less that the government will use force; second, their expected gain is equal to or greater than their expected loss. Are there any changes in their beliefs, their expected gain and loss, in the second round? What are the dynamics in the repeated game?

The intertemporal dependency of strategy sets is an important concept in answering these questions as well as being a key factor in the repeated game. This term means that "the choices made by the actors in period  $t$  affect their set of feasible choices in a future time period  $t + t'$ , where  $t' > 0$ " (Tirole 1988, p. 206). This section will show how the government's suboptimal strategies in the first round affect the demonstrators' choice in the second round, and the next section will assess how the government's options in the third round become a function of the demonstrators' actions in the second round. Here, the study of the dynamics in the repeated game will focus on the changes in the demonstrators' beliefs and their expected gains and losses in the second round.

### A Decrease in the Demonstrators' Beliefs

When the government concealed its preferences as private information, the demonstrators developed beliefs about them. These beliefs developed from a learning process in which they drew inferences and formed beliefs based on the government's statements and actions. In the repeated game, the demonstrators also modified their beliefs in each round.

There are three variables in this learning process: prior beliefs, new information, and posterior beliefs. First, the demonstrators have prior beliefs in the beginning of each round. In 1989, their prior belief in the first round was that there was a 50% chance that the government would use force to suppress their movement. The second variable is new informa-



tion. The government's statements and actions in each round provide new information that the demonstrators use to assess the government's preferences. The third variable is posterior beliefs. After receiving new information, the demonstrators always revise their prior beliefs and develop posterior beliefs. Posterior beliefs in the first round become prior beliefs in the second round, and so on.

Bayes's rule, a consequence of the axioms of probability theory, describes how posterior beliefs are based on prior beliefs and new information. According to Bayes's rule,

$$P(H|D) = \frac{P(D|H) P(H)}{P(D|H) P(H) + P(D|-H) P(-H)}, \quad (5)$$

in (5),

- $P(H)$  = probability (the hypothesis  $H$  is true)
- $P(-H)$  = probability (the hypothesis  $H$  is false)
- $P(D|H)$  = probability (the data would be observed if the hypothesis  $H$  were true)
- $P(D|-H)$  = probability (the data would be observed if the hypothesis  $H$  were false)
- $P(H|D)$  = probability (the hypothesis  $H$  is true conditional on having observed new data  $D$ )

More specifically, in the main model (fig. 2) the hypothesis  $H$  is that the government is tough, and the data  $D$  is that the government shows force but does not use it. Therefore,

- $P(H)$  = probability (the government is tough)
- $P(-H)$  = probability (the government is weak)
- $P(D|H)$  = probability (the government shows force but does not use it | the government is tough)
- $P(D|-H)$  = probability (the government shows force but does not use it | the government is weak)
- $P(H|D)$  = probability (the government is tough | the government shows force but does not use it).

From (5),

$$\frac{P(H|D)}{P(-H|D)} = \frac{P(H)}{P(-H)} \times \frac{P(D|H)}{P(D|-H)}. \quad (6)$$

In (6), the effect on posterior beliefs of prior beliefs and new information is much easier to see than the effect shown in (5).

On the left side of (6),  $P(H|D)/P(-H|D)$  is the posterior odds that the

TABLE 4

## DEMONSTRATORS' BELIEFS IN THE REPEATED GAME

Round	Prior Odds	Likelihood Ratio	Posterior Odds
1	$P(H)/P(-H)$	$P(D H)/P(D -H)$	$P(D H)/P(D -H)$
2	$P(D H)/P(D -H)$	$P(D H)/P(D -H)$	$[P(D H)/P(D -H)]^2$

hypothesis  $H$  is true, conditional on having observed the data  $D$ . On the right side of (6),  $P(H)/P(-H)$  is the prior odds that the hypothesis  $H$  is true, and  $P(D|H)/P(D|-H)$  is the likelihood ratio that the data  $D$  would be observed if  $H$  were true rather than false. The posterior odds are equal to multiplying the prior odds and the likelihood ratio.

In 1989, the demonstrators' prior odds changed in every round, but the likelihood ratio was a constant because the government repeatedly played the suboptimal strategies in the first and second rounds, and its words and deeds that the demonstrators observed in the different rounds were the same. The likelihood ratio,  $P(D|H)/P(D|-H) < 1$  because the probability that  $D$  (the government showed force but did not use it) would be observed if the government were tough was always smaller than the probability that the data would be observed if the government were weak.

According to (6), in the first round,  $P(H) = P(-H) = 50\%$  (i.e., the students believed that there was a 50-50 chance for the government to suppress by force), then  $P(H)/P(-H) = 1$ . The likelihood was  $P(D|H)/P(D|-H)$ . Therefore, the demonstrators' posterior odds were equal to  $P(D|H)/P(D|-H)$ , which became their prior odds in the second round. While the likelihood was a constant, the demonstrators' posterior odds in the second round were equal to  $[P(D|H)/P(D|-H)]^2$ . Table 4 summarizes this discussion.

$$\begin{aligned} \because 0 < \frac{P(D|H)}{P(D|-H)} < 1, \quad \frac{P(H)}{P(-H)} &= 1, \\ \therefore \frac{P(H)}{P(-H)} > \frac{P(D|H)}{P(D|-H)} > \left[ \frac{P(D|H)}{P(D|-H)} \right]^2. \end{aligned} \quad (7)$$

Equation (7) shows that the demonstrators' beliefs about the probability of governmental suppression by force decrease in the second round of the repeated game, and table 4 contains evidence that there are two reasons for the decrease. First, the likelihood ratio (new information) is constant and always smaller than "1." Second, the posterior odds in every round are smaller than the prior odds.

TABLE 5  
 DEMONSTRATORS' EXPECTED GAIN IN THE REPEATED GAME

Round	Substantive Gain ( $G' > 0$ )	Probability of Success ( $0 < P < 1$ )	Expected Gain ( $G > 0$ )
1 - - - - -	$G1'$	$P1 = 50\%$	$G1 = G1' \times P1$
2 . . . . .	$G2' > G1'$	$P2 > P1$	$G2 = G2' \times P2$ and $G2 > G1$

### An Increase in the Demonstrators' Expected Gain

As mentioned before, the demonstrators' expected gain is equal to multiplying a substantive gain and the probability of the success of their resisting. Table 5 shows how the expected gain changes in the repeated game.

In table 5,  $P2 > P1$  because the demonstrators' beliefs about the probability of governmental suppression by force decrease, which means that the demonstrators believe that the probability of the success of their resistance increases.

As a consequence of the government's suboptimal strategies,  $G2' > G1'$ . In 1989,  $G1'$  was the students demands in the first round that were the legitimization of the first unofficial student organization. After the military police withdrew, the government was forced to negotiate with the unofficial student organization for the first time because no unofficial organizations were allowed to exist after the Communist party took control of China in 1949. In the second round, intellectuals, factory workers, private businessmen, and government officials participated in the movement. Substantive gain in the second round is greater than that in the first round because all of the demonstrators asked to have the political rights to establish their own unofficial organizations. The government, anxious about losing political power, threatened the movement by imposing martial law, which was much harsher than the first threat. But again the government backed down.

In sum, the 1989 movement occurred because the demonstrators tried to change the allocation of political rights in China. The movement's development was a process in which power and rights were reallocated. In each round, the government lost power when it backed down, and the demonstrators demanded more after they had obtained some political rights, which increased their substantive gain.

TABLE 6  
DEMONSTRATORS' EXPECTED LOSS IN THE REPEATED GAME

Round	Substantive Loss ( $-L' < 0$ )	Probability of Failure ( $0 < P < 1$ )	Expected Loss ( $-L < 0$ )
1 .....	$-L1'$	$P1 = 50\%$	$-L1 = -L1' \times P1$
2 .....	$-L2' < -L1'$	$P2 < P1$	$-L2 = -L2' \times P2$ and $-L2 \approx -L1$

#### Constant Value of the Demonstrators' Expected Loss

Table 6 shows how the demonstrators' expected loss changes in the repeated game. Numbers 1 and 2 denote the number of the round,  $-L'$  is a substantive loss, and  $p$  is the probability of failure of the demonstrators' resistance. We see that  $P2 < P1$  because the demonstrators' beliefs about the probability of governmental suppression by force decrease, which means that the probability of the failure of the demonstrators' resistance decreases. If the substantive loss is constant, the value of the expected loss in the number axis is moving to the right toward 0 when  $P2 < P1$  in the repeated game.

In 1989, the demonstrators' substantive loss increases in the repeated game, that is,  $-L2' < -L1'$  because the government's threats escalated step by step. In the first round, the government threatened the movement by erecting seven police blockade lines. The second threat was much harsher than the first, when thousands of troops with tanks were deployed around Beijing. This indicates that the harsher the threat, the greater the demonstrators' substantive loss. If the probability of the failure of the demonstrators' resisting is constant, the value of the expected loss in the number axis is moving away from 0 to the left when  $-L2' < -L1'$ . In the repeated game, the demonstrators' expected loss is almost constant because their substantive loss increases, while the probability of the failure of their resistance decreases.

In sum, this section shows the dynamics of the repeated game. After the government played the suboptimal strategies in the first round, the demonstrators' beliefs about the probability that the government would use force decreased, and their expected gain increased in the second round. Due to these dynamics the two conditions under which the demonstrators resist the threat still existed in the second round. The demonstrators believed that the chance for the government to use force was 50% in the first round, but less than this in the second round. While their expected

gain exceeded their expected loss in both rounds, the difference between the gain and the loss in the second round was greater than that in the first round. Therefore, the demonstrators resisted again in the second round, even though the government's threat was much harsher than that in the first round.

#### INFORMATION GAPS: DISCLOSING AND ACQUIRING PRIVATE INFORMATION

According to Bayes's rule, in the third round, the demonstrators' prior beliefs about the probability that the government would use force were lower than they had been in the second round. Therefore, the dynamics of the repeated game did not change. Why did the movement end in a military crackdown?

The third round began with a hunger strike on June 2, 1989. Most of the student leaders decided, contrary to their earlier position, to pressure the government by occupying Tiananmen Square until June 20, when the standing committee of the National People's Congress was expected to convene. On June 3, the government warned that troops enforcing martial law were ready to pounce, but the demonstrators did not withdraw. When they tried to stop the troops, a tragedy occurred.

There were two significant differences in the third round: first, the government decided "to use force"; second, it did not conceal this preference as private information because it tried to avoid a major confrontation. Why did the government's strategies change, and why did the demonstrators continue to resist? I would say it was because both sides were affected by "interdependent choice." In his discussion of this concept, Kreps explains that "the action taken by any individual depends on the opportunities that are presented to the individuals. Those opportunities, in turn, often depend on the collective actions of others" (1990, p. 5).

What were the "opportunities" presented to the government when it chose "to use force"? In the third round, hundreds of tanks and thousands of soldiers were blocked by barricades around Beijing, and the demonstrators started their second hunger strike at Tiananmen Square. At this stage, the government was desperate. After backing down twice, it had lost its credibility, and some of its political power was shifting to the demonstrators. The best indicator of this shift was that the demonstrators' expected gain had gradually increased (as shown above). In the first round, the students simply demanded official recognition of the first unofficial student organization. In the second round, organizations for intellectuals, workers, and private businessmen were conceived. In the third round, the demonstrators questioned the legitimacy of the government, criticized some of its top leaders, and asked them to resign.

		The Demonstrators	
		Resist	Back Down
Government	Suboptimal Strategies (m)	$Y_3$ $\uparrow \downarrow$ $X_3$	$Y_2$ $\leftarrow \uparrow$ $X_2$
	Optimal Strategies (1-m)	$Y_4$  $X_4$	$Y_2'$ $\rightarrow$ $X_2$

FIG. 3.—A strategic form of the subgame

The possible emergence of a new "society" threatened party officials. Tsou (1992) observed that, in China, before the 1989 movement, "the party-state, through various instrumentalities, occupied most of the social spaces. Society had no institutions or organizations that spoke for it freely and authentically. One can safely come to the conclusion that civil society as such did not exist" (p. 271). In the repeated game, a new "society" was conceived and the government lost political power. In the third round, the government could either continue to lose power or reverse this trend. Since it opposed the emergence of any new "society," it rejected the suboptimal strategies and decided to use force.

The preceding argument suggests a causal relationship between the shift in political power and the change in the government's strategies. One way to test this is to present a strategic form of the main model.

In figure 3, after the government threatened the movement and showed force, each side had two choices. The demonstrators could either resist or back down, and the government could play either the suboptimal or the optimal strategies. If it played suboptimal, it would not use force; otherwise, it would use force. Note that  $Y_2$ ,  $X_2$ ,  $Y_3$ ,  $X_3$ ,  $Y_4$  and  $X_4$  are the same payoffs as those in figure 2. Thus,  $Y_2'$  is the government's payoff if it uses force when the demonstrators back down;  $U(Y_2) > U(Y_2') > U(Y_3)$ , and  $U(Y_2) > U(Y_2') > U(Y_4)$ . Here,  $m$  is the probability that the government plays the suboptimal strategies in the repeated game, and  $1 - m$  is the probability that it plays the optimal strategies.

In figure 3, the demonstrators' preferences are common knowledge, that is,  $U(X_3) > U(X_2) > U(X_4)$ . The government's preferences are private information, but it has been known that for the tough government  $U(Y_2) > U(Y_2') > U(Y_4) > U(Y_3)$ , and for the weak government  $U(Y_2) > U(Y_2') > U(Y_3) > U(Y_4)$ . There is no pure strategy Nash equilibrium in figure 3. In the mixed strategies Nash equilibrium,

$$m = \frac{X_2 - X_4}{X_3 - X_4}, \quad (8)$$

and

$$1 - m = \frac{X_3 - X_2}{X_3 - X_4}. \quad (9)$$

In (8), in the repeated game, the probability  $m$  that the government chooses the suboptimal strategies is determined by the demonstrators' payoffs  $X_2$ ,  $X_3$ , and  $X_4$ . Taking a comparative-static approach (Chiang 1984), the effect of  $X_2$ ,  $X_3$ , and  $X_4$  on  $m$  could be examined.

$$\begin{aligned} \therefore m &= \frac{X_2 - X_4}{X_3 - X_4}, \\ \therefore \frac{\partial m}{\partial X_2} &> 0, \frac{\partial m}{\partial X_3} < 0, \frac{\partial m}{\partial X_4} < 0. \end{aligned} \quad (10)$$

The results from comparative-static analysis are summarized in table 7.

In table 7,  $X_2$  is the demonstrators' payoff when they back down,  $X_2 = -F$ . And  $X_3$  is their payoff when they resist and the government does not use force,  $X_3 = H + G$ . Their payoff when they do resist and the government uses force is  $X_4$  ( $X_4 = -L + H$ ).

It has been demonstrated that the dynamics in the 1989 movement were  $G_1 < G_2 < G_3$ , and  $-L_1 \approx -L_2 \approx -L_3$ . The honor bestowed on the demonstrators in the  $n$ th round is  $H_n$  ( $n = 1, 2, 3$ ). When they resist, they run a risk because of the expected loss. The greater the expected loss, the greater the risk, and the greater the honor. If the expected loss is constant, that is, if  $-L_1 \approx -L_2 \approx -L_3$ , then  $H_1 \approx H_2 \approx H_3$ . Therefore,  $X_3$  ( $X_3 = H + G$ ) is increasing, and  $X_4$  ( $X_4 = -L + H$ ) is constant in the repeated game. What about  $X_2$ ? Here,  $X_2 = -F$ , and  $-F$  is a general cost for the demonstrators when they lose face by backing down. The greater the expected loss, the smaller the cost of losing face. If the expected loss is

TABLE 7

COMPARATIVE-STATIC ANALYSIS IN THE  
STRATEGIC FORM OF THE MAIN MODEL

Independent Variable		Dependent Variable	
$X_2$	$\nearrow$	$m$	$\nearrow$
$X_3$	$\nearrow$	$m$	$\searrow$
$X_4$	$\nearrow$	$m$	$\searrow$

TABLE 8

A DECREASE IN THE PROBABILITY  $\pi$ 

Independent Variable	Dependent Variable
$X_2$	$\pi$
$X_3$	$\pi$
$X_4$	$\pi$

constant,  $-F$  will also be constant. In 1989, the effect of the demonstrators' payoffs  $X_2$ ,  $X_3$ , and  $X_4$  on  $\pi$  is summarized in table 8.

In table 8, an increase in the demonstrators' expected gain  $G$  is a condition under which the probability that the government plays the suboptimal strategies decreases. This is because  $X_3 = H + G$ , and  $H$  is constant. The causal relationship between the shift in political power and the change in the government's strategies is confirmed in table 8. Although the government tried to maximize the sum of its utility in a two-level game in the first two rounds, it lost its game with the international community in the third round. As a result of the demonstrators' gains in political power, the government chose "to use force."

To avoid a major confrontation, the government warned the demonstrators of its plan to use force. The first warning was issued at about 2:00 P.M. on June 3, 1989, at an intersection near Tiananmen Square. More than 1,000 soldiers and armed police attacked the crowds, firing tear gas and clubbing any opposition. This was the first time the government had acted with force to suppress the demonstrators since the emergence of the movement. The second warning was sent about five hours later. Government-controlled radio and television stations repeatedly warned that, if the people entered the streets, "they should bear responsibility for their own fates." The government also sent out vehicles to broadcast this warning. This was the first time the government had issued a serious warning by official broadcast since the emergence of the movement.

News that the government was planning to use force spread quickly, and the demonstrators clearly received the message. It is paradoxical that when they perceived that a crackdown was least likely, the government was clearly signaling its plan to use force.

How would the demonstrators be expected to respond? Most of them did not heed the warning, primarily because not only had the government backed down twice, but had also refrained from using force to suppress popular demonstrations since Liberation. Most of the demonstrators believed that the people's army would never harm the people. Some scholars



who studied the 1989 movement noticed a difference between the government's message and the demonstrators' beliefs. Mu and Thomson argue that "despite the government warning, most people still did not believe that the people's army would really fire on the people" (1989, p. 81). Tsou stated: "At that time, there was a widespread belief that the People's Liberation Army would not turn its guns on the people. It is impossible to know who first began promoting that opinion and how it became so widely accepted. But it is true that many students and Beijing residents believed that the army was 'the people's army' and would not carry out any orders to harm the people" (1992, p. 313).

There was an information gap, which might be defined as the difference between the government's message and the demonstrators' beliefs. How did this gap originate? Did it reflect the demonstrators' subjective wishes or misperceptions? Here misperceptions are defined as "the distorted beliefs and unjustified inferences produced by psychological biases" (Fearon 1992, p. 101).

The information gap between the student movement and the Chinese government resulted from a learning process in which the demonstrators formed new beliefs based on their prior beliefs and on new information, according to Bayes's rule. In order to show how this happened, let

- $P(H)$  = probability (the government is tough)
- $X$  = probability (the government shows force but does not use it | the government is tough)
- $1 - X$  = probability (the government shows force but does not use it | the government is weak)
- $Y$  = probability (the government not only shows force but uses it | the government is weak)
- $1 - Y$  = probability (the government not only shows force but uses it | the government is tough)

In the first two rounds, the likelihood ratio (new information)  $P(D|H)/P(D|-H) = X/(1 - X)$  and  $(1 - X) > X$ . In the third round,  $P(D|H)/P(D|-H) = (1 - Y)/Y$  and  $(1 - Y) > Y$ .

According to Bayes's rule, the demonstrators' prior beliefs in the third round were equal to their posterior beliefs in the second round, which were  $[P(D|H)/P(D|-H)]^2$  in table 4, and can be expressed as  $[X/(1 - X)]^2$ . The likelihood in the third round,  $P(D|H)/P(D|-H)$ , can be expressed as  $(1 - Y)/Y$ . Then after the government signaled its intentions, the demonstrators' posterior beliefs would be

$$\left(\frac{X}{1 - X}\right)^2 \times \left(\frac{1 - Y}{Y}\right) \sim \frac{X}{1 - X}. \quad (11)$$

In (11),  $X \approx Y$ , because in principle the probability that the tough government does not use force is very close to the probability that the weak government uses force. In (11),  $(1 - Y)/Y > 1$ , which means that the probability of governmental suppression by force is greater than 50%, based on the message sent by the government. But the posterior beliefs  $X/(1 - X)$  are still smaller than 1, which meant that the demonstrators continued to believe that the probability of governmental suppression by force was less than 50%. This reflects their prior beliefs. According to (11), the demonstrators' beliefs were not subjective wishes or misperceptions. Let  $Z$  denote the information gap, then

$$Z = \frac{1 - Y}{Y} - \frac{X}{1 - X}. \quad (12)$$

Thus,  $Z$  resulted from the process of disclosing and acquiring private information in the repeated game. The government clearly signaled its intention to use force. The demonstrators received and revised the message, based on their prior beliefs. When the government began to use force, the demonstrators still resisted because they perceived that the probability of governmental suppression by force was less than 50%, and their expected gain was much greater than their expected loss. In this game, the unintended outcome of the movement was a perfect Bayesian equilibrium.

Following the government's broadcast on June 3, hundreds of thousands of demonstrators, sensing that the people's army would never hurt the people, appeared on Changan Boulevard to block the troops. At about 10:30 P.M. armed with tanks, assault rifles, and machine guns, the troops arrived at West Changan Boulevard. When the demonstrators confronted them, a military crackdown occurred, and the confrontation was costly for both sides. Many demonstrators died defending their ideal, economic sanctions from other nations ensued, and more than 40,000 intellectuals and students left China. For both sides, the cost was predictable before the crackdown, therefore, both sides wanted very much to avoid it, but the information gap between the government and the demonstrators led to an unintended outcome, even when both sides behaved rationally. The cost of this outcome was the price of proving and acquiring private information.

## CONCLUSION

By applying game theory to the study of the 1989 Chinese student movement's development and outcome, this article explains why threats made by the Chinese government, backed by a strong military, could not deter the demonstrators in 1989, and why the outcome was unexpected. The

interactive framework—who does what when and with what information—is a principal focus of this study, and information is the most important element in this framework. The demonstrators' actions, which were based on incomplete information, were different from those based on more complete information. The information was incomplete because the government concealed its preferences as private information in a two-level game. Intertemporal dependency of strategy sets was another important element in the framework. Decisions made by the Chinese government at the beginning of the movement affected its strategic choices in the final round; the demonstrators' expectations of the government's strategies, formed in the first round, resulted in the information gap in the third round when the message sent by the government failed to influence the demonstrators' beliefs. The unintended outcome of the movement resulted from rational interaction between the two sides. The cost of the unintended outcome was the price of disclosing and acquiring the government's private information that the Communist Party did not want to relinquish any political power, even though there had been a successful economic reform in China.

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# Peasants Help Destroy an Old Regime and Defy a New One: Some Lessons from (and for) the Study of Social Movements<sup>1</sup>

John Markoff  
*University of Pittsburgh*

Recent work on social movements has drawn attention to the interactions of movements and power-holding elites. A dual examination of the changing character of rural revolt in France between 1788 and 1793, on the one hand, and the collapse of the Old Regime and subsequent legislation on rural affairs, on the other, shows that both elite action and insurrectionary mobilization were shaping each other. The revolutionary assault on "feudalism" was forged in the course of this short-term coevolution. Sociologists who study social movements sometimes need to devote considerable research to uncovering the detailed histories of elite actions.

## MOVEMENT CHALLENGERS AND ELITE POWER HOLDERS IN REVOLUTIONARY FRANCE

As recently as the early 1990s, a prominent student of social movements (Tarrow 1991) could regret the scarcity of studies that attempted to connect social movements with change in social policies, a scarcity truly remarkable in light of the role that the goal of change plays in many scholars' very definition of a movement. Perhaps the explanation for this "curious puzzle" (Tarrow 1991, p. 71) is that such studies fell between self-imposed disciplinary boundaries. Political scientists, who described with loving care the minutiae of open electoral competitions, the semipublic world of institutionalized lobbyists, and the hidden realm of bureaucratic infighting, hardly dealt with movements at all. Sociologists, on the other hand, had a long tradition of describing with loving care the more curious, quirky, inspiring, or repulsive features of social movement action; the Marxists among them devoted considerable attention to the ways in which

<sup>1</sup> This article originated as a talk at Northwestern University. I thank all the participants in that event for their comments, especially Arthur Stinchcombe for the suggestion to make of that talk an essay. I also thank Sidney Tarrow and Charles Tilly for reactions to an earlier draft. Send correspondence to John Markoff, Department of Sociology, University of Pittsburgh, Pittsburgh, Pennsylvania 15260.

the organization of work and the state of economic institutions fostered movements. But the crafting of policy was largely left to the political scientists and the evolution of institutions to the historians (although the turn toward a focus on the state by some sociologists somewhat blurred these disciplinary distinctions).

Complementing this deficit was a gap in the study of movements as dynamic entities. Before the 1970s, scholars focused on explaining the emergence of movements, but studies of how movements respond to change and, in responding, themselves change were scarce (McAdam, McCarthy, and Zald 1988).

In recent years, several important concepts have opened the way to a far more dynamic conception of social movements. A number of scholars (see, e.g., Doug McAdam 1982), in pointing to "political process," have set us the agenda of looking for the ways in which the emergence, strategies, tactics, and organizational forms of movements have a good deal to do with the way elites create opportunities.<sup>2</sup> Sidney Tarrow's (1991) notion of interlinked cycles of "protest" and "reform" shaped by the actions and interactions of movement challengers and power-holding elites not only suggests a good deal of short-term change on both sides but also tries to specify how change on one side helps bring out change on the other. And Charles Tilly (1988, p. 2) even insists by way of definition that social movements "consist of sustained interactions among groups and individuals," putting the interactive character of the relationship between movements and power holders in the spotlight. A significant body of work has begun to employ such perspectives.<sup>3</sup>

In this article, I want to apply such insights to a particularly dramatic, even world historic, instance: the dialogue that took place from 1788 into 1793 between rebellious peasants in thousands of French villages and the Revolution's emerging elite. The point of this instance is not simply to demonstrate that such conceptions can illuminate a central episode in modern history, although they can do that, but to try to make two rather stronger points: first, that it is sometimes simply impossible to make much sense of the behavior of a challenging movement on the one hand or of governing elites on the other without fairly detailed attention to the actions of both parties; second, that some very important things sometimes

<sup>2</sup> Earlier work emphasizing opportunities includes Lipaky (1968), Elsingner (1973), Schwartz (1976), Piven and Cloward (1977), Jenkins and Perrow (1977), and Tilly (1978).

<sup>3</sup> Work from the 1980s and beyond exemplifying these and similar perspectives include Katznelson (1981), McAdam (1983), Tarrow (1989, 1993), Koopmans (1993), Kriesi (1993), Duyvendak (1995), Franzosi (1995), Giugni (1995), Tilly (1995), and McAdam, McCarthy, and Zald (1996).

come together in a very short span of time that are not adequately accounted for by long-term processes.

In the spring and summer of 1789, French peasants refused payments in cash and kind, broke into the lords' homes to compel them to renounce publicly their claims, seized or destroyed the lords' documentary evidence of their rights, drove their animals onto the lords' fields, killed the pigeons or rabbits the lords were permitted to raise, and destroyed the emblems that indicated that the *château* was more than just an attractive house (its weathervane, its coat of arms, its decorative turrets). For some three years thereafter, attacking the rights of the lords was the dominant form of rural collective action (Markoff 1995, 1996).

On August 4, 1789, the revolutionary National Assembly began a hectic week of meetings to discuss the rural situation (and much more besides), summarizing its decisions by proclaiming the abolition of "the feudal regime in its entirety" (Mavidal and Laurent 1879, 8:397–98).<sup>4</sup> The work of the legislature was no more finished with this ringing declaration than the insurrectionary activity of the peasants was finished that same summer. The next several years saw a large number of laws on the fate of the lords' rights. By Peter Jones's (1988, p. 87) count, there were some 96 enactments by the fall of 1791—with more to come. The very first contribution to the discussions of August 4, 1789, introduced the notion of distinguishing some of the lords' rights that were to be simply abolished from others that were to be maintained pending the payment of an indemnity to the lord (Mavidal and Laurent 1879, 8:343–44). Some of the subsequent items of legislation were concerned with spelling out the terms of indemnification, others with clarifying which rights were in which category, still others with defining the legal procedures for adjudicating disputes about which category a particular right was in, and others yet again with developing a legal theory on which to base such specific decisions. After much redefinition of the categories, ultimately the very distinction was eliminated. We have two processes then: an insurrectionary process with peaks and troughs of rural collective action and a legislative process with moments of relatively minor legal refinements and moments of major revisions in the law.

In this essay, I consider the relationship of these two processes—insurrection and legislation—to one another. While the empirical focus of this article is one very specific historical episode, I believe that its examination suggests that certain ways of thinking about social movements are likely to be very fruitful in relation to many other episodes and that structural accounts of revolution have some very important limitations.

<sup>4</sup> The translations from the French throughout the article are mine.

A PEASANT-BOURGEOIS ALLIANCE

Revolutionary peasants and revolutionary legislators together ended the seigneurial regime. How was this antiseigneurial convergence achieved? There have been two principal grand narratives of the Revolution within which this joining of forces has an important place: a Marxian story and a Tocquevillean one.<sup>5</sup> In the Marxian account, changes in the material conditions of existence bring about new structures of interest. These structures align people in new patterns of conflict as they come to have a sense of their commonalities of interest with some and their antagonistic interests in relation to others. An advancing capitalism forged an antifeudal alliance between a cramped elite, chafing under outmoded restrictions on their initiatives, and a threatened peasantry, hard pressed by the lords' deployment of traditional privileges for their advantage in the modern market.

Tocqueville (1955), too, saw a coincidence of peasant and bourgeois interests against seigneurial rights, but he located the matrix of this tacit alliance in a cultural shift that was in turn rooted in the enlargement of state power. The long process of state development eroded the basis on which others would accept the positions of nobles and lords in French society. To the extent that they were central in the provision of services through their responsible domination of public affairs, their privileges could be seen as deserved. But, as these functions passed into the hands of the central state, the entire justification for noble and seigneurial privilege evaporated (Tocqueville 1955, pp. 32–41). Elite commoners came to find unbearable the continuation of social distinctions; as for the peasantry, the military and political erosion of the lord's role had turned a once-genuine protector into an exploiter.

Both interpretive frameworks make sense of a peasant-bourgeois alliance against the lords. But, as Lynn Hunt (1984, pp. 1–16) tellingly points out, neither grapples with the Revolution itself as a political process. The alliance is seen as a straightforward by-product of the common interest of peasants and bourgeois in dismantling the seigneurial regime. Long-term structural change reoriented group interests, turning an envious commoner elite and a subordinate peasantry against the lords of France: revolutionary antiseigneurialism was a foregone conclusion.<sup>6</sup>

<sup>5</sup> Although I specifically consider the two best known explanations of the Revolution as the outgrowth of long-term structural change, the argument developed here should apply as well to other interpretations that assign the lion's share of explanatory significance to long-term forces.

<sup>6</sup> The phrase is used in the title of Tocqueville's final chapter: "how, given the facts set forth in the preceding chapters, the Revolution was a foregone conclusion" (1955, p. 203); a Marxian equivalent is the evocation of the chains, which, having to be broken, were broken (Marx and Engels 1955, 1:39).



## PREVIEW OF THE ARGUMENT

I will present data that demonstrate that an antiseigneurial convergence of villagers and the new men at the center of power did not exist at the beginning of the Revolution, but that it developed. It follows that attempting to explain the antiseigneurial actions of peasants or of legislators in terms of slow-changing social structures must omit something essential that happened in the course of the Revolution itself.

I will argue that a good deal of what happened can only be understood by examining how the actions of country people and legislators were each contexts for the actions of the other, that there was, in short, a dialogue. Rather than a peasantry and a new elite whose common antipathy for feudalism was forged, separately, over the centuries, a significant aspect of their antiseigneurial convergence came from their interaction during a relatively brief period.

## THE COURSE OF RURAL INSURRECTION

### The Old Regime's Insurrectionary Tradition

What were the usual targets of popular insurrection before the Revolution? A research team headed by Jean Nicolas and Guy Lemarchand has been scouring French archives to build a data set for collective actions from 1661 to the spring of 1789 (Nicolas 1986; Lemarchand 1990). Their preliminary published classification is revealing: conflicts over subsistence account for some 22% of all events, conflicts over taxation and clashes with police or judicial authorities make up like proportions, while another 13% are conflicts involving youth groups (e.g., intervillage brawls; Nicolas 1986; 1990, p. 140). Only then do we get to antiseigneurial actions, a scant 7% of all conflictual events in the century and a quarter preceding the Revolution (Lemarchand 1990). This picture of subsistence and tax conflicts as the major arenas of social struggle in the 18th-century countryside is affirmed by other more regionally circumscribed studies (Frayssenge and Lemaître 1985; Pillorget 1985; Poitrineau 1985).

The Nicolas-Lemarchand data also show that the overall pace of rural insurrection picks up in 1775 and, after a brief fallback, rises again in the 1780s. During the last five years of the Old Regime, the proportion of antiseigneurial disturbances was rising. Nonetheless, even in that last half decade, antitax events still were outnumbering antiseigneurial events two to one (Nicolas 1986).

Other studies support this picture of a limited antiseigneurial shift. Bouton's (1993) research, for example, shows that the great wave of food riots of 1775, "the Flour War," was marked by the appearance of a new form of subsistence battle. Since the emergence of widespread conflicts over the

price and availability of grain, flour, and bread in the 1690s, the major forms of subsistence action were (1) invasions of marketplaces, including seizures of food and coerced price setting, and (2) blockades of grain shipments, including attacks on grain convoys. In 1775, bands in northern France, in addition to engaging in these by now classical forms of action, "visited"—in the 18th-century idiom—large grain producers to search for stocks withheld from the market—and these large producers included lords. So an antiseigneurial twist was becoming a part of the repertoire of subsistence struggle, to use a coinage of Tilly's. Subsequent waves of conflict over food supply continued to incorporate the innovations of 1775. Nonetheless, these newer forms of conflict were still a minuscule proportion of subsistence events.

### Peasant Grievances at the Onset of Revolution

In the spring of 1789, some 40,000 village assemblies throughout France drafted statements of their grievances. These grievance lists (the *cahiers de doléances*) served as instructions to deputies in the complex elections for the Estates General. Nobles, clergy, and town dwellers also produced such documents. We may use a content analysis of a sample of these documents to see the place of the seigneurial rights among rural complaints.<sup>7</sup> A baker's dozen of the most frequent subjects of rural attention in that spring includes no fewer than 10 concerning taxation (see table 1), and not a single one dealing with seigneurial rights. If we consider all rural grievances, classified by whether they address a material burden on the peasantry, we see in table 2 that although such burdens are a very significant part of the rural sense of grievance, taxation far outweighs seigneurial rights.

We are able, to a very limited degree, to compare the salience of the seigneurial regime in the grievances of 1789 with the past. A study of the grievances of one small electoral circumscription in 1614 (the date of the last previous Estates General) shows that 49% of grievances addressed taxation, compared to 3% on the seigneurial regime. In 1789, the comparable taxation figure in that circumscription has fallen to 33%, while the seigneurial category is up to 11% (Chartier 1982, pp. 108–9). Just as the pattern of insurrection shows antiseigneurial actions to have risen in the 125 years that preceded the Revolution, so, judging by the one local study available, did complaining about the seigneurial regime. But that

<sup>7</sup> The claims made here about the grievance lists are drawn from a content analysis of them carried out together with Gilbert Shapiro. For an account of the documents, the sample, and the methods of content analysis, see Shapiro and Markoff (1997).

TABLE 1

SUBJECTS MOST WIDELY DISCUSSED IN PARISH *CAHIER*S  
RANKED BY FREQUENCY OF DOCUMENTS DISCUSSING THEM

Rank	Subject
1 .....	Taxation in general
2 .....	Salt tax ( <i>gabelle</i> )
3 .....	Tax on alcoholic beverages ( <i>aides</i> )
4 .....	Salt monopoly
5 .....	Tax on legal acts ( <i>droit de contrôle</i> )
6 .....	Compulsory labor service on roads ( <i>corvée royale</i> )
7 .....	Provincial estates
8 .....	Principal direct tax ( <i>taille</i> )
9 .....	Praise of Louis XVI
10 .....	Tax advantages of clergy
11 .....	Tax advantages of nobility
12 .....	Court officers who supervised auctions ( <i>priseurs</i> )
13 .....	Customs duties ( <i>traites</i> )

TABLE 2

PARISH GRIEVANCES CONCERNING  
BURDENS AS PERCENTAGE  
OF ALL GRIEVANCES

Grievances	%
Taxation .....	32
Clerical tithe and <i>casuels</i> .....	4
Seigneurial regime .....	10
Total burdens .....	46
<i>N</i> .....	27,742

increased attention still left it far less discussed than the more traditional target of complaint and mobilization, taxation.

If we ask what the people of the countryside proposed to do about taxation as opposed to seigneurial rights, however, we can observe that the latter occasioned more drastic demands. Some 36% of grievances about seigneurial rights involved demanding the abolition without compensation of one or more of these rights; for taxation, the corresponding figure is 24%. On the other hand, some 42% of demands about taxation envisage "reforms"—the elimination of abuses, greater equity in assessments, greater efficiency in collection, and many other ameliorative measures; a

mere 15% of relevant rural demands envisage seigneurial reforms. By March 1789, then, the peasantry were more profoundly hostile to seigneurial rights but remained more preoccupied with taxation.

### Targets of Peasant Insurrection at the Onset of Revolution

One of the symptoms (and causes) of the collapse of the Old Regime was the rising incidence of rural upheaval. Did attacks on the seigneurial regime occupy a prominent place among peasant actions toward the beginning of the Revolution? To answer this question, we may draw upon a data set of 4,689 rural "events," episodes in which some collectivity openly seized or damaged the resources of another party or defended themselves against another party's claims upon them, that took place between June 1, 1788, and June 30, 1793. The time frame was chosen to begin with the point in the elites' political crisis when the monarchy felt compelled to summon the Estates General and to end at the point that the legislature definitively abolished the seigneurial regime.

The data were gathered from an extensive survey of the published literature. The starting point was Ado's (1971) extensive documentation of the French peasant movement, an attempt to synthesize historians' research as well as exploring administrative correspondence, reports of committees of revolutionary legislatures, letters from local officials, and petitions. Ado's documentation is particularly extensive on conflicts involving the claims of the lords and subsistence questions but needs extensive supplementary exploration of the substantial literature on other manifestations of rural contention.

Such sources are much clearer about when some form of clash commenced than when it ended, making the study of duration unfeasible. The sources are often poor on the size and usually poor on the composition of groups. What was feasible, however, was to record places, dates, and targets. In the present study, I shall classify the events into a small number of very broad categories based on their targets (such as "antiseigneurial" or "counterrevolutionary"). A data file culled from the published literature probably favors large and dramatic actions and events occurring near larger urban centers. It probably also undercounts conflicts around taxation compared to many other forms of conflictual events because such conflicts have not been the focus of much interest in historians' debates about the Revolution. (Since this latter bias is of particular relevance to this article, most of my arguments will draw on changes in the proportions of attacks on the seigneurial regime rather than on their overall frequency; see Markoff 1996.)

From the summer of 1788 into the spring of 1789, elites were quarreling over the form of the Estates General and much else besides; many groups

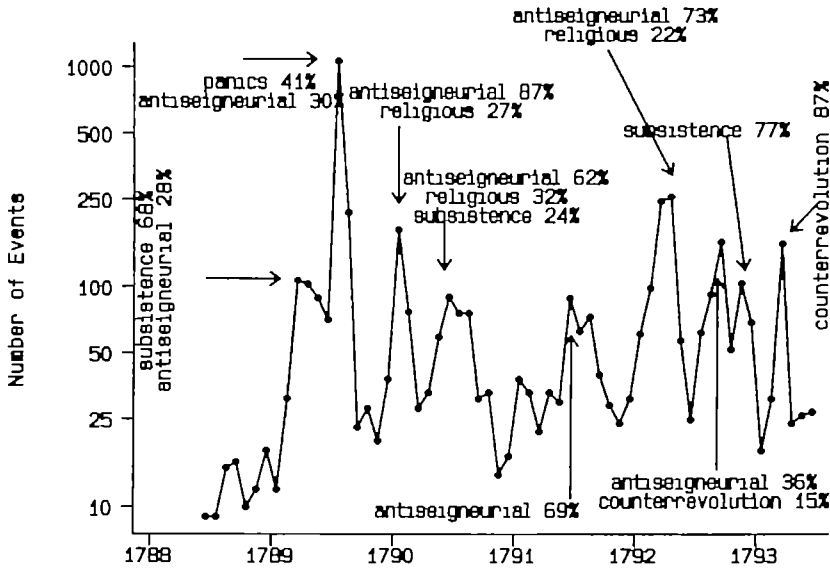


FIG. 1.—Insurrectionary events (by month; based on 4,689 events)

and individuals campaigned for the allegiance of the country people. If we look at the actual insurrectionary actions of those same country people in the period from June 1, 1788, through February 28, 1789—stopping just short of the moment when assemblies were electing delegates and drawing up grievance lists—our data set contains some 132 incidents of which 12% have an antiseigneurial aspect. At the onset of the Revolution, antiseigneurial events were far outweighed by struggles over food. To whatever degree the structural changes over the centuries in economy, state, and society had redefined the interests of French country people, their complaints and their disruptive collective actions continued to be directed, for the most part, at other targets at the beginning of the Revolution.

#### Peasants Turn to Antiseigneurial Action

Over several months, the targets of country people shifted. Figure 1 shows the ebb and flow of insurrectionary events in the countryside and, for each of the points of peak activity, roughly indicates the nature of the events. (Note that these categories are not exclusive ones.) By March 1789, 28% of events were antiseigneurial, in August, 30%. But it was after the sum-

mer that peak periods of rural activism became overwhelmingly antiseigneurial (87%, 62%, 69%, and 73%) and remained so into the fall of 1792, after which antiseigneurial actions recede. So the question to be addressed is Why did peasants *turn to* antiseigneurial actions?

#### THE NEW ELITES: FROM GRIEVANCES TO LEGISLATION

Roughly half the delegates to the Estates General—those representing the Third Estate—were elected by meetings dominated by France's urban notability, the nonnoble upper stratum of professionals, officeholders, businessmen, and landholders of France's towns and cities. Since the assemblies drafted grievance lists for their deputies, those grievances indicate the tenor of the discussions those deputies themselves had been involved in during spring 1789. A significant minority of Third Estate demands concerning the seigneurial rights advocated uncompensated abolition of one or another right (27%). But the most distinctive feature of the Third Estate's grievances was the support for the payment of an indemnity to the lord as a condition of abolition. Table 3, which distinguishes one seigneurial right from another, shows that, for the very important periodic dues, indemnification rather than simple abolition was preferred.

Five months later, in early August, the National Assembly—the new, revolutionary name that had been assumed by the deputies—spent an intense week discussing “the feudal regime.” Although the National Assembly stirringly proclaimed that it was abolished “in its entirety” (Mavisdal and Laurent 1879, 8:350, 397–98), the debate from the beginning had accepted the distinction of rights to be abolished with and without compensation, following up the positions staked out in the *cahiers*. Pending such compensation, the National Assembly made clear that payments were still to be paid by peasants to those now defined officially as their “former” lords. Detailed legislation specifying precisely what was to be abolished and what was still a peasant obligation, pending an indemnity, awaited March 1790; the indemnification rates were not set until May. The new legislation that claimed to abolish feudalism, and had indeed abolished many important seigneurial rights, nonetheless left many people in the countryside still expected to pay what they had always paid and to the same person to whom they had always paid it (only now that person was no longer called a “lord” but was a “proprietor”).

Early in the Revolution, then, the National Assembly had not moved beyond the positions taken in the grievance lists of the Third Estate: a limited abolition of seigneurial rights, and a gradual phaseout of the rest, was enacted, which would no doubt have taken generations to complete,

if completion were possible at all.<sup>8</sup> A very large number of subsequent enactments followed, modifying, in larger and smaller ways, the structure of the initial legislation. Only with the legislation of July 17, 1793, may one speak of a definitive abolition of seigneurial rights. For the new revolutionary elite, as for the peasants, it is a *process* we need to explain, not a set of interests nor an ideology that the Revolution merely carried out.

Charles Tilly has urged us to see that major shifts in the nature of contention are not just outgrowths of the contingencies of the moment. There are deep, long-run processes that realign interests and that create or destroy capacities for action. We must, Tilly (1986, p. 9) writes, "look beyond narrowly political explanations." But deep, long-run processes may also not be enough: we need to look at the contingencies of the moment as well, for if long-run processes may realign interests and resources, short-run contingencies may provide vital opportunities. Sometimes these contingencies may be a more important part of the story.

#### REVOLUTIONARY PEASANTS AND REVOLUTIONARY ELITES: A DIALOGUE

##### Peasants Respond to Elites

Those engaged in risky actions are likely to be very sensitive to which sorts of activity are likely to lead to immediate violent responses or subsequent judicial retribution. We ought not forget that insurrectionary peasants ran serious risks, as we are reminded in noting the traveler Arthur Young's (1915, pp. 303-4) remarks on the countryside that greeted his eyes on entering France: "not far from Verpilliere pass the burnt château of M. de Veau, in a fine situation with a noble wood behind it. Mr. Grundy was here in August, and it had then but lately been in ashes; and a peasant was hanging on one of the trees of the avenue by the road, one among many who were seized by the *milice bourgeoise* for this atrocious act." Unless we assume that those who engage in such risky actions are utterly lacking in rational faculties, surely they adapt their actions to some sense of likely success, success understood not merely in avoiding being hanged but in obtaining goals.

For anyone in France's 40,000 villages trying to understand the potentials of the unprecedented opportunities (and risks) afforded by the disin-

<sup>8</sup> The closest parallel to, and most important model for, the French revolutionary abolition was that of neighboring Savoy, whose duke decreed an indemnificatory process in 1762. Thirty years later, seigneurial rights were still alive and well and were only terminated by virtue of occupation by the revolutionary French army (Bruchet 1908).

TABLE 3  
PERCENTAGE OF THE THIRD ESTATE *CAHIER*S WITH VARIOUS POSITIONS ON SEIGNEURIAL RIGHTS

Right	Maintain	Reform	Abolish without Compensation	Abolish with Indemnification	No. of <i>Cahiers</i> Discussing Right
Periodic dues:					
<i>Cens</i> (a cash payment) .....	7	32	4	57	28
<i>Champart</i> (a portion of the crop) .....	2	26	13	60	61
<i>Cens et rentes</i> (a cash payment) .....	3	35	0	59	37
General .....	0	23	7	55	30
Miscellaneous .....	0	36	7	9	22
Seigneurial monopolies:					
Ovens .....	2	8	56	29	50
Milling .....	0	13	44	35	70
Wine press .....	0	5	59	31	44
General .....	0	15	40	43	103
Assessments on economic activity:					
Seigneurial tolls .....	0	9	53	27	117
Dues on fairs and markets .....	0	16	36	33	45
Property transfer rights:					
Dues .....	0	39	37	12	49
<i>Retrait</i> (substitution of lord for purchaser of property) .....	4	25	44	0	48





tegration of the Old Regime and the attempts at constructing a new order, a vast amount of information was readily available. During the crisis surrounding the convocation of the Estates General, France was flooded with pamphlets that not only advocated various positions on the immediate issues of the Estates General itself but also took up a wide variety of questions. Champions of the Third Estate and of the nobility waged an intense campaign for the hearts and minds of the countryside as the elections approached. "Model *cahiers*," that is, *cahiers* recommended by one or another contentious party, circulated widely. The elections themselves sent rural delegates to meet with townspeople. Once the Estates General convened in May 1789, its deputies frequently wrote home. A new journalism was born that competed to provide rapid and public accounts of the high and mighty, a practice sanctioned by the very claims of the revolutionaries to be eager to submit themselves to the stern scrutiny of the public. Thousands of political clubs linked in loose networks added to the diffusion of information. The early revolutionary years saw a proliferation of elections for local, departmental, and national positions, providing for still more information flow (Popkin 1990; Boutier and Boutry 1992; Edelstein 1990; Tackett 1989).

Against this background, let us consider the ebb and flow of insurrection, and the rising—and then ultimately falling—proportion of insurrections targeting the seigneurial rights. Table 4 presents percentages of anti-seigneurial events over time and will help structure the discussion that follows.

*The crisis of the Old Regime and the elections for the Estates General.*—Between June 1788 and the elections, the French were discovering that the costs of insurrection were falling and the potential rewards rising as coercive capacities declined and the Estates General seemed imminent. During this period, only one event in eight had anything to do with the seigneurial regime. The electoral period intensified contact with the well to do. As pamphleteers floated model *cahiers*, as village activists sought out information on what was happening in the towns, as urban and semi-urban advice givers sometimes helped villagers in the actual crafting of the parish documents, the French countryside became more aware of the antiseigneurial sentiment within the elites. Rural delegates by the hundreds at one electoral meeting after another could not have helped discovering the elaborate antiseigneurial programs of the dominant urban strata of the Third Estate, which, while perhaps disappointing in the emphasis on indemnification, were surely very promising in the degree to which the subject was not only broached but broached in detail—often in much greater detail, indeed, than in the country documents themselves. Furthermore, the rhetorical temperature of these urban texts was often

TABLE 4  
PERCENTAGE OF EVENTS WITH ANTISEIGNEURIAL ASPECTS  
OVER TIME

	%	No. of Events
June 1788–February 28, 1789 .....	12	132
March 1–June 30, 1789† .....	25	370
July 1–August 11, 1789† .....	31	1,235
August 12–December 31, 1789 .....	37	158
January 1–March 28, 1790* .....	78	285
March 29–May 31, 1790 .....	32	95
June 1–June 30, 1790* .....	62	90
July 1, 1790–May 31, 1791 .....	49	403
June 1–June 30, 1791* .....	69	89
July 1, 1791–January 31, 1792 .....	34	321
February 1–April 30, 1792* .....	47	605
May 1–August 25, 1792 .....	31	202
August 26–October 31, 1792† .....	36	248
November 1–31, 1792† .....	9	104
December 1, 1792–February 28, 1793 .....	14	118
March 1–March 31, 1793† .....	3	158
April 1–June 30, 1793 .....	12	77

\* Antiseigneurial peaks, from fig. 1

† Insurrectionary but not antiseigneurial peaks, from fig. 1.

very elevated in their depiction of the barbarous past and the generally abusive nature of the political and social systems of feudalism.<sup>9</sup>

The country delegates were not only certain to discover all this as they sat at these meetings, they were also fairly likely to discover as well a good deal about the position of the local nobility, for there was often much contact between the two orders and occasional attempts to adopt a joint document. And in the weeks before the *bailliage* meetings of the spring, noble propagandists, like those of the Third Estate, had pamphlets and model *cahiers* distributed. Peasant delegates to the town might well have noted, for all the conservative thrust of the noble *cahiers*, that the nobility did not mount a full-blown defense of seigneurial rights but were choosing silence as a course of prudence. Some quarter of noble *cahiers* have not

<sup>9</sup> In the *cahier* of the Third Estate of Poitou, e.g., we read: "If, for many centuries, France languished in ignorance, anarchy and confusion, those were the centuries of the feudal regime, when the lords, enjoying their usurped authority, crushed possessions and persons alike under an equal servitude" (Mavidal and Laurent 1879, 5:412).

a word about seigneurial rights. The villagers were no doubt discovering as well that sympathetic parish priests were triumphing over the bishops and canons in the clerical elections and over the delegates of the monasteries that often were the most important ecclesiastical lords (Lemay 1991, p. 171).

In response, the proportion of insurrections targeting the seigneurial regime doubled in the spring. The country people were discovering that if they pushed hard there would be at least partial support from significant portions of the Third Estate and an important portion of the clergy, and they were probably aware of the incapacity of the nobility to defend themselves. If, early in the Revolution, villagers' sense of enlarging possibilities came from the doings of local and regional elites, we ought to be able to explain regional variations in peasant action as a seizure of opportunities or a recognition of constraints, posed by the local state of elite politics.

During this period, the spotlight must fall on two regions, Brittany and Provence. From June 1788 to January 1789, some 70% of incidents were in western France, many of them grain riots that began in Brittany in June 1788. The insurrectionary politics of the Breton countryside was keenly sensitive to the actions of men of wealth and power. The Breton elite was unusually polarized at an early date, and the prelude to the convocation of the Estates General was unusually bitter, with an increasing tension between a radicalizing urban commoner elite, who went on to constitute an initial nucleus of the left at the Estates General, and an intransigent nobility and upper clergy, who ultimately refused to participate in the elections. Egret (1955) showed in a classic essay how the struggle over the Estates General drew on urban collective actions in the form of pitched battles in major towns. Dupuy now urges us to see the whole period leading up to the convocation of the Estates General as a fruitful opportunity for peasant action as well, as the old order came apart at the top. From the summer of 1788 on, in fact, the Breton nobility attempted to gain the support of peasants in their struggles with the king, on the one hand, and the nonnoble urban elites, on the other, by circulating a great deal of literature in Breton as well as French. The urban notables replied with their own pamphlet counterattack (Dupuy 1989, pp. 24–32). In the countryside, the rioting began. Thus the "precocious politicization," in Dupuy's fine phrase (1989, p. 19), of the Breton countryside has a lot to do with the fierce intraelite battles and their efforts to enlist peasant allies.

If Brittany's case shows an especially bitterly divided elite provoking rural mobilization, Provence's shows an elite invitation to take up the banner of antiseigneurialism. In the period between the elections and the Estates General's redefinition as the National Assembly in June, no rural area was more explosive than the French southeast, a third of whose actions focused on the seigneurial regime. Provence, then, was a leader in

the peasant turn to antiseigneurial action. Like Brittany, Provence had an intransigent noble elite hoping to use the weakening of the monarchy to reassert its claims of tradition, but in this case the intransigent core was Provence's fief-holding nobles who had lost their exclusive right to speak for all the province's nobility in 1639 and now were demanding a provincial estates on the old model. At the same time, elements of the urban elites saw their own chance. The result was an intensive campaign of petition and counterpetition that reached down into towns and villages as "general assemblies" of the heads of families met to pass resolutions. The provincial commandant, at one point, noting pamphlets circulating in Provençal, expressed his shock at the effort of the notables of Sisteron to "address the peasants and workers in their usual language in order to get them to take an interest in present affairs" (quoted in Cubells 1987, p. 68).

Cubells suggests that the great division between fief-holding nobles and their non-fief-holding fellows made the seigneurial regime an unusually salient element in the struggles around the Estates General in Provence. It is striking that Provence's spring upheavals were particularly intense during those March weeks in which rural communities were formulating their grievances.

*The first summer of revolution, and beyond.*—If regional political contexts help us grasp popular engagement in revolution prior to the summer of 1789, during that summer, peasant communities throughout France all began to have a common, powerful interlocutor: the revolutionary legislature. The national legislature had to deal with 40,000 villages, many of which had discovered their capacity to mobilize.

As spring gave way to summer, the greatest peak of peasant actions exploded. Many very different targets were attacked. In spite of the diversity of targets, the antiseigneurial propensity of France's villagers rose further still. The percentage of antiseigneurial actions in the summer of 1789 was two and one-half times what it was before the elections.

The August decrees of the National Assembly, although backing indemnification for many payments, at least made it clear that revolutionary legislators were willing to move on seigneurial rights: it was one thing to call for such a program in the *cahiers*, and it was a step further to actually enact it. It was also a ringing declaration of the new center of gravity of national politics. The countryside was learning where and how it could expend its energies and which risks were worth running. Even though the decrees may have been unsatisfactory in much of the countryside, they testified to a significant elite antiseigneurialism.

Revolutionary legislation revealed, suggested, and created opportunities for action, not necessarily in accord with the legislators' wishes. Consider the comment of the deputy Bailly (1821, 2:244): "The National As-

sembly's decree removing the right to a monopoly in hunting was poorly understood by the multitude who, perhaps, did not want to understand it." He pictures the rural population as going way beyond the Assembly's intentions like a flood at "the opening of a dike holding back the waters." Far to Bailly's right, the reactionary count de Montlosier agreed that it was elite action—particularly the enactments of the National Assembly—that had encouraged popular revolt. Commenting on the intralite debates at the end of the old order, he observed that if the elite wished to effectively manage to conserve "this collection of ancient debris"—such is his characterization of the Old Regime—it would have had to be done quietly: "When you are in Alpine passes subject to avalanches, it's good advice to avoid any noise" (Montlosier 1830, 1:160). He goes on to characterize the work of the National Assembly in August 1789 as first encouraging crimes, then "hurrying to regularize these crimes by registering them as laws" (1:235).

Following the declarations of August 1789, antiseigneurial events became the central form of peasant action and remained so for the next three years. During periods of relative peace, between one-third and one-half of insurrections had an antiseigneurial element. But at moments of more intensive mobilization, the antiseigneurial element became overwhelming. In the wave of insurrection of late 1789–early 1790, 78% of events had an antiseigneurial aspect (and in the peak month of January, 87%). In the lesser peaks of June 1790 and June 1791, antiseigneurial events still made up an impressive two-thirds of insurrections. Among peak times in the three years that followed the legislative breakthrough of August 1789, the one with the lowest antiseigneurial propensity is the "mere" half of all events during the great wave of February–April 1792. Even in this wave, on the peak day of February 5, 91% of events targeted the lords.

*Peasant antiseigneurialism in decline.*—After August 1792, antiseigneurial events underwent a decline as precipitous as their earlier rise. In the peak period of early fall, the antiseigneurial component fell back to 36%, comparable to the spring of 1789; beyond that point, peak periods of peasant insurrection were dominated by other issues, and quieter moments also had few antiseigneurial events. The cause of the sudden decline is not far to seek. After several years of tinkering with the initial antiseigneurial legislation (e.g., by shifting rights from the "indemnified" to the "abolish without indemnification" category), the legislature enacted a radically different measure on August 25, 1792, that made all remaining seigneurial rights presumptively illegitimate, that is, suitable for outright abolition unless a lord had evidence that a particular right derived from a voluntary contract. Among lords who actually might have had documentary evidence (and many did not), some had seen their documents perish

in peasant attacks; for those who still had such papers in their possession, however, the invitation to initiate a lawsuit before a revolutionary judge in a climate far more radical than three years earlier could have hardly been very enticing, especially with tensions running high because of the war that had just begun (and begun badly). Although the legislature had not yet declared all seigneurial rights over and was not to do so until the following summer, the new legislative structure made it virtually impossible for any lord to collect anything.

So the peasants ceased their attacks on the lords because there was little to be gained by further disruption. It is worth noting that the smaller group of antiseigneurial actions that nonetheless followed this new legislation is distinctive for its level of destructiveness. Among those antiseigneurial actions of the period August 26–October 31, 1792, 81% involved attacks on or penetration of the châteaux, the highest rate of such incidents in the entire data set. I venture the hypothesis that in the wake of the legislation that made production of relevant evidence in court the lord's only route to collection, some rural militants endeavored to seize and destroy the lord's papers, sometimes destroying the house in the process.

*A note on the forms of peasant action.*—These last reflections on the latest wave of attacks on the châteaux point up the degree to which French villagers were responding to the actions of local and national elites, not only in the overall ebb and flow of mobilizations and in the choice of targets but in the choice of forms of action as well. We have looked at the way in which the law of August 25, 1792, may well have encouraged those peasants who still thought it worth their while to mount antiseigneurial actions to destroy the lord's documents. Let us go back to the initial legislative structure: in March and May 1790, the National Assembly not only set the terms of indemnification but placed the burden of proof in disputed cases—whether a particular right was to be regarded as coerced and hence abolished without compensation—on the peasants (as it was later to shift it onto the lords). Villagers now had an interest in obtaining documentary evidence refuting claims that a lord had a legitimate right to collect. In the wake of this legislation, there was a very sharp rise in the frequency with which antiseigneurial events included forcing the lord to renounce his rights, often in the form of a public declaration written down by a notary. Such actions would put a relevant document into the hands of a peasant community. From March 1789 through March 1790, the proportion of events involving such an action hovered between 8% and 10%; in April and May, it jumped to 30%. In June, following the setting of the indemnification rates in May, fully half of antiseigneurial events involved such actions. So, early in the Revolution, peasants coerced

renunciations and late in the Revolution burned the lord's home and the documents within when legislative action redefined the value of such pieces of paper.

### Legislators Respond to Peasants

If villagers were tailoring their challenges to France's legislators with an eye on the opportunity presented by the elite's antiseigneurial program to push those elites to move much further and faster, the legislators in turn were responding to peasants. Moments of intense peasant mobilization paid off in antiseigneurial legislative action. Let us follow the peaks of peasant insurrection from the formation of the National Assembly (see fig. 1 above). The great peak of July 1789 was followed almost instantly by the declaration in early August that proclaimed the abolition of the feudal regime. The peak day of rural insurrection was July 27; allowing several days for news to travel, we can see that a debate that started on August 4 was a very quick response. The intensive debates of August 4–11 were permeated by an awareness of the rural upheaval (Kessel 1969). As a single instance among many, consider count de Virieu, soon to become a noted reactionary, who cheerfully joined in the renunciation of seigneurial rights. When count de Montlosier asked him to explain his astonishing behavior, he responded that "when the people are delirious, there are only two ways of calming them: generosity and force. We had no force" (Montlosier 1830, 1:239–40).

The Committee on Feudal Rights took its time sorting rights into those to be indemnified and those to be simply abolished, pondering what to do about disputed rights, considering the modalities and rates of indemnification, and developing an intellectual rationale for the whole structure. But when the major new wave of peasant insurrection of January–February 1790 broke out, the Committee reacted at once. This insurrectionary wave peaked on January 24. On February 8, the Committee of Feudal Rights presented a preliminary report.

The Committee's chair virtually defined the legislation as an effort to meet peasant demands without further alienating the ex-lords: "It is necessary to give the people a law whose justice will force into silence the egotistical feudatory who, for the last six months, has been screaming so indecently about spoliation, and a law whose wisdom may return to the course of duty the cultivator who has, for the moment, strayed out of resentment of long oppression" (Mavidal and Laurent 1879, 11:498–99).

A smaller wave of rural actions with a significant antiseigneurial component broke out in June 1790; on June 19, the National Assembly voted to abolish all hereditary distinctions including coats of arms and distinc-



tive liveries (Mavidal and Laurent 1879, 16:378). The abolition of the symbols that made one man more than another had been critical matters in the *cahiers* of the Third Estate but were of far less note in the villages, whose *cahiers* had a much greater focus on material burdens. For the losers—the nobility—however, this was a very serious matter and occasioned the very greatest bitterness. In the retrospective analysis of marquis de Ferrières, indeed, it was this measure that led the old elites to bury their differences and unite against the Revolution (Ferrières 1821, p. 76). The same marquis was painfully aware of the rural force behind the legislative deed. In his letters home, he urged nonresistance to the distressing law because defiance “may become the signal for a general massacre of provincial nobles and for the burning of the *châteaux*. There is no public force able to protect them” (Ferrières 1932, p. 208). As strong as the sentiment among many in the National Assembly must have been on these issues, to judge by the Third Estate *cahiers*, they waited for a peasant insurrection before pushing this legislation so repugnant to a considerable number of their noble colleagues.

When we recall how little the parish *cahiers* were concerned with such matters, we are tempted to see this as an instance of plebeians getting an elite to take notice, after which that elite uses that situation for its own initiatives. We could speak of the greater success of movements in getting issues on agendas than in realizing programs. Peasants may have compelled an antiseigneurial action by the legislature—but a part of the legislature used the occasion to push its own specific program. Perhaps the lesson is that relatively small episodes of disruption like June 1790 are not especially well rewarded. A year later the similarly small spurt of antiseigneurial events of June 1791 did not even trigger any new antiseigneurial legislation.

The major insurrectionary wave of February–April 1792 was markedly antiseigneurial and led the legislature to begin seriously rethinking the whole idea of a gradual buyout. On February 29, as reports were drifting in of mounting rural turbulence in southern France, Couthon pointed out that the elimination of the symbols of deference was plainly not enough to pacify the countryside: “It is not exactly the honorific aspects of the feudal regime that weigh on the people” (Couthon 1792, p. 3).

Peasant insurrection continued and other legislators took up the issue. On March 12, Golzart argued that the Legislative Assembly—the National Assembly’s successor—“must finally convince the people of the countryside that the abolition of the feudal regime is not an almost worthless benefit” (Mavidal and Laurent 1879, 39:565). As the violence went on, the Feudal Committee was invited to propose a new legislative course. My data show April 5 to be the peak of this second greatest wave of

rural insurrection and overwhelmingly antiseigneurial (91% of that day's events); six days later, as the bad news was reaching Paris, the committee's report conceded the bankruptcy of the National Assembly's policies (Mavidal and Laurent 1879, 41:470–74). A new draft law, radically shifting the burden of proof on some of the seigneurial rights was prepared by April 20. The peasant movement now eased off; perhaps that relaxation of pressure explains the piecemeal and somewhat dilatory character of the adoption of the new principles in the form of several distinct pieces of legislation in June and August, culminating in the August 25 law that declared all seigneurial dues presumptively illegitimate and therefore abolished outright unless a lord could produce evidence of the voluntary character of an agreement.

The antiseigneurial movement had won in the sense that peasants could no longer, in practice, be compelled to pay, and antiseigneurial actions soon fell off sharply. But there is one final piece of legislation to account for, the abolition of all payments to the lord, in principle, that was enacted in the summer of 1793. Rural upheaval did not end with the falling off of antiseigneurial actions in late 1792. In the wave of unrest of August–September, counterrevolutionary events had risen to prominence; indeed on the peak day of that wave, more than one-third of all events could well be labeled counterrevolutionary. In November 1792, there was yet another wave of troubles, this time overwhelmingly composed of subsistence events (77%). And in March 1793, the simmering counterrevolution exploded in western France—87% of disturbances that month had a counterrevolutionary character.

Recognizing the multifarious nature of rural turbulence, Bertrand Barère, on March 18, called for a multifaceted package of policies (Mavidal and Laurent 1879, 60:290–94). After several more politically tumultuous months, a diversity of measures on land sales and landownership were passed and, along with them, on July 17, 1793, the definitive abolition of seigneurial rights.

At every step, then, we see the legislators responding to continuing rural defiance, not just realizing their own program. Their own program? Perhaps this is already a distorted simplification. Look again at figure 1. The very documents that we have taken as the starting point for understanding the thinking of the assemblies that elected delegates to the Estates General were meeting at a moment of rapidly rising rural insurrection, already beginning to show some shift toward the choice of seigneurial targets; although the urban delegates dominated those assemblies, decisions were nevertheless taken in the presence of and with the participation of delegates from rural communities. The antiseigneurial programs expressed in the Third Estate *cahiers* in the spring of 1789 already reflect the rural pressure.

## Legislators Talk about Rural Revolt

Deputies made rhetorical use of the rural disturbances to press particular political agendas, some with little bearing on the agendas of the insurgent countryside. On July 25, 1789, a deputy from Franche-Comté urged the National Assembly to see its disorders as stemming from popular desire to abolish that province's Parlement (Creuzé-Latouche 1946, p. 278). Count d'Agoult thought that the left wanted to prevent any effective pacification "because if the troubles cease, they will lose their influence" (Egret 1950, p. 106). Adrien Duquesnoy suggested that a peaceful England could double the rate of taxation that is feasible in France (Duquesnoy 1894, 2:417–18; see also 2:441). Jérôme Pétion used the rising of early February 1790 to attack primogeniture, blamed for dividing the countryside into "proletarians and colossal properties" (Lameth 1828, p. 366). In the course of another discussion of the same wave of disturbances, one deputy pointed to the insurrections as the reason for completing the new constitution (Mavidal and Laurent 1879, 11:49).

The troubles of November 1792 provided another occasion that could be pointed to on behalf of a radical vision. Although the data show the disturbances that month to be overwhelmingly over subsistence, when Robespierre rose to speak on November 30, rural unrest became an argument for accelerating the lagging trial of the king (Robespierre 1950, 9: 106–9).

Conservatives, like radicals, could point to rural troubles in making a case for their own concerns. In the course of the insurrectionary wave of the winter of 1789–90, Duquesnoy noted with amusement and irritation those who exaggerated the threat of peasant movements to try to scare deputies into placing more military force under the king's command (Duquesnoy 1894, 2:413–14, 418–19, 422–23).

As a final instance of putting peasant insurrection into the rhetorical service of an elite political agenda, consider Barère's magnificent performance in the Convention—the legislative body that succeeded the Legislative Assembly—on March 18, 1793. Counterrevolution was at its height. Barère paints a tableau in which "passions, intrigues, and divisions" among the revolutionary leadership, foreign enemies, émigré nobles, priests, and peasant "fanatics" in the west reinforce one another and encourage poorer peasants to attempt to seize the properties of the wealthy. The rural slide into "anarchy" can only be halted by stern, repressive measures to reassure the rich and generous measures (access to émigré land, division of the commons, poor relief, progressive taxation) to reassure the poor; to coordinate all this, while dealing simultaneously with divisiveness at the top, Barère urges, as if it were an afterthought, the creation of "a

committee of public safety" (Mavidal and Laurent 1879, 60:290–94). Rural insurrection becomes the justification for beginning to set up the central institution of the Reign of Terror.

#### INTERLINKED CYCLES OF PROTEST AND REFORM

Insurrectionary peasants and varying legislative factions presented opportunities for one another. As peasants discovered in the spring of 1789 that the delegates-to-be favored significant, if partial, dismantling of the seigneurial regime, peasants began to shift toward more antiseigneurial action. As the legislators in early August claimed a ringing commitment to the abolition of feudalism in its entirety, while leaving many peasant burdens in force, insurrectionary peasants came to make their attack on seigneurialism their most common action. Indeed, new groups of peasants entered the fray. Peasants from impoverished regions in south central and southwestern France, who had not mobilized early on, began to enter the insurrectionary arena at the tail end of 1789, presenting a fine example of a cautious group who only mobilize after others have demonstrated the possibility and legislative action has suggested some chance of success. Once these southern sharecroppers entered the fray, however, they proved almost impossible for the revolutionary government to placate, short of actually dismantling their own initial attempt at a compromise. With success—the legislation of August 25, 1792—peasant actions against the lords fell off rapidly.

On the elite side, their *cahiers* show an initial receptiveness to some significant measure of change in seigneurial rights. Counterrevolution or land redistribution were not greeted with anything like the same propensity to further concession as was peasant antiseigneurialism. If the peasants were not kicking in an open door, as the French put it, when they continued to rise against their former lords, they were not kicking in a firmly locked and stoutly defended door of solid oak either. The elites opened the door halfway, then squabbled about whether to open it further or take firm action against the besiegers; in response the country people kicked the door wide open, in fact, kicked it off its hinges.

The antiseigneurial program of a portion of the legislators made it easier for them to respond to any sort of peasant pressure with antiseigneurial concessions. They made a major step in early August 1789 in response to the risings of July, which were not even overwhelmingly antiseigneurial. Following a series of continued rural insurrections from the fall of 1792 into the spring of 1793, they terminated seigneurial rights in principle as well as practice, even though peasants were no longer pushing them on this issue. When peasants mobilized, legislators made concessions on seigneurial rights—even when peasants had other targets.

More generally, legislators attempted to define peasant insurrections for their own purposes. Thus peasants could get seigneurial rights on the agenda, but they, on their own, could not secure a meaningful abolition in its entirety. Consider the major legislative turning points: the initial declaration of August 4–11, 1789, the detailed legislation of March and May 1790, the radical redefinition of the burden of proof in disputed cases of August 1792, and the simple abolition of all remaining seigneurial rights in July 1793. Each of these turning points but one pushed the abolition of seigneurial rights further along: the exception was the legislation of March and May 1790, which provided a quite detailed and conservative interpretation of the ringing principles of the previous August. Each of these turning points was a response to peasant insurrection; the first and last of these legislative actions, however, responds to insurrections in which seigneurial rights were not the dominant form of peasant action. Whenever peasants did anything troublesome, they stood to gain concessions on seigneurial rights. (It was this legislative habit that convinced peasants that if they doggedly insisted on a real abolition, they might get it.)

Now let us also note that every one of these turning points but one follows hard upon, not only peasant insurrection, but major risings in Paris itself, risings that drastically altered the political scene. In mid-July 1789, Parisian popular forces, in mobilizing to take the Bastille, saved the National Assembly and the cause of the Third Estate from the threat of a military deployment and dissolution of the assembly; in August 1792, Parisian popular forces and National Guard units pulled down the king and brought the Legislative Assembly's history to an end; in late May and early June 1793, Parisian militants invaded the Convention and forced the purge of its Girondist members, leaving a more radicalized rump convention to govern. In these three cases, where peasant action was coupled with urban popular action that altered the political situation of the legislature, not only were seigneurial rights placed on the legislature's agenda, but the legislature acted to advance their destruction. The rural uprisings of the winter of 1789–90, however, were not complemented by urban popular militance: the legislators responded, to be sure, to a peasant-induced agenda, but this time they detailed modalities of payment that limited the promises of August 4–11. So while legislative groups used peasant actions for their own agendas, at moments they were also responding to other sorts of pressures. Without that tacit urban alliance, the peasants obtained a good deal less.

We could track the play of pressure and opportunity in the multiplex dialogue of legislators and villagers into many other areas: Why was the spring and summer of 1792, but not earlier, the point where rural disruption paid off so decisively? What of the ebb and flow of other forms of

conflict (over food supply, over taxation, over land, over wages)? How do we explain the varying geography of rural revolt during the three years when antiseigneurial struggles were dominant? Not only does space forbid a pursuit of these questions, but we already have plenty of material to reflect upon here.

#### LESSONS FOR THE STUDY OF SOCIAL MOVEMENTS

First of all, accounts of revolutionary action in terms of long-standing or slow-changing structures that realign the interests and resources of various social strata may be essential to understand the concerns and options that people take into revolutionary situations, but they may be quite inadequate in accounting for the options actually chosen. Among the many sorts of burdens known to France's peasants and the many arenas of institutional change on the minds of revolutionary legislators, the mutual concentration on the abolition of the feudal regime was not given at the outset. France's peasants had concentrated on other targets in most of their earlier experience of insurrection and entered the Revolution mobilizing around subsistence issues. In their grievances, expressed early, both France's country people and its urban elites devoted far more attention to taxation. Although the *cahiers* attack the seigneurial system, in those documents, "the feudal regime" does not carry the full weight and significance that that expression was to have only a few months down the road. The peasant-bourgeois alliance against feudalism, central to both Marxian and Tocquevillean understandings of the Revolution, was simply not there at the beginning of the Revolution, the product of centuries of French history; the alliance was forged. The Revolution was a process.

Second, the dialogue of revolutionary villagers and revolutionary legislators was not a debate resulting in compromise but a convergent radicalization on both sides. We need to do a lot more thinking than is found in the literature on the varying ways such dialogues can unfold. We need to be exploring the dynamic character of social movements. Theories that stress the strategic deployment of resources are inadequate to the task of understanding such dynamics (although they do a part of the job); resources after all are deployed to some purpose, and a sense of efficacy is a major element in choosing among possible purposes.

Third, we cannot take the study of grassroots mobilizations and the study of the politics of power holders as best left to their respective specialists. Here, as elsewhere, the devil is in the details: we need studies that treat with equal seriousness both parties to such dialogues. If students of social movements treat mobilization as some function of available resources, expressive actions, and cultures of solidarity and leave to others the question of what impact, if any, movement actions have on power

holders, then an absolutely essential context for understanding mobilization, expression, and solidarity as dynamic processes will be omitted. If students of power holders treat policy formation as some function of elite political culture, the organizational structure of decision making and the conflicts of power holder coalitions, an essential context of policy-making—namely the impact of policy on potentially disruptive movements and the impact of disruption on policy—gets omitted. In the instance at hand, we have found helpful such notions as Tilly's (1988) conception of movement as interaction, Tarrow's (1991) sense of linked cycles of protest and reform, and McAdam's (1982) stress on political opportunity structures (at least so long as one thinks about the opportunities movements present to elites as well as those elites present to movements).

Fourth, and as a corollary of the previous points, if we are to seize the moments of social creativity, we need to study the messy details of historical processes and not just the grand trends. If sociologists seek to limit their inquiries to the grand trends and leave the details to others, they will often doom their conclusions to irrelevance, for the critical transformations and turning points can sometimes only be understood much closer up. It was not the work of the centuries that produced an antifeudalism that was more than words in revolutionary France but a series of decisions over a few, short years in thousands of villages and at the heights of authority as villagers and lawmakers faced situations they had never before experienced. We need to look not just at long-term evolutionary processes but at intensified moments of short-term evolution—and be sensitive to the possibility that, as in the case at hand, there will be much explanatory power in thinking about short-term coevolutions. It was not the work of the centuries that ended France's seigneurial regime but the work of the months. It was the mutual adaptations and seizures of opportunity by legislators and country people over a short stretch of time that ended the seigneurial regime. The "abolition of feudalism" was not accomplished as the working out of an elite program nor as the risings of popular forces, alone; it was the dialogue of the center of power and the periphery, of elites and plebeians, of the palace and the village that made history happen.

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## Commentary and Debate

To conserve space for the publication of original contributions to scholarship, the comments in this section must be limited to brief critiques. They are expected to address specific errors or flaws in articles and reviews published in the *AJS*. Comments on articles are not to exceed 1,500 words, those on reviews 750 words. Longer or less narrowly focused critiques should be submitted as articles. Authors of articles and reviews are invited to reply to comments, keeping their replies to the length of the specific comment. The *AJS* does not publish commenters' rebuttals to authors' replies. We reserve the right to reject inappropriate or excessively minor comments.

### SAME DATA, DIFFERENT CONCLUSIONS: COMMENT ON BERNHARDT ET AL.<sup>1</sup>

In "Women's Gains or Men's Losses? A Closer Look at the Shrinking Gender Gap in Earnings" (*AJS* 101 [September 1995]: 302–28), Annette Bernhardt, Martina Morris, and Mark S. Handcock make an important contribution to understanding recent changes in the gender earnings ratio by expanding the scope of research to look at the full distribution of men's and women's earnings and not just at what is happening at the median. Among a variety of interesting results, all of the authors' conclusions rest on the central description of where the changes in the earnings distribution occur. We find this conclusion particularly problematic.

The authors conclude that "while the median ratios suggested that women made real progress over the past two decades, the relative distributions make clear that this progress was limited to women at the bottom of the earnings distribution" (p. 314). This argument is not tangential to their thesis but is made throughout their article. For example, they infer that women's earnings gains in the 1980s "occurred largely at the bottom of the relative earnings scale" (p. 314). Later, Bernhardt et al. claim that "the inroads that women made at the higher earnings levels were small and played a weaker role" (p. 324).

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We were surprised at their main conclusion since we had reached almost the exact opposite conclusion based on the same data but with somewhat different methods. We present data below that demonstrate that the increased earnings of women in the last two decades have been remarkably broad based. Not only have women moved up from the bottom of the earnings distribution, they have gained access to the top as well.

To some extent, our disagreement hinges on the interpretation of the word "largely" in determining where women made their greatest gains. Under one interpretation, Bernhardt et al. are correct that the largest number of women who made gains were at the bottom of the earnings distribution; that is, after all, where most women have been located so that is also the location of most of the progress. However, that should not imply that women have not also gained at the top of the earnings distribution nor that the gains at the top have been any less rapid. Further, this should not be misconstrued as a question of semantics. We think that Bernhardt et al.'s conclusion needs at least some reservations, if it is not to be misleading.

Finally, to describe the changes in women's earnings, Bernhardt et al. investigate only absolute levels of variation. We believe that they should examine the rates of change within each decile of the earnings distribution in order to assess where women have improved their relative earnings across the distribution. We undertake such an investigation in this paper.

### Data

Like Bernhardt et al., we use the March uniform series of the Current Population Survey (CPS; see Mare and Winship 1990; U.S. Bureau of the Census 1989-94). We begin with the 1963 earnings data (from the 1964 CPS) and supplement those data with more recent data from 1988 through 1993. As do Bernhardt et al., we restrict our sample to nonfarm workers 16-65 years old who are not in school or in the military and who worked full-time, year round. Bernhardt et al. reallocate earnings around points in the distribution where respondents tend to "heap" their responses (e.g., at \$10,000). We have not followed this practice, so our trend lines are perhaps more uneven, but the long-term trends would not seem to be much affected by this refinement. We have also not imputed earnings at the top-coded category since the specific value assigned to the top-coded category does not affect the percentile distributions.

The Census Bureau changed processing procedures several times during this series (see Welniak 1990). The most important change occurred in the 1976 CPS when improvements in the imputation of full-time status resulted in many fewer of the workers in our full-time/year-round sample with very low earnings. This change raised men's distributions even more

than women's so there are noticeable discontinuities at 1976 especially in the relative distributions at low ends of the earnings spectrum. We have not corrected for these processing changes so these discontinuities will be evident in the results that follow.

## Results

Following Bernhardt et al., we define white male earnings percentiles as the basis for comparing white and black women's earnings distributions. For clarity, we focus only on the tenth, thirtieth, fiftieth (median), seventieth, and ninetieth percentiles. Table 1 reports the percentages of women whose earnings are equal to or greater than those reported levels for each year. (See app. table A1 for the white male earnings at each percentile for each year.) For example, in 1993, 81.67% of white women earned at or above white men's tenth earnings percentile (compared to, by definition, 90% of white men); for black women, 75.95% earned at or above the tenth percentile (again, compared to 90% of white men). The closer the percentages for the women come to the percentages of the white men at or above each percentile, the closer to parity are the relative earnings of women and men.

As shown in table 1, since the late 1970s, both white and black women's representation has generally increased at all levels. If we confine our attention to the period 1980–93, the percentage of white women who earn at or above men's tenth percentile has grown from 66.30% to 81.67%, a gain of 15.37 percentage points. Black women experienced a similar increase of 15.00 percentage points during this period; that is, whereas 60.95% of black women in the 1980 CPS earned at or above the bottom 10% of white men, this number increased to 75.95% in 1993. At white men's thirtieth percentile, the growth for white women has been from 26.68% to 50.55%, a 23.87 percentage point gain; black women's representation at this level changed from 22.93% to 41.51%, an increase of 18.58 percentage points. Across all five percentiles presented in table 1, white and black women consistently increase their representation over time.

At the top end of earnings, white men's ninetieth percentile, the gain has been from 0.82% to 1.94% for white women and from 0.38% to 1.06% for black women. These numbers are small and are consistent with Bernhardt et al.'s conclusion that "women earn dramatically less than white men. . . . The great majority of women are concentrated in the lower tail of the men's distribution" (p. 314). However, these findings are not consistent with the conclusion that the "inequity has changed little over time" (p. 314). Not only has there been positive change, the change can be observed at every level of the earnings distribution.

A more difficult question to address is where the change "largely" oc-

TABLE 1  
PERCENTAGE OF WOMEN WHOSE EARNINGS EXCEEDED WHITE MALE EARNINGS

DATE	MALE EARNINGS PERCENTILE				
	Tenth	Thirtieth	Fiftieth	Seventieth	Ninetieth
<b>White women:</b>					
1963	87.18	25.57	9.31	3.75	.67
1964	85.95	24.77	9.58	3.35	.65
1965	87.97	25.66	8.40	3.39	.41
1966	84.90	23.41	8.80	3.44	.62
1967	86.82	25.40	9.93	3.27	.61
1968	87.92	24.38	8.79	3.11	.45
1969	86.31	22.96	9.17	3.46	.56
1970	83.58	25.62	10.16	4.21	.72
1971	84.61	25.73	10.35	3.96	.64
1972	85.54	26.06	10.98	3.65	.67
1973	83.68	26.02	10.31	3.75	.59
1974	85.53	27.25	10.03	3.91	.47
1975	58.74	22.08	9.70	3.54	.58
1976	61.96	23.07	9.52	3.28	.71
1977	61.96	23.06	9.68	3.04	.63
1978	64.25	27.70	10.17	3.73	.60
1979	64.17	24.76	9.66	3.73	.75
1980	66.30	26.68	10.89	3.72	.82
1981	68.41	29.26	11.80	4.47	.63
1982	72.82	34.22	13.43	5.01	.95
1983	76.26	34.33	14.35	5.27	.97
1984	74.25	34.44	15.82	5.46	1.11
1985	75.66	39.89	17.09	5.93	1.15
1986	76.65	38.64	17.08	6.51	1.51
1987	76.37	38.65	17.78	7.73	1.44
1988	77.77	42.27	19.62	7.96	1.47
1989	79.69	41.79	20.42	8.35	1.48
1990	78.71	44.89	22.96	9.22	1.74
1991	79.22	42.91	24.35	9.92	1.89
1992	80.82	46.85	23.52	10.04	1.98
1993	81.67	50.55	26.17	10.94	1.94
<b>Black women:</b>					
1963	62.60	9.16	2.34	.74	.00
1964	71.29	10.66	3.33	1.54	.35
1965	77.00	12.35	2.25	1.18	.00
1966	67.31	11.41	4.46	1.68	.00
1967	75.63	14.72	4.94	1.59	.09
1968	79.68	14.42	5.72	1.16	.00
1969	76.79	14.41	5.07	2.32	.09
1970	73.45	17.92	7.70	2.86	.22
1971	77.09	20.66	8.35	2.36	.35
1972	78.83	19.62	8.33	2.65	.52
1973	76.94	18.38	7.54	2.40	.13

TABLE 1

(Continued)

DATE	MALE EARNINGS PERCENTILE				
	Tenth	Thirtieth	Fiftieth	Seventieth	Ninetieth
1974	81.95	23.77	7.37	2.23	.38
1975	55.11	17.70	6.22	2.01	.12
1976	57.14	20.20	8.12	2.53	.14
1977	55.80	21.14	8.48	2.27	.53
1978	59.37	23.87	9.39	2.21	.37
1979	57.87	23.67	8.75	3.20	.16
1980	60.95	22.93	7.53	2.39	.38
1981	60.71	21.42	7.63	2.72	.28
1982	69.37	27.40	9.83	3.09	.33
1983	71.05	28.17	11.16	3.20	.40
1984	68.36	29.95	12.64	3.47	.37
1985	68.76	32.25	13.26	3.15	.38
1986	69.29	31.03	12.92	3.97	.85
1987	72.35	32.24	12.95	3.58	.48
1988	70.76	36.05	13.53	4.26	.73
1989	74.84	37.39	16.06	5.62	.71
1990	72.83	38.37	17.26	5.48	.92
1991	73.08	35.66	17.49	5.86	.90
1992	75.08	40.04	18.16	6.57	.30
1993	75.95	41.51	19.05	7.01	1.06

curred. For example, in absolute terms, the increase for white women of 23.87 percentage points at the thirtieth percentile is much larger than the 1.12 percentage point increase at the ninetieth percentile. Many fewer white women moved into the top white male decile than up to the third decile. But the increase from 0.82% to 1.94% for white women at the ninetieth percentile is quite dramatic in relative terms. The representation of white women at the top decile more than doubled during these 13 years. Similarly, the increase for black women from 0.38% to 1.06% more than doubled their representation at the top. Both are still a long way from equality (when 10% of all women would earn at white men's ninetieth percentile), but the changes are steady and consistent throughout the last 20 years. (The data for black women in the top decile fluctuate more around the trend line but the fluctuations derive from the small numbers in those cells; the trend is still positive.) Thus, it is reasonable to conclude that the gains made by women at the upper end of the earnings distribution were as great as those at the lower end and that this is true for both white and black women.

One common way of comparing the pace of percentage changes that

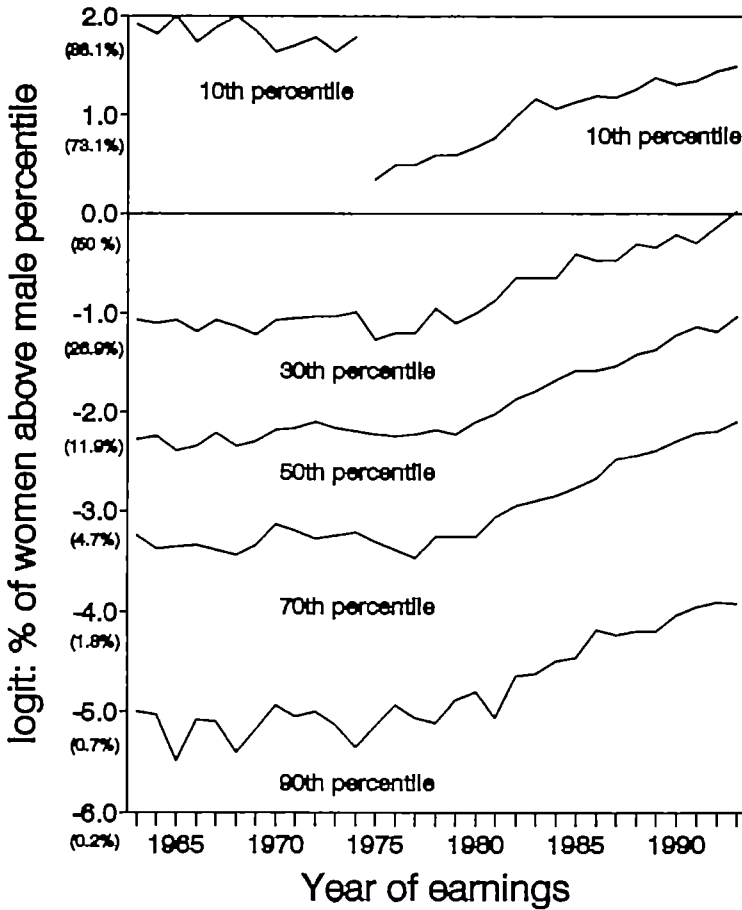


FIG. 1.—White women's access to white male earnings percentiles

begin at very different absolute levels is to transform the percentages into a continuous metric that is not bounded by 0% and 100%. The logistic transformation ( $\ln[p/(1 - p)]$ ) is most frequently used for this purpose and is familiar from log-linear models and logistic regressions. Figures 1 and 2 illustrate these transformed percentages for white and black women respectively. The figures illustrate the parallel nature of the changes at every level of the earnings distribution. For white women (fig. 1), all of the lines fluctuate around a steady state from the beginning of the series until some point in the mid to late 1970s (with the exception of the previously noted discontinuity from the 1976 CPS). Then they all turn upward and increase steadily throughout the remaining years. Figure 2 shows that



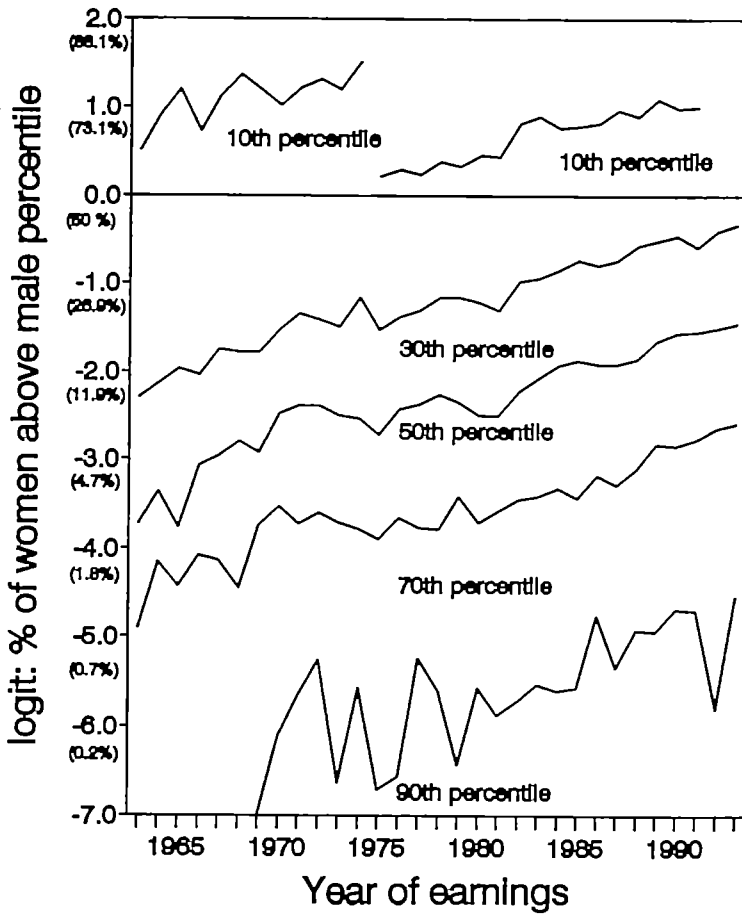


FIG. 2.—Black women's access to white male earnings percentiles

for black women there have been steady increases throughout all 30 years. The patterns of change demonstrated by the lines in figures 1 and 2 confirm our suspicion that women improved their relative earnings across the earnings distribution and not just at the bottom end of the distribution.

To demonstrate this point more precisely, we have calculated the average growth rates since 1980 for each of the series in figures 1 and 2 by regressing the logit values on year (logit values for both figures are found in app. table A2). These regression results are presented in the first column of table 2. The slope coefficients for white women range from .0527 at the tenth percentile to a high of .0854 at the seventieth percentile; for black women, they range from a low of .0457 at the tenth percentile to a high

TABLE 2  
RATES OF WOMEN'S ACCESS TO WHITE MALE EARNINGS

Percentile	1980-93	1980-87	1987-93
White women:			
Tenth . . . . .	.0527	.0702	.0467
Thirtieth . . . . .	.0660	.0795	.0668
Fiftieth . . . . .	.0785	.0848	.0775
Seventieth . . . . .	.0854	.0950	.0660
Ninetieth . . . . .	.0806	.1085	.0630
Black women:			
Tenth . . . . .	.0457	.0630	.0325
Thirtieth . . . . .	.0661	.0783	.0523
Fiftieth . . . . .	.0784	.0988	.0778
Seventieth . . . . .	.0837	.0597	.1102
Ninetieth . . . . .	.0743	.0901	.0302

NOTE.—Coefficients are annual increases in logged odds of women being at or above the white male earnings percentiles.

of .0837 at the seventieth percentile. The highest rates of growth are at the *upper* percentiles and not at the lower percentiles as Bernhardt et al. would have us conclude. However, the differences in slopes across deciles are small for both white and black women; therefore, the best conclusion might be that the rates of increase in women's access to higher earnings are quite consistent across the earnings distribution.

Columns 2 and 3 of table 2 divide the 1980-93 period into two subperiods, breaking the series at 1987 where Bernhardt et al.'s data end. The consistency of increases can be seen just as clearly for the 1980-87 subperiod. It is interesting that most of the increases slow down in the subsequent period (col. 3), exactly as Bernhardt et al. predict. But even these slower rates of access are quite consistent across all levels of the white male earnings distribution.

So, the conclusion that women's gains have been limited to women at the bottom of the distribution is not well supported by these data. White and black women have gained throughout the earnings distribution, and the rate of progress has been similar at all levels. Of course, since most women started at the bottom of the distribution, that is where most women have gained. Only in this limited sense have the gains "largely" been observed at the bottom of the distribution. But there have been gains at the middle and at the top as well. Indeed, the gains by women at the top are particularly noteworthy given that the top white male deciles are also experiencing the highest earnings increases over time (see table A1). The women at the top are catching up with a "moving target," whereas

the women at the bottom are moving ahead of a standard that has become lower over time. In general, there are still very few women at the top, but the rates of increase at the upper levels are at least as great as anywhere else in the earnings distribution.

DAVID A. COTTER

*Union College*

JOANN DEFIORE

*University of Washington—Bothell*

JOAN M. HERMSEN

*University of Maryland*

BRENDA MARSTELLAR KOWALEWSKI

*Weber State University*

REEVE VANNEMAN

*University of Maryland*

## APPENDIX

TABLE A1  
PERCENTILES OF WHITE MALE EARNINGS

DATE	PERCENTILE				
	Tenth	Thirtieth	Fiftieth	Seventieth	Ninetieth
1963 .....	6,005	18,369	23,550	28,456	39,642
1964 .....	5,627	18,616	23,371	29,611	41,498
1965 .....	4,586	19,108	24,841	30,573	42,290
1966 .....	7,426	19,308	25,668	31,561	44,557
1967 .....	5,761	19,443	25,204	32,405	43,927
1968 .....	5,710	20,801	26,168	33,143	46,802
1969 .....	8,625	21,794	27,865	34,831	49,759
1970 .....	9,532	22,153	28,482	34,811	50,001
1971 .....	9,097	21,670	28,102	35,177	50,339
1972 .....	9,420	22,466	29,437	36,502	52,986
1973 .....	9,706	22,429	29,629	37,382	53,443
1974 .....	8,814	21,657	28,709	36,264	50,870
1975 .....	15,233	23,256	30,233	37,210	54,589
1976 .....	15,402	23,984	30,805	38,506	55,008
1977 .....	15,138	23,989	31,021	39,293	55,837
1978 .....	15,103	23,236	30,981	38,726	58,089
1979 .....	15,013	23,314	31,439	38,857	56,812
1980 .....	14,306	23,186	30,204	38,614	55,583
1981 .....	14,506	22,339	29,379	37,716	56,574
1982 .....	13,672	21,874	30,077	38,280	57,421
1983 .....	13,122	22,308	29,888	39,367	58,329
1984 .....	12,718	22,615	30,191	38,620	58,494
1985 .....	13,012	21,864	30,367	39,291	60,734
1986 .....	13,118	22,658	31,005	40,546	59,626
1987 .....	13,231	23,011	30,781	40,268	59,827
1988 .....	13,258	22,096	30,383	39,773	59,660
1989 .....	12,648	21,560	30,356	40,053	63,242
1990 .....	12,480	20,910	29,000	39,000	60,000
1991 .....	12,356	21,112	28,789	38,385	58,109
1992 .....	12,110	20,495	28,413	38,195	60,552
1993 .....	11,758	19,899	27,587	37,989	60,601

NOTE.—Amounts given are in 1990 U.S. dollars

TABLE A2  
LOGITS OF WOMEN'S EARNINGS

DATE	MALE EARNINGS PERCENTILE				
	Tenth	Thirtieth	Fiftieth	Seventieth	Ninetieth
<b>White women:</b>					
1963 .....	1.92	-1.07	-2.28	-3.25	-5.00
1964 .....	1.81	-1.11	-2.24	-3.36	-5.03
1965 .....	1.99	-1.06	-2.39	-3.35	-5.49
1966 .....	1.73	-1.19	-2.34	-3.33	-5.08
1967 .....	1.89	-1.08	-2.21	-3.39	-5.09
1968 .....	1.98	-1.13	-2.34	-3.44	-5.40
1969 .....	1.84	-1.21	-2.29	-3.33	-5.18
1970 .....	1.63	-1.07	-2.18	-3.12	-4.93
1971 .....	1.70	-1.06	-2.16	-3.19	-5.05
1972 .....	1.78	-1.04	-2.09	-3.27	-5.00
1973 .....	1.63	-1.04	-2.16	-3.25	-5.13
1974 .....	1.78	-.98	-2.19	-3.20	-5.36
1975 .....	.35	-1.26	-2.23	-3.31	-5.14
1976 .....	.49	-1.20	-2.25	-3.38	-4.94
1977 .....	.49	-1.20	-2.23	-3.46	-5.06
1978 .....	.59	-.96	-2.18	-3.25	-5.11
1979 .....	.58	-1.11	-2.24	-3.25	-4.89
1980 .....	.68	-1.01	-2.10	-3.25	-4.80
1981 .....	.77	-.88	-2.01	-3.06	-5.06
1982 .....	.99	-.65	-1.86	-2.94	-4.65
1983 .....	1.17	-.65	-1.79	-2.89	-4.63
1984 .....	1.06	-.64	-1.67	-2.85	-4.49
1985 .....	1.13	-.41	-1.58	-2.76	-4.45
1986 .....	1.19	-.46	-1.58	-2.66	-4.18
1987 .....	1.17	-.46	-1.53	-2.48	-4.23
1988 .....	1.25	-.31	-1.41	-2.45	-4.21
1989 .....	1.37	-.33	-1.36	-2.40	-4.20
1990 .....	1.31	-.21	-1.21	-2.29	-4.03
1991 .....	1.34	-.29	-1.13	-2.21	-3.95
1992 .....	1.44	-.13	-1.18	-2.19	-3.90
1993 .....	1.49	.02	-1.04	-2.10	-3.92
<b>Black women:</b>					
1963 .....	.52	-2.29	-3.73	-4.90	
1964 .....	.91	-2.13	-3.37	-4.16	-5.65
1965 .....	1.21	-1.96	-3.77	-4.43	
1966 .....	.72	-2.05	-3.06	-4.07	
1967 .....	1.13	-1.76	-2.96	-4.13	-7.01
1968 .....	1.37	-1.78	-2.80	-4.45	
1969 .....	1.20	-1.78	-2.93	-3.74	-7.01
1970 .....	1.02	-1.52	-2.48	-3.53	-6.12
1971 .....	1.21	-1.35	-2.40	-3.72	-5.65
1972 .....	1.31	-1.41	-2.40	-3.60	-5.25
1973 .....	1.20	-1.49	-2.51	-3.71	-6.64
1974 .....	1.51	-1.17	-2.53	-3.78	-5.57

TABLE A2

(Continued)

DATE	MALE EARNINGS PERCENTILE				
	Tenth	Thirtieth	Fiftieth	Seventieth	Ninetieth
1975	.21	-1.54	-2.71	-3.89	-6.72
1976	.29	-1.37	-2.43	-3.65	-6.57
1977	.23	-1.32	-2.38	-3.76	-5.23
1978	.38	-1.16	-2.27	-3.79	-5.60
1979	.32	-1.17	-2.34	-3.41	-6.44
1980	.45	-1.21	-2.51	-3.71	-5.57
1981	.44	-1.30	-2.49	-3.58	-5.88
1982	.82	-.97	-2.22	-3.45	-5.71
1983	.90	-.94	-2.07	-3.41	-5.52
1984	.77	-.85	-1.93	-3.33	-5.60
1985	.79	-.74	-1.88	-3.43	-5.57
1986	.81	-.80	-1.91	-3.19	-4.76
1987	.96	-.74	-1.91	-3.29	-5.33
1988	.88	-.57	-1.85	-3.11	-4.91
1989	1.09	-.52	-1.65	-2.82	-4.94
1990	.99	-.47	-1.57	-2.85	-4.68
1991	1.00	-.59	-1.55	-2.78	-4.70
1992	1.10	-.40	-1.51	-2.65	-5.81
1993	1.15	-.34	-1.45	-2.59	-4.54

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## PERCENTAGES, ODDS, AND THE MEANING OF INEQUALITY: REPLY TO COTTER ET AL.

Cotter et al.'s comment essentially raises a single question: What matters more for understanding changes in gender earnings inequality, changes in percentages or trends in odds? The percentage changes tell us that over 20% of women made it out of the bottom male earnings decile while only

1% made it into the top decile during the past two decades. At the same time, however, the log odds of making it out of the bottom decile were growing at roughly the same rate as the log odds of making it into the top decile. Which measure is more important? Both statements are empirically true, so they cannot be in conflict. The conflict stems from substantive interpretation and emphasis. The percentage point changes suggest that most of women's gains occurred at the bottom of the men's earnings scale, while the odds suggest that progress was in some sense equal across the earnings scale.

Cotter et al. clearly favor the log-odds measure and the optimistic interpretation it carries. They argue that the percentage point changes represent a "very limited" aspect of the trends in gender inequality and that by focusing on this measure we have painted an unnecessarily bleak picture of women's past and future progress.

We disagree. In research on gender inequality, the core issue is that women's earning power is unequal to men's. Any meaningful definition of progress must therefore track changes in the proportion of women at each level of the men's earnings scale. And it is exactly this fundamental aspect of inequality that is lost by focusing on the log-odds measure. Equal odds of progress at both high and low earnings levels mean little when only 1% of all women have actually experienced the gain at the top earnings level.

We suspect that the authors' insistence on the log-odds measure is driven by an unarticulated and ultimately questionable framework. The fact that the logit trends are similar at all earnings levels seems to be interpreted by them as indicating a *current* equalization of opportunity (because the measure removes the effects of past inequality). But here the authors have not understood the main point of our article. Our decomposition analysis made clear that the gains observed at different parts of the earnings scale were not produced by a single trend, but rather by distinctly different trends, which potentially imply very different labor market dynamics. It is therefore highly doubtful that the similarity in the logit trends has the substantive meaning that the authors would like to ascribe to it.

In short, we maintain that women's recent economic progress has been made largely at the bottom of the men's earnings distribution. As we show below, this is reaffirmed by the inherent limitations of Cotter et al.'s log-odds measure, as well as by the results from our decomposition analysis.

### Changes in Percentages versus Changes in Logits

Since Cotter et al. use basically the same data source we do, our different conclusions are due solely to differences in the measures used and the

interpretations drawn from them. Understanding what the measures represent—and what they do not represent—can help to clarify the source of disagreement.

Our descriptive measure is straightforward. It tracks the proportion of women whose earnings fall into each of the men's earnings deciles and expresses change in terms of percentage point growth or decline. So, for example, between 1967 and 1987 the fraction of white women earning in the bottom male earnings decile fell from 48% to 27%, a net drop of 21 percentage points.<sup>1</sup> Over the same period, the fraction of white women earning in the top male earnings decile grew from 0.58% to 1.59%, a net gain of 1 percentage point. When we say that "white and black women's gains occurred largely at the bottom of the relative earnings scale," this is what we are referring to: 21 times more white women were affected by the changes at the bottom of the earnings scale than were affected by the changes at the top. For black women, the comparable statistics are even more skewed: a 37% drop in the bottom decile, and a 0.7% rise in the top. By expressing change in terms of the percentage of women affected, our measure preserves the extent of distributional inequality when analyzing changes over time.

Cotter et al. argue that instead of looking at the percentage of women affected at different earnings levels, the main focus should be on the log odds of change. Their measure tracks the logit of the percentage of women earning above several men's earnings percentiles. Like conditional probabilities, log odds and log-odds ratios (the logit trends) ignore the size of the group in each category and therefore condition away the primary facts of inequality. This is easily understood using a simple example, such as the one shown in table 1. Say we are interested in understanding changes in the percentage of women earning in the top male earnings decile under two different scenarios. In scenario A, women's representation in the top decile of the male distribution is very low, starting at 1% and rising to 2.67% by the end of the observation period. Using Cotter et al.'s logit measure, this is a change of 1.00 units. In scenario B, women are equally represented at the start, with 10% in the top decile, and by the end of the period are doing better than the men, with 23.2% in the top decile. Using Cotter et al.'s logit measure, this too is a change of 1.00 units. Does it seem as though their measure is missing something important about inequality?

The last column shows how our measure captures the same observed

<sup>1</sup> Note that this is not an absolute measure of change, as Cotter et al. claim, but it is the basic rate of change that describes this process: a 21 percentage point change per 21-year period. Cotter et al.'s measure, which they claim is the rate of change, is in fact a conditional rate of change that controls away the underlying distributional inequality, as we will show below.



TABLE 1  
TWO MEASURES OF DISTRIBUTIONAL CHANGE UNDER TWO DIFFERENT SCENARIOS

SCENARIO	% OF WOMEN IN TOP MALE EARNINGS DECILE		LOGIT CHANGE	PERCENTAGE POINT CHANGE
	Starting	Ending		
A . . . . .	1.00	2.67	1.00	1.67
B . . . . .	10.00	23.20	1.00	13.20

change under the two scenarios. When presented alongside the starting and ending percentages, this measure makes it possible to observe *both* the absolute and the relative magnitude of the change that occurred over time (see table 1, p. 318, in our original paper). Nothing is hidden, and, in particular, the degree of inequality is retained. By contrast, logits ignore the very different proportions of high-earning women under the two scenarios. Removing these "marginal effects" is what odds measures are meant to do, and they do it quite well. But in this context, where inequality is the focus, the size of the group is not a marginal effect, it is a central characteristic. Cotter et al.'s measure is thus inappropriate for the task at hand.

To see how this principle works with our actual data, we replicate the author's logit-trend figures and compare them to the trends in percentage point changes. Figure 1 uses the data from our original article and shows the logits of the percentage of black and white women earning above the tenth, thirtieth, fiftieth, seventieth, and ninetieth percentile of white men's earnings, from 1967 to 1987. The message is clear: the logits increase at roughly the same rate across the earnings scale.<sup>2</sup> We have no dispute (and never did) with the authors' conclusion that "the rates of increase in women's access to higher earnings are quite consistent across the earnings distribution" (p. 1150).<sup>3</sup> In fact, the terms in the multiplicative version of our decomposition capture exactly this aspect of change in the relative distribution.

The problem is that the authors conclude from this finding that "the

<sup>2</sup> It should be noted that the cumulative measures used by Cotter et al. in fig. 1 are highly dependent series, so similar patterns are less surprising than one might initially think. Noncumulative measures (such as the relative distribution) avoid this confounding effect.

<sup>3</sup> See pp. 308–12 in the original article. Readers who are interested in a detailed presentation of the multiplicative decomposition should see Morris (1996).

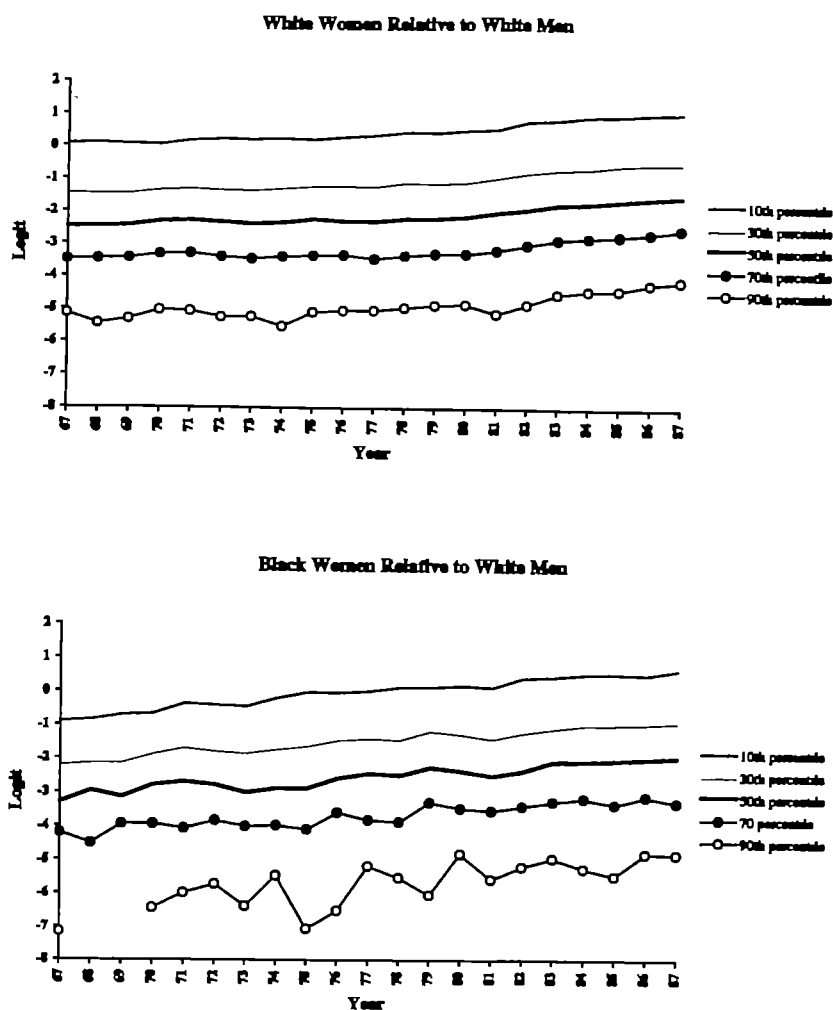


FIG. 1.—Logits of the percentage of white and black women earning above white men's earnings percentiles, 1967–87.

gains made by women at the upper end of the earnings distribution were as great as those at the lower end" (p. 1147). This leap lies at the heart of our disagreement, because it implies that women have made proportionately similar gains across the earnings distribution. Again, this has clearly not been the case. To illustrate this point we simply use Cotter et al.'s method of locating women's earnings on the men's earnings scale, but express the change in percentage terms, rather than logits. Figure 2

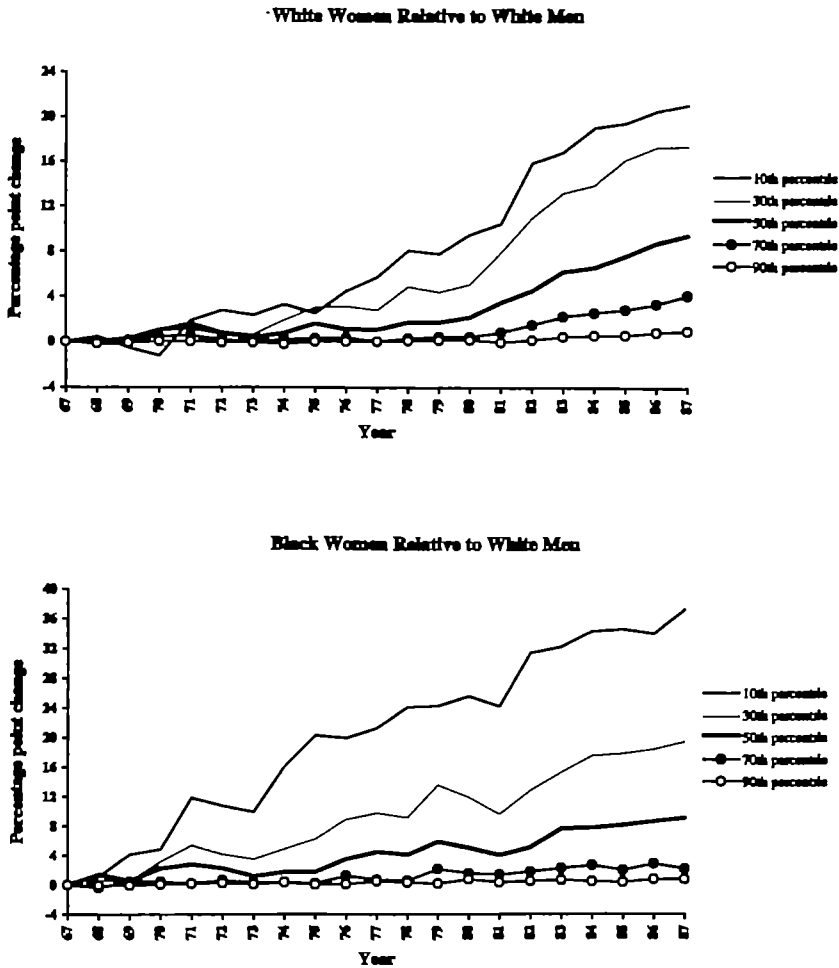


FIG. 2.—Percentage point change of the percentage of white and black women earning above white men's earnings percentiles, 1967–87.

shows the percentage point changes in the percentage of black and white women earning above the tenth, thirtieth, fiftieth, seventieth, and ninetieth percentile of white men's earnings. There is no question that the bulk of the gains made over this period were located in the bottom half of the distribution and that the gains at the top of the distribution have been quite small in comparison. Even by 1987, fully 80% of women continued to earn less than the median male worker, and the fraction of women with earnings in the top decile of men's earnings remained well under 2%.

The Meaning of the Logits

Why, then, do the authors argue that the log odds of change are the more important indicator of progress? They never really say. They simply introduce their measure and turn the crank, making no effort to justify why logits provide a better understanding of the substantive trends of interest here. A clue to their intentions can be found in their conclusion: "Of course, since most women started at the bottom of the distribution, that is where most women have gained. Only in this limited sense have the gains 'largely' been observed at the bottom of the distribution. . . . In general, there are still very few women at the top, but the rates of increase at the upper levels are at least as great as anywhere else in the earnings distribution" (pp. 1150-51). The authors present this as a purely descriptive and empirical conclusion. In fact, it implies a particular framework for analyzing changes in inequality. Cotter et al. do not make this framework explicit, so we will try to clarify it here. The authors effectively define two components of change in between-group inequality:

1. *The baseline distribution of inequality.*—The unequal distribution of women's to men's earnings is treated as a given—a legacy of the past history of gender inequality in labor market processes. One of the reasons that this distribution is still unequal may be that it contains older cohorts of women. These cohorts had the twin disadvantage of entering a gender-biased labor market with less human capital than men, and their earnings reflect this disadvantage.

2. *The odds of changes at different levels of that distribution.*—Once the residues of an unequal past have been accounted for, the only thing that is left is current inequality in how the labor market operates. This is presumably what the logit trends are meant to capture. The fact that these trends have been roughly equal across the earnings scale suggests to Cotter et al. that the *process* of earnings determination is now relatively blind to gender at all levels. The *outcomes* remain unequal because of the legacy of past discrimination, which has left the majority of women to start this new equal opportunity race at the bottom of the distribution.

This is a clear argument that can be systematically evaluated: Does the observed similarity in the logit trends indicate that the labor market is now operating under gender equality? Our conclusion is no, for several reasons.

First, the link between the change in logits and the absence or presence of labor market discrimination is an extremely tenuous one. The authors' logit trends are mechanical measures that have no direct relationship to discriminatory mechanisms. If one really wanted to test whether the *process* of earnings determination has become gender blind, these are neither the data nor the methods for the task. Such an analysis would require

measures that tap the *mechanisms* of discrimination: for example, occupational segregation and within- and between-employer wage mobility. Analysis of these measures would need to be cohort specific, testing for a decline in discrimination for young women who have recently entered the labor market and comparing this to the experience of women with longer work histories. Longitudinal data are necessary because only this would allow for an accurate measure of women's work experience. This is clearly an important and interesting research task. This point, however, is that Cotter et al.'s logits do not even begin to tap this information.

Second, the logit measures mask more than they reveal. The authors effectively see the similar trends in figure 1 as indicating an equalization of opportunity across the earnings distribution. But a closer look at what lies behind these aggregate trends suggests that the similarity is superficial and misleading. Here we draw on the decomposition that formed the main contribution of our paper (and that the authors ignore). The decomposition showed that women's gains at the bottom, middle, and top of the earnings distribution were driven by very different changes and that these effects also differed between white and black women. For white women, the striking polarization in *men's* earnings drove the large gains at the bottom of the distribution; the small gains at the top were driven by the recent polarization in *their own* earnings. For black women, the dominant effect was the strong growth in their median earnings relative to men's; here, the polarization in men's earnings played a more subdued role.

In short, a complex combination of trends has generated what the authors call "parallel . . . changes at every level of the earnings distribution" (p. 1148). This is where their interpretation goes wrong. Because their logit measure only describes aggregate trends, it cannot capture the variation in sources of distributional change. The logits therefore cannot be used to infer the kind of substance and meaning that Cotter et al. would have us infer. Indeed, it was this kind of limitation, shared by most summary measures, that led us to develop the relative distribution methodology. Our explicit intention has been to provide a set of techniques that are more responsive to the theoretical questions in the field.

Finally, by ignoring our decomposition findings, Cotter et al. have misrepresented our conclusions. It was the decomposition findings, and not the descriptive summary statistics, that were the basis of our pessimistic predictions for the future. Asking how the gender gap might behave in the future requires that we think about the different change components underlying current trends. What will happen if the polarization in men's earnings begins to stabilize? (The later years of our series suggest this may be the case.) What will happen if the growth in black women's median earnings begins to slow? (This possibility is suggested by the completion of their exodus from low-pay domestic work.) One plausible answer is

that the other sources of change will start to take over—in the case of white women, the polarization in their own earnings, and in the case of black women, the actual downgrading of their own earnings. Both imply a slowing of women's progress in the future. And, as Cotter et al. state, their updated time series supports this prediction.

In sum, Cotter et al.'s comment does not have much to recommend it. The authors have focused a lengthy and repetitive argument on the introductory descriptive findings of our article, ignoring its central contribution. They have introduced an arbitrary measure and made no attempt to present a framework that would justify using this measure to analyze changes in inequality. As a result, their statements based on this measure do not add to our understanding. To the extent that these statements are correct, they are descriptive, available in the original article, and do not contradict our own findings. To the extent that they move beyond description, the conclusions are unsound and unsupported by the results of our decomposition.

ANNETTE BERNHARDT

*Teachers College—Columbia University*

MARTINA MORRIS

*Pennsylvania State University*

MARK S. HANDCOCK

*Pennsylvania State University*

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## Review Essay

*The Rise of Christianity: A Sociologist Reconsiders History.* By Rodney Stark. Princeton, N.J.: Princeton University Press, 1996. Pp. xiv+246. \$24.95.

Jonathan Z. Smith  
*University of Chicago*

Despite the more than 900 entries listed in David May's *Social Scientific Criticism of the New Testament: A Bibliography* (Mercer University Press, 1991), Rodney Stark's study is a rare and sophisticated instance of deductive social science theory applied to the history of Christianity in the first four centuries. It is a work as significant for the questions it brings to the historical data as for its answers. The overarching question, "How was it done? How did a tiny and obscure messianic movement from the edge of the Roman Empire . . . become the dominant faith of Western civilization?" (p. 3), dissolves into a set of basic queries with respect to quite ordinary features of social formation: How many early Christians? Who were they? Where were they? What were their comparative advantages? Each question is answered by recourse to "strong" theory, formal methods of analysis, and, where possible, quantification. In a manner largely eschewed by professional scholars in the field, early Christianity is taken as an example of a general theory of religion as expressed in Rodney Stark and William S. Bainbridge, *A Theory of Religion* (Peter Lang, 1987).

The central focus of the work is a series of studies of early Christian demographics. Stark's estimate of a 40% per decade increase in the number of Christians between the years 40 and 350 is held to fall well within the parameters of comparable population dynamics and requires no extraordinary explanations whether they be claims of miraculous rates of conversion or the effect of Constantine. The rise of a Christian majority was "purely a function of a constant rate of growth" (p. 10). Like other "cults," "it was not a proletarian movement but was based on the more privileged classes" (chap. 2). Early Christianity's most successful network for conversion was within the widespread population of hellenistic Jews (chaps. 3, 6). Because of their ethic of concern for others, early Christians had a "superior survival rate" during the Empire's periodic epidemics (chap. 4). Christianity's rejection of abortion and (female) infanticide led to a surplus of Christian women. In turn, this resulted in a higher Christian fertility rate and encouraged marriages of Christian women with non-Christian men, thus increasing secondary conversions (chap. 5). Each

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of these empirical proposals is buttressed by theoretical propositions as to the roles of social networks and interpersonal relations in conversions, differentiations between "sect" and "cult," social epidemiology, sex ratios and the status of women, and so on.

The theoretical heart of the work is the application of elements of rational choice theory and the notion of religious "economies" to the question of the relative advantages of Christianity (chaps. 8–9). In the absence of attainable rewards, individuals will accept "compensators" if their value is socially established and if participation in the social group produces some tangible rewards. The high value of early Christian compensators is guaranteed by the examples of voluntary martyrdom; Christian demands for self-sacrifice and Christianity's socially stigmatized status produced a strong collectivity with its attendant rewards and its elimination of the socially costly problem of "free riders." In short, "it was not simply the promise of salvation that motivated Christians, but the fact that they were greatly rewarded here and now for belonging. Thus while membership was expensive, it was, in fact, a bargain. That is, because the church asked much of its members, it was thereby possessed of the resources to give much" (p. 188).

Stark's pioneering enterprise of applying strong social theory to early Christianity and of treating Christian data as characteristic examples of a generic theory of religion is long overdue, and will be, as he suggests, of equal importance to both students of Late Antique religion and the social sciences. Stark is correct in his critique of earlier social theorists. However, despite the currency of many of his citations, he remains indebted to an understanding of early Christianity that is of the same vintage and subject to equivalent questions. That is to say, while his studies go far to make plain emergent Christianity as an ordinary, human, social phenomenon, the work by scholars of early Christianity on whom he relies persist in treating it as extraordinary.

While there is passing acknowledgment of the diversity of early Christianities (p. 140), this has not resulted in an effort by Stark to define Christianity, to eschew the inappropriate use of the singular terms, "Christianity" or "church," or to entertain the possibility of differing social formations for different Christianities or for various regions. Stark has not questioned the dramatic myth of Christian origins canonized by Luke and Eusebius—"However, on the morning of the third day something happened" (p. 44)—and he is overly confident on both the Jerusalem church and the effect of Paul's "missionary" endeavors. (See, among others, Ron Cameron, "Alternate Beginnings—Different Ends: Eusebius, Thomas and the Construction of Christian Origins," in *Religious Propaganda and Missionary Competition in the New Testament World*, edited by Lukas Borman, Kelly Del Tredici, and Angela Standhartinger [E. J. Brill, 1994]: pp. 501–25; Merrill Miller, "Beginning from Jerusalem: Reexamining Canon and Consensus," *Journal of Higher Criticism*, vol. 2 [1995]). He has not confronted the counterevidence on the role of women



in early Christianity (Kathleen E. Corley, "Feminist Myths of Christian Origins," in *Reimagining Christian Origins* edited by Elizabeth A. Castelli and Hal Taussig [Trinity Press International, 1996]: pp. 51–67) or on Jewish and Christian proselytizing (Martin Goodman, *Mission and Conversion: Proselytizing in the Religious History of the Roman Empire* [Clarendon Press, 1994]).

For the third and fourth centuries, Stark surprisingly confines his data almost wholly to literary sources and to scholarly opinion. With the exception of his appeal to Roger S. Bagnall's papyrological work on Egyptian Christianity, there is no substantive use of nonliterary evidence, such as Michele R. Salzman's statistically sophisticated and socially informed prosopographical study of 319 cases of Christian senatorial aristocrats in the period 282–423 ("How the West Was Won: The Christianization of the Roman Aristocracy in the West in the Years after Constantine," in *Studies in Latin Literature and Roman History*, edited by Carl Deroux [Latomus, 1992]: 6:451–79). This literary bias is more pronounced with respect to the non-Christian religions of Late Antiquity which, following Christian apologetic praxis, are collectively lumped together under the title, "pagans," and are allowed but rarely to speak for themselves, being heard largely through the descriptions of both ancient and modern Christian polemicists.

At a time when there is much interest in social redescription of early Christianities concurrent with a reevaluation of early Christian histories, Stark's work provides a model for the former enterprise and an occasion for reconsideration in light of the latter.

## Book Reviews

*Power Restructuring in China and Russia.* By Mark Lupher. Boulder, Colo.: Westview Press, 1996. Pp. xv+335. \$69.00 (cloth); \$22.95 (paper).

Marc Garcelon  
*University of California, Berkeley*

Drawing on a wide array of recent scholarship, Mark Lupher's *Power Restructuring in Russia and China* develops a powerful comparative sociology of autocratic domination and social revolution in Russia and China from the Mongol conquest through the Tiananmen Square massacre and the collapse of Soviet communism. The continuity of power struggles between rulers, elites, and nonprivileged groups since the rise of the Ming and Muscovite autocracies forms the leitmotif of the analysis. Lupher builds this leitmotif around a central analytic device: Weber's concept of patrimonialism, the "key features" of which are "personalism, patron-client networks, fluid property relations, the secondary significance of the rule of law, and processes of power and resource appropriation" (p. 299). Following Franz Schurmann's *Ideology and Organization in Communist China* (University of California Press, 1970), Lupher distinguishes "power decentralization"—the shift of decision-making authority down the chain of command—from "power deconcentration"—the shift of power from autocratic officialdom to independent "power centers" at regional levels, such as landed nobles and voluntary associations (this crucial distinction is, unhappily, only fully spelled out in n.83, p. 162). Centralizing periods of autocratic power concentration thus alternate with turbulent periods of power "deconcentration" in favor of local notables and nonprivileged groups until new waves of power concentration based on new political and cultural syntheses restructure and revitalize patrimonial autocracy.

Lupher recasts the revolutions of 1917 and 1949 as the formation of new empires based on a creative synthesis of patrimonialism, revolutionary ideology, and the destruction of traditional elites in favor of a communist "new class" drawn originally from lower-class elements. After reconstructing the subsequent historical development of the Soviet and Chinese "communist empires," Lupher argues that both the attenuation of central power in favor of regional power centers, such as local governments and enterprises in post-Mao China, and the disintegration of the USSR in the wake of perestroika represent variations on a recurring pattern of imperial decay and disintegration in Russian and Chinese history. Moreover, he confidently forecasts authoritarian nationalism as the likely future of both countries.

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Lupher's treatment of the Cultural Revolution is of particular interest, as it draws on the author's experience as foreign student and participant-observer in Red Guard actions in Beijing between 1965 and 1967. Lupher frames the Cultural Revolution as a revolutionary autocratic's attempt to revitalize a "bureaucratically encrusted socialism" by "deconcentrating" power away from the "new class" in favor of mass organizations, thus combining radical populism with a classic patrimonial maneuver of attacking entrenched elites simultaneously from above and below, what Lupher calls "the top and bottom versus the middle strategy." (In contrast, Stalin during the Great Purges never "deconcentrated" power out of the hands of his personal staff in party and police organs.) According to Lupher, the Cultural Revolution backfired, as officialdom defensively appropriated Mao's strategy from below. By mobilizing "royalist" mass organizations and making common cause with other elite groups such as regional military leaders, "new class" representatives outflanked "the left" on a local level and steered revolutionary violence away from themselves and onto intellectuals and members of the old regime. After successfully co-opting the Cultural Revolution, a now "deconcentrated" officialdom bided its time until Maoists could be purged from the party and a program of market incentives and authoritarianism—in embryo since the early 1960s—could be implemented in the 1980s. Lupher then compares the similar logic of Mao's Cultural Revolution strategy and Gorbachev's perestroika—which also sought to deconcentrate power away from party officials to soviets and voluntary associations—in the process downplaying Gorbachev's stress on "socialist legality" and his avoidance of repression and personality cultism.

Lupher's unusual comparative framing of eight centuries of Russian and Chinese history—an audacious and remarkably successful undertaking—aims no less at demolishing analyses of Soviet and Chinese communism that invoke modernization, rationalization, and democratization. Lupher thus repeatedly takes unconventional stands on a wide range of contentious issues—from the significance of the Mongol legacy in Eurasian political and social history to the historical character of Soviet and Chinese communism—a strategy sure to provoke objections from historians and area specialists on any number of points. And yet, Lupher's command of extant scholarship and his compelling account of the Cultural Revolution give his tightly focused argument the depth to weather careful scrutiny. If there is a serious weakness in Lupher's continuity thesis, it lies in his failure to address seriously the growing integration of contemporary Russia and China in the global economy, with all of its attendant consequences: Lupher acknowledges globalization in passing, yet says little regarding how global integration may alter the long cyclical pattern he elucidates. Despite this problem, the book's forcefully argued central thesis will prove highly stimulating to area specialists, historical sociologists, and policymakers alike.

*China's Motor: A Thousand Years of Petty Capitalism.* By Hill Gates. Ithaca: Cornell University Press, 1995. Pp. x+326.

Prasenjit Duara  
*University of Chicago*

Hill Gates argues that a dialectic between the "petty capitalist mode of production" (PCMP) and the "tributary mode of production" (TMP) has dominated Chinese society since the Song dynasty (960–1279) and continues to this day in both Taiwan and the People's Republic of China, although in different combinations and with different effects in the two places. The work is in the tradition of Marxist historical sociology, but one in which the Eurocentric provenance of Marxist categories is abandoned for ones truer to Chinese historical realities—such as PCMP and TMP. Even so, the work faces the challenge of writing macrohistory at a time when historical and ethnographic microanalyses suggest great differences over time and space. Gate's thesis, based principally on English language studies, is suggestive and captures significant aspects of Chinese society, but the fuller argument has too many weaknesses to explain successfully many dimensions and details of state-society relations in China.

Petty capitalism is family-based production for the market that does not transmute into full-fledged capitalism (where the goal is capital accumulation through legally autonomous private property and rights) because it is dependent upon, but is also competitive with, the tributary state. TMP has existed in the Chinese imperial state since the third century B.C., and its mode of surplus extraction—through revenues and gifts—necessitates production for the market among petty producers. While PCMP follows its own logic of commodity production that exceeds the requirements of the tributary state, it remains dependent upon this state to ensure the patriarchal authority structure through which surplus is extracted from women and children to profit men in the family and state. PCMP also necessitates the commodification of family members as a means of generating profits.

In broad outline, the thesis has much going for it: it is clear that market society in China has had a long existence without capitalist legal institutions and has developed its own social mechanisms to survive and grow—even after 30 years of antimarket socialism. It is also the case that the state in China has displayed strongly paternalistic tendencies through various different formal structures and regimes. More particularly, Gates's analysis of the structure of domestic production: the phenomenon of bride price, the imperial state's sanction of patriarchy and lineage ideology, and, most of all, the pervasive sale and devaluation of girls, is generally persuasive.

However, there are chapters in the book that do not add much to the argument and possibly even detract from it. Thus, for instance, there is almost an entire chapter devoted to G. W. Skinner's macroregional system (*City in Late Imperial China* [Stanford University Press, 1977]).

While Skinner's model, which has little space for state imperatives in the definition of regions, is justifiably criticized for its rigidity, Gates not only throws out what is of value in Skinner's core/periphery distinction, her own alternative is cruder and unconvincing. The chapter on folk religion is even less convincing as it tries to persuade us that Buddhist and Taoist deities represented two distinct types of authority: merchant/capitalist versus official. On the subject of ritual money, apparently, ordinary people depicted their gods as capitalist exploiters because they used money as interest-bearing capital, whereas mortals used money simply for repayment of debt. The ethnographic evidence for all of this is very thin (see p. 170), and the argument just does not emerge from the materials in the powerful way that Taussig's (*The Devil and Commodity Fetishism in South America*, 1975) analysis of commodity fetishism emerges from his ethnography of devil worship.

Finally, in the analysis of contemporary Chinese states, the author usefully corrects the mis- (or over-) attribution of Taiwanese economic success to rational, strategizing bureaucrats; the Kuomintang state in Taiwan was a tributary state—at least until the mid-1960s. But her suggestion that even large private corporations are part of the PCMP because of their dependence on the tributary state is difficult to assess because she has little on the state after the 1960s, and the assertion does not do much to clarify PCMP. Also, while there are overt similarities between the tributary qualities of the People's Republic of China and the imperial state, it is hardly illuminating to see them as basically the same. Indeed, in attributing to the state such great continuity in the face of such momentous historical transformations, the author, who is determinedly materialist, may well appear to some as philosophically quite idealist.

*Social Capitalism: A Study of Christian Democracy and the Welfare State.* By Kees van Kersbergen. London: Routledge, 1995. Pp. xii+289. \$69.95.

Stathis N. Kalyvas  
New York University

To say that Christian democracy is an overlooked phenomenon would be an understatement. A dominant political movement in most of Western Europe, Christian democracy remains as neglected and misunderstood as it is genuinely puzzling. Its distinctiveness has been routinely denied: it is either confused with Catholicism and the Catholic Church or with conservatism and conservative parties. Kees van Kersbergen's book, which should be welcomed by students of European politics and the welfare state, sociologists and political scientists alike, clears up much of this confusion. He shows, first, that a distinctive Christian democratic welfare state emerged that was significantly and systematically different from

both the social democratic and the conservative/liberal social policy regimes. He further demonstrates why Christian democracy should not be confused with the official church, or even with Catholic culture: social Catholic ideology went far beyond the teachings of the Catholic Church, while Christian democratic political and social movements transcended the church's social and political practice. As a result, this book emerges as a fundamental contribution to our understanding of Christian democracy and, hence, European politics at large.

The author sets out to perform two goals in a way that is methodologically rich and eclectic, combining statistical analysis with carefully constructed case studies and a detailed account of the evolution of Catholic social and political ideas. He shows that a distinct welfare-state regime emerged in countries dominated politically by Christian democratic parties. He, then, connects the rise of this welfare-state regime with the ideology of social Catholicism and Christian democracy.

The main finding is that Christian democracy is associated with a core of social policies, dubbed "social capitalism," that aggregate into a welfare state funded by social democratic rates of spending but with very different features. This is a welfare state that tends to be generous but passive and transfer oriented. Its main features are income replacement rather than job protection or creation, the privileging of families rather than individuals, the fragmentation and semipublic character of major aspects of the administration and execution of welfare policies rather than their centralization and state control, the reproduction of social status rather than the refashioning of social structure, and the relative obstruction of women's access to the labor market.

This is an important finding that goes against most of the relevant literature that explains the growth of the welfare state by reference either to nonpolitical (demographic, cultural, etc.) variables or to the impact of social democracy. The analysis offers, correctly I think, a pessimistic prediction about the future of Christian democracy. For a variety of reasons, social capitalism (much like social democracy) has reached its limits and this will affect negatively the future prospects of both Christian democratic parties and the kind of societies they built. Many interesting questions follow from this analysis. For instance, how will countries with different, but equally generous, social policy regimes respond to the (similar) challenges they face?

The second part of the book, a review of the theory of social Catholicism and an examination of the origins and evolution of Christian democratic ideology, is less convincing than the first. Although offering a superb analysis of ideological evolution and transformation, the author fails to demonstrate convincingly that it is this ideology that accounts primarily for the emergence and evolution of the Christian democratic welfare-state regime. The causal link between social Catholic ideology and the development of a specific set of social policies remains unspecified. As a result, the analysis tends to be inconsistent. It is, for instance, full of references to the impact of electoral constraints and electoral strategy: social

capitalism turns out to be an "important electoral asset," a "resource," and a "medium" for "Christian democratic power mobilization" (pp. 233–38). The case studies, in particular, privilege a strategic interpretation of welfare state building. The "politics of mediation" (the practice of accommodating conflicts between different social groups, which is associated with Christian democratic welfare policy) is sometimes the result of religious inspiration and sometimes the outcome of electoral constraints and calculations. Variation in social policies of Christian democratic countries is explained in terms of electoral strategy rather than ideology, while the present demise of social capitalism is accounted for by structural factors (decline of religion, decline of traditional identities, drying up of resources) rather than ideological ones. In addition, this account remains unconnected to the thriving theoretical debate on the role of ideas and interests in policy-making.

These shortcomings do not dilute the book's main contribution. They rather suggest what the next research step should be: a systematic and analytical account of the links between the electoral strategy and the political economy of Christian democratic parties—in times of both socio-economic growth and retrenchment. This is precisely the book's central merit: by furthering our understanding of Christian democracy, it opens up new areas of research. In the future, no study of the welfare state and of European politics, not to mention Christian democracy, can afford to ignore this book.

*Origins of a Spontaneous Revolution: East Germany, 1989.* By Karl-Dieter Opp, Peter Voss, and Christiane Gern. Ann Arbor: University of Michigan Press, 1995. Pp. ix+280. \$49.50.

Rod Aya  
*University of Amsterdam*

Ann Vogel  
*University of Washington*

Thucydides blamed revolution on hope of success, and Plato blamed that hope on weak government torn by faction. The book under review bears them out. It argues that the East German revolution of 1989—the least violent revolution in German history—happened because people thought that by demonstrating en masse they would get concessions and get away with it. They were right. The government wavered for several months and resigned. Flouting Marxist prophecy, the Communist state withered away soon after its fortieth birthday.

Opp is a leading exponent of rational-choice theory, which he and his colleagues use to explain why many citizens chose to demonstrate and why the government chose to hold fire. To explain each choice of action as rational, they argue that the benefit of the action taken was greater

than the cost—the cost being the benefit of the best alternative action forgone. Rational-choice theory says that people aim to maximize profit (benefit minus cost) and that from among the actions they see open to them they will take the one that they think will achieve their aim. Hence to explain an action as rational is to argue that the actors thought it would succeed. (Success is relative to aim, so if the aim is to maximize profit, an action succeeds if it achieves that aim.) Then the question is, What evidence exists that the actors thought the action would work? Without such evidence, the explanation is circular.

To clinch their explanation of the choices that combined to make the East German revolution, the authors cite an opinion survey of 1,300 people in Leipzig (where the pivotal demonstrations took place) plus some insider anecdotes on how the government weighed its options. The latter evidence is journalism at best, but the opinion survey (made one year after the revolution) lets the authors test their hypotheses with systematic data rather than (as is typically the case in research on revolutionary situations) make do with hit-and-miss reports by observers. The testable hypotheses are empirical assumptions about what the participants wanted and thought they had to do to get it that (in conjunction with rational-choice theory) would explain why they took the actions in question. Thus the problem is to show that citizens thought that demonstrating and the government thought that holding fire would maximize profit, that is, succeed.

For citizens, maximized profit was two sorts of benefit besides impunity—political rights and reforms plus various moral and social rewards of participation. For the government, it was staying in power. Both sides thought that their strategy would succeed mainly because Gorbachev refused to rescue Communist diehards. Soviet policy raised (for citizens) the net benefit of demonstrating and (for the government) the net benefit of holding fire, so that making these choices would maximize profit.

So far, the explanation is cogent, the evidence is credible. About a fourth of respondents demonstrated, and they were virtually unanimous in seeking democratic rights and reforms. The discontent that motivated them to demonstrate was chiefly political, not economic or social, and it was no greater in 1989 than before. What *was* greater was the expected benefit of protest compared to that of obedience.

For the demonstrations to succeed, however, three problems had to be solved—coordination, critical mass, and free riding. Since there was no organization to tell citizens where and when to assemble, to make enough of them turn out, or to reward joiners and punish shirkers, the question is how these problems got solved. The authors argue that Monday church in central Leipzig was the focal point for demonstrators, who thought that many other people would also turn out, that success depended on their taking part along with everyone else, and that participation was a moral obligation as well as a source of social approval. Obligation and approval hinged on expected success, which again owed mainly to Gorbachev. The evidence here is credible—most respondents agreed with the



authors' propositions—though one should note apropos the free-rider problem that obligation and approval are selective incentives and that people who think they can influence events do not face a choice situation where a free ride is tempting.

Not all of the argument holds water, however. The book bristles with hypotheses for which the evidence is feeble—the footnotes cite vanishingly insignificant correlations between professed opinions and reported behavior. One precarious hypothesis is that repression (unless overwhelming) incites demonstrations by raising the incentives to resist. The evidence is an artifact of questionnaire semantics: “repression” meant arrest, injury, problems at work, and trouble for one’s family, not getting killed or consigned to the gulag.

Also, the account of rational-choice theory is confused and confusing, though not fatal—the reader (with effort) gets the general idea. But the authors omit to consider Herbert Simon’s argument that people can maximize profit only if they know all actions open to them so they can choose the best, whereas in fact they know just a few and so choose one that is good enough—better than any known alternative. Opp’s best point about theory is that structural explanations of revolution bring individuals back in to do the rough stuff, and that structures as macrobehavior or (in Alvin Gouldner’s phrase) “persons *doing* structure” enter those explanations as constraints on choice. This excellent point makes the book’s neglect of West Germany—a constraint that loomed large for East Germans—all the more curious.

A final trouble is presentation. This is not an integrated book with a cumulative argument, but 16 essays, which read as if written for as many conferences. Discussions often stop before they conclude, and the many cross-references sometimes lead nowhere. The English resembles office memoranda, with annoying lapses of usage, and needs a rewrite editor or even a ghostwriter like the artisans who produce best-sellers for celebrities. Lack of an index complicates reference, though the chapter summaries help.

*Can Europe Work? Germany and the Reconstruction of Postcommunist Societies.* Edited by Stephen E. Hanson and Willfried Spohn. Seattle: University of Washington Press, 1995. Pp. 238. \$35.00 (cloth); \$17.50 (paper).

Thomas Faist  
*University of Bremen*

The economic, political, and cultural origins and effects of the societal transition in Eastern Europe and their implications for the reconstruction of these countries pose an unparalleled challenge to sociological analysis. The contributors to this volume tackle the task from a macrosociological and macrohistorical view. The three parts of the book concern national-

ism, Germany's historical and contemporary role in rebuilding Eastern Europe, and economic reforms in transition countries.

The rich and theoretically informed analyses can be seen as valuable case studies of large-scale change. Their Polanyian perspective focuses on the interplay of domestic and international factors and political and economic change, enriched by a special sensitivity to the cultural sphere. In an imaginative article, Stephen Hanson applies Polanyi's analysis of what effects the ideology of the self-regulating market had on the disruption of cultural life in Russia. In his view, what both domestic political elites and foreign actors did not realize was that implementing policies that commodify land, labor, and money would be problematic at best. Arista Maria Cirtautas sees nationalism not as an irrational product of immature societies but as a rational response to both the socioeconomic and cultural features of latecomer societies and the structure of the liberal international community. She argues that, in the liberal countries of Western Europe, the nation never supplanted the individual, while latecomer societies farther East found it easier to accept national rights without accepting individual rights. This is a typological juxtaposition that also runs through Liah Greenfeld's contribution. What this means for contemporary politics is shown by Ewa Morawska. In her thorough analysis of the Polish Catholic Church, she ties changes in the political context—the collapse of the Communist regime and the emergence of a differentiated and plural society—to the public's perception of the role of religious institutions. The long-standing conflation of religious and national ideas and institutions in the minds of the public had begun to dissolve in the 1990s. Kazimierz Poznanski presents an illuminating analysis of the causes for the collapse of the regional economy in the early 1990s. He argues that distorted incentives for state enterprises resulted largely from the dilution of property rights in the public sector. This seems to suggest not only that it takes time to fill the "property vacuum" but also, beyond the conclusions of the author, that difficult economic problems can only be solved by the emergence of credible and stable political institutions.

The title of the book is misleading. It does not deal with the implications of East European transition for all of Europe; for example, only two articles refer in passing to the role of the European Union (EU). The possible accession of selected East and Central European states to the EU is not raised, an odd omission given the associated status these countries have achieved in past years. Moreover, the promise of the subtitle is only redeemed in Willfried Spohn's mindful article on Germany and in Aleksa Djilas's analysis of Germany's role in the Yugoslav conflict. The latter author emphasizes the destructive potential of German politics; perceptions of German policymakers go back to 19th-century dichotomies between a Christian West, reflected in Catholic Croatia, and a barbaric East, exemplified by Serbian nationalism.

Though most contributors have admirably mastered the longer historical perspective, one certainly would have liked to hear more about the

question raised by Daniel Chirot's brief introductory comparison of two large actors: the role of the United States in reconstructing Europe after World War II and Germany's position in Eastern Europe in the 1990s. Unfortunately, the comparative threads are not taken up in the case studies and are not tied together in the end. For instance, Spohn, applying Stein Rokkan's work on state building, finds that Germany is now firmly rooted in Europe through its political and economic integration in the EU and the cultural basis of democratic legitimacy, despite an increasing renationalization of German political culture after unification. Yet, equally plausible are the conclusions to be drawn from Ivan Berend and Djilas, who view the uneasy relationship from an East European angle. The structural and ideological circumstances that once made Germany so threatening to its Eastern neighbors are still present. Nonetheless, although the volume clearly lacks an integration of the case studies and common conclusions, it can claim its rightful place in the literature of transition through its consistent application of a historical-sociological approach.

*The Radical Right in Western Europe: A Comparative Analysis.* By Herbert Kitschelt with Anthony J. McGann. Ann Arbor: University of Michigan Press, 1995. Pp. xiii+332.

George Steinmetz  
*University of Chicago*

Herbert Kitschelt has published widely on the libertarian left and has now turned his attention to the new radical right. This book attempts to account for the rise of new radical right parties in Europe and for cross-national variations in their strategies and success. After developing a theoretical explanation of these parties, Kitschelt presents a series of national case studies, contrasting successful parties like the French National Front with relative failures like the German Republikaner.

The first step in the argument plots voters' political preferences within a two-dimensional space. One axis runs from socialist to capitalist politics, another opposes libertarian to authoritarian politics. During the Cold War period, most voters were arrayed along the socialist-capitalist axis. Contemporary preferences are distributed diagonally, clustered in the left libertarian and right authoritarian corners. To explain this shift, Kitschelt develops a model of the genesis of libertarianism and authoritarianism. Libertarian preferences for individual freedom and participatory democracy are generated by higher education and by work processes centered around human relations. Authoritarian political proclivities are generated by lower levels of education and by work in "instrumental economic settings" where task structures are "heavily guided by rules and orders."

This social-psychological model is then embedded within an account

of ongoing socioeconomic transformations. The shift to a postindustrial economy increases the proportion of libertarians by multiplying skilled white-collar jobs while lowering the proportion of authoritarians by eroding blue-collar employment. Moreover, many blue-collar workers feel threatened by international competition and, in an effort to remain competitive, come to oppose state spending. Political authoritarianism is therefore increasingly associated with neoliberalism. By contrast, "people-centered" white-collar jobs are typically shielded from the international economy, which leads their incumbents to support redistributive policies. Contemporary libertarianism is therefore predominantly leftist, although this association is historically contingent.

The new landscape of political "demands" does not automatically transform the "supply" of political parties. The next stage of the argument focuses on political opportunity structures. New radical right parties can only make inroads into the electorate when mainstream right and left parties converge towards the center, freeing up right authoritarians for realignment. In countries like Britain and the Netherlands, where the conventional conservative and leftist parties have remained more polarized, right-wing voters are less tempted to switch their allegiances.

The third step of the argument concerns party strategy. Only if new radical right leaders adopt the "winning" formula of authoritarian neoliberalism will they be able to maximize their voting coalition. Antistate slogans will attract small businessmen and workers in sectors exposed to international competition, while marginal workers rally to the racist and authoritarian appeals. By contrast, parties who pursue a "welfare chauvinist" strategy, which combines support for social spending with racism and authoritarianism, will only appeal to the shrinking pool of marginal workers. What prevents some of these parties from making the rational choice is a cognitive framework resulting from the earlier socialization of older leaders into anticapitalist fascism, traditions that are then passed on to younger extreme-right militants. Fascist socialization is obviously a more significant factor in Germany than, say, Norway, and partly accounts for the failure of the German parties.

Kitschelt's book marks a major advance in the study of the new right. He uses voter surveys to show that these parties are more than a diffuse protest against elites or a single issue response to immigration. He shows that cross-national variation in the percentage of foreigners is only weakly associated with their success. Brief histories of each country's radical right underscore the differences between fascism and the new radical right. Kitschelt also carefully distinguishes between the different types of these parties and their specific etiologies.

The book also has several flaws. Its account of preference formation is overly economic and politically fatalistic. One of the central goals of radical political parties is to reshape political preferences rather than simply responding to them. Leftist parties should combat authoritarianism rather than pandering to it or turning away from workers altogether.

Moreover, the two-dimensional issue space seems too crude to capture all of the relevant differences in political preferences, and this has ramifications for the rest of the analysis. For example, Kitschelt's scheme associates all state interventions in the market with the "socialist" pole, regardless of content. Yet clearly the "anticapitalist" positions of the new white-collar class differ from the "anticapitalism" of industrial proletarians. Indeed, the increased emphasis on white-collar interests within many social democratic parties may better explain workers' shift to the right than any transhistorical model of authoritarianism. Finally, I wish Kitschelt had compared his parties to the new radical right social movements, which are clearly part of the same conjuncture. Radical right-wing extraparlimentary movements may sometimes be able to exert more pressure on the governing parties than new radical right parties, as was the case in the German government's 1993 decision to curtail the right to political asylum.

Despite my quibbles, this is the most sophisticated study of the new radical right parties to date and required reading for anyone interested in the radical right. Above all, this is an alarming book, underscoring the extent to which the new radical right has become a permanent feature of post-Fordist politics as a result of the abandonment of the popular classes by the traditional left.

*State Autonomy or Class Dominance? Case Studies on Policy Making in America.* By G. William Domhoff. New York: Aldine de Gruyter, 1996. Pp. x+296. \$49.95 (cloth); \$24.95 (paper).

Elisabeth S. Clemens  
University of Arizona

Over the past two decades, the state has reemerged as a major concern among political sociologists. In debates over the character of power in modern industrial democracies, the juxtaposition of "state-centered" and "class dominance" theories has been particularly important. In *State Autonomy or Class Dominance*, G. William Domhoff opens fire on state-centered analyses as represented by selected works of Theda Skocpol, Kenneth Finegold, Ann Orloff, and Gregory Hooks (since this critique is sometimes extended to "Skocpol and her students," I should acknowledge that I belong to that lineage; Hooks does not). In chapters devoted to agricultural policy, the National Recovery Administration, and Social Security during the New Deal, to industrial mobilization during World War II, and to progressive-era social legislation, Domhoff relentlessly musters evidence in favor of a class dominance interpretation.

Domhoff's argument begins with the proposition that, in the United States, there exists a coherent dominant class as demonstrated by network studies of shared schooling, membership in elite organizations, and

interlocking corporate directorates. This class pursues a distinctive class interest (fractions of the dominant class may disagree on specific policy issues, but these disagreements are not held to undermine his core claims). Given these propositions, any individual tied to the dominant class by family background, schooling, employment, or even friendship is treated as acting in the interests of that class.

This theoretical arsenal is brought to bear on the determinants of U.S. social policy through the deployment of the "decisional method," which asks who decides and who benefits from particular policy outcomes (pp. 16–18). The dominant class influences policy outcomes through special interest lobbying, policy planning, candidate selection, and ideological work (pp. 45–46), but these case studies rest on the close analysis of policy formation and searingly close examinations of citations and sources—this is a style of criticism certain to inspire nightmares among aspiring historical sociologists. Where state-centered analysts have credited government bureaucrats and academic experts with influence over policy outcomes, Domhoff responds with two contentions: first, if bureaucrats or experts were influential, they were tied to the dominant class and, therefore, did not make an independent contribution to policy formation; second, state-centered analysts have erred in accepting the accounts of liberal bureaucrats and academics where a closer examination of archival sources would reveal that the decision had been shaped by corporate elites, bureaucrats tied to corporate elites, reformers funded by corporate elites, or "the southern plantation owners" within the Democratic Party. (The fact that the power of this bloc may be attributed *either* to the economic position of its members *or* the institutional rules of the American party system suggests the difficulty in adjudicating between these theories.) The contribution of these critical case studies is to underscore the many ways in which business elites shape political outcomes and to warn against any easy equation of reformers and intellectuals with "elites."

Yet upon rereading the arguments excoriated by Domhoff, one might well conclude that a good bit of shadowboxing has gone on. The strong argument for state autonomy attacked by Domhoff is difficult to find. Beginning a discussion of social security, Domhoff observes that "perhaps recognizing that her analysis cannot sustain a case for a 'state-centric' theory in the United States, Orloff says she is merely emphasizing the 'institutional' context for policymaking" (p. 117). Ann Orloff (*The Politics of Pensions* [University of Wisconsin Press, 1993], p. 41) is, indeed, quite explicit on this point, explaining that she prefers "the term *institutional* to *state-centered* for several reasons; most important, the earlier term tended to give readers the impression that factors outside the state were unimportant to the explanation" and devoting most of *The Politics of Pensions* to the presence or absence of the cross-class coalitions (between workers and middle-class or elite liberals) that put old age pensions on the political agenda in Britain, Canada, and the United States. In a section headed "The Alleged Growing Autonomy of the USDA, 1933–1938,"

Gregory Hooks is mocked for mistaking "battles that had within them the potential for departmental autonomy as evidence for actual autonomy" (p. 72). Yet Hooks (*American Sociological Review* [55:35]) explains that "agricultural politics of the 1930s provide a vivid case of New Deal-era contradictions—both the Farm Bureau and the USDA bureaucracy gained power simultaneously. . . . Instead of being captured, the USDA remained insulated from these forces and challenged the AFBF at the national and local levels . . . This account exposes the New Dealer's vulnerability. Unlike their adversaries, New Dealers lacked a viable base of strength in society." To this reader, this passage is consistent with an account of "battles with" the potential for departmental autonomy—even Domhoff concedes that the ability of southern landowners to "keep the rural poor in total destitution" was constrained by "the disruptive potential of the poor people . . . and the leverage that liberals gained within the federal government because they were part of the Roosevelt coalition" (p. 73). No constructive purpose is served by such a skewed representation of a literature.

If disentangled from his sustained feud with state-centered theory, however, Domhoff's class dominance argument provokes a series of important questions that might contribute to a constructive refinement of sociological theories of policy outcomes. First, how should outcomes be characterized? Domhoff's targets are all *comparative* analyses—cross-national, across policy domains, across time—that explore differences in timing and policy content, whereas his own use of the "decisional method" emphasizes the balance of benefits in single policy decisions. These choices of method lead different scholars to ask about different levels or faces of power: Will a policy appear on the political agenda? What alternatives will be formulated? What specific options will be enacted in law? Second, what constitutes evidence of business dominance? Must a preponderance of corporate elites support a position or only the minority who have positions within the policy establishment? If the latter, how is it that one section of the capitalist class does for capital what it cannot do for itself? Finally, what can we conclude from the presence of a social tie? Domhoff treats most ties as strong ties, arguing that scholars funded by corporate elites promote elite interests rather than contributing to the definition of elite policy preferences. Yet network studies in economic sociology have demonstrated the power of "weak ties" to alter in different socioeconomic (and perhaps ideological) positions as well as the varying significance of types of ties within systems of interlocking directorates. Although they are frequently overshadowed by his feud with state-centered arguments, in *State Autonomy or Class Dominance?* Domhoff raises important questions that must be met by a more adequate theory of the interaction of state *and* society.

*From the Outside In: World War II and the American State.* By Bartholomew H. Sparrow. Princeton, N.J.: Princeton University Press. Pp. xv+354. \$45.00.

Michael K. Brown

*University of California, Santa Cruz*

Modern states have been formed by capitalism and war. There is by now some understanding of how the political and social conflicts unleashed by industrial capitalism have shaped 20th-century welfare states. We know considerably less about the effects of war on state building. If the importance of war for the growth of the modern state is acknowledged, there is but a sparse literature examining the question in the case of the United States, and most of that concentrates on World War I. Bartholomew Sparrow's *From the Outside In*, a book of theoretical sophistication and detailed historical research, now rectifies this with a searching and instructive examination of the effect of a single, dramatic event—the hegemonic war of the 1940s—on the political development of governmental institutions and domestic public policy.

Sparrow conceptualizes state building as a “process of organizational change” in governmental bureaucracies, in ties between public agencies and societal actors, and in the instruments of administration that in this case take place under conditions of crisis characterized by extreme shortages of resources—money, men, and materials—and great pressure to organize state and society for battle (p. 20). He examines what difference the crisis of war made in four policy domains—social security, regulation of labor relations, public finance (taxation and public borrowing), and weapons procurement. Utilizing the resource dependence approach drawn from organization theory, which examines how organizations adapt to a changing environment, Sparrow asks in each policy domain who directs state building during and after the crisis (the president or Congress), whether state building entails expansion of bureaucratic agencies or new linkages with societal actors, whether the war-induced changes endure and lead to permanent shifts in policy agendas, and how the war affects the distribution of resources between governmental agencies and among different citizens.

Not surprisingly, Sparrow finds that the president and executive agencies exerted far more control over labor, public finance, and procurement than Congress during wartime but lost influence in the postwar period. But he also finds evidence for dramatic changes in all these policy domains, some of them countering the conventional wisdom. He argues that the war derailed efforts to go beyond the 1935 Social Security Act and to build a more universalistic, redistributive welfare state; deepened government regulation of labor unions and led to the deradicalization of labor leaders when they fell in line with Roosevelt's demands for a no strike pledge; expanded the fiscal capacity of the state by making the income tax the primary source of governmental revenue and institutionalized



fiscal policy as a tool of macroeconomic control; and resulted in a much larger navy bureaucracy and higher defense budgets but did not create a "military-industrial complex."

Resource dependence theory proves to be a useful tool for investigating changes in discrete policy domains and gives Sparrow's analysis a rigor that is very often lacking in most discussions of state building. In addition, the book will be of interest to historically minded social scientists for the way in which Sparrow combines organization theory with the historian's craft. He makes excellent use of archival sources in fashioning his empirical arguments. The main questions that one might raise about the book pertain to his theoretical assumptions about the effects of war on state building.

As a causal factor, the significance of war in *From the Outside In* is that it is unusual and disruptive; it shatters the "equilibrium" of routine domestic politics and deflects ongoing patterns of state development from preordained trajectories. This assumption can be problematic. War certainly leads to a search for new financial resources or the creation of state capacity—the invention of a weapons procurement bureaucracy for instance—but Sparrow also appears to assume that World War II interrupted linear patterns of political development emerging from the 1930s. Alternative explanations get short shrift as a consequence. For example, Sparrow argues that the United States might have created a more universalistic welfare state had it not been for the war. Yet after the 1938 elections, the conservatives were in control, and Roosevelt had given up any hope for substantial reform sometime before the war began. In other words, how much weight one should place on the war as an event leading to the failure of the Third New Deal rather than on the post-1938 emergence of the conservative coalition is unclear and unexamined. One might raise a similar question about his assessment of how the war permanently changed policy agendas. Sparrow argues that the war stimulated greater federal regulation of unions to the detriment of the labor movement, but it is not clear that the war had any such effect, as David Wellman's subtle investigation of collective bargaining in the longshoreman's union demonstrates (see *The Union Makes Us Strong: Radical Unionism on the San Francisco Waterfront* [Cambridge University Press, 1995]). *From the Outside In* is an important book that raises new questions about state building and the place of war in this process. It is a book that will be of great interest to all political scientists and sociologists concerned with these issues.

*Globalisation in Question: The International Economy and the Possibilities of Governance.* By Paul Hirst and Grahame Thompson. Cambridge: Polity Press. 1996. Pp. vi+227. \$21.95.

Roland Robertson  
*University of Pittsburgh*

*Globalization in Question* is a book written by a social theorist and an economist about the international economy and the control of economic activity in the contemporary world. The book pays virtually no attention to sociological issues, and one would get no idea from reading it that the sociological discussion of globalization has been rapidly expanding for the past 20 years or so. In fact in a one-page section toward the end of the book, entitled "Further Reading" (p. 212), there are just three references mentioned in connection with sociological views of globalization. But even some of those who do not specialize as sociologists in this field will know that the sociological discussion of globalization was well under way before economists and people working in the field of business studies moved into the arena. By now, in spite of the pathbreaking work of sociologists and anthropologists—as well as practitioners in other fields, such as those in religious, cultural, and communication studies—influential figures in politics and journalism around the world conceive of globalization as an almost exclusively economic process, so this perspective has permeated everyday discourse.

This unfortunate circumstance is not entirely the fault of economists and practitioners of business studies. Economistic and materialistic sociologists have played a role in the tendency to consider globalization as a basically, even an entirely, economic phenomenon. Nonetheless I cannot recognize the name of a single world-system theorist in the index of this book and only one such name in the list of references (although it should be said, in any case, that *globalization* is not a favored term of most world-system theorists).

What, then, is this book actually about? Essentially, Hirst and Thompson set out to challenge "the more extreme claims of the most enthusiastic globalization theorists" (p. 2), meaning the claims made by economists concerning the recent crystallization of a global economy. In undertaking this task, the authors persuasively show that the modern "internationalized economy" is nowhere as new as some economists now claim it to be. They also argue convincingly their second major thesis, that fully transnational companies are relatively rare; this discussion is important in view of the tendency of many in the social sciences to talk as if the world is now dominated by such companies. The authors' third main thesis is that the mobility of capital is producing a high concentration of foreign direct investment among the major industrial economies, with the developing countries largely remaining marginal so far as trade and investment are concerned. In more general terms, Hirst and Thompson make the (unoriginal) argument that financial and investment flows are

concentrated in the triad consisting of Europe, Japan (and, one must assume, increasingly other parts of Asia), and North America (assuming in this case that North America means the United States and Canada but not Mexico).

*Globalization in Question* hinges upon a crucial distinction between the concept of "an inter-national economy," on the one hand, and "a globalized economy," on the other. The globalized economy is regarded by Hirst and Thompson as an ideal type. It is, they propose, a necessary myth, in large part because by seeing it as a myth we can obtain a full picture of "the fundamental problematicity of its governance" (p. 10). Other important consequences of a truly global economy, as opposed to the "inter-national" economy that actually exists, would be the transformation of multinational corporations (with what might be called national homes) into transnational corporations, a further decline in the power and influence of organized labor, and a growth in the multipolarity of the international political system.

The inter-national economy is one in which the primary units are national economies. Hirst and Thompson show that the degree of autonomy in this respect was considerably less under the gold standard up to World War I for the more advanced national economies than it is at the present time. A fully global economy would be one in which "distinct national economies are subsumed and rearticulated into the system by international processes and transactions" (p. 10). Undoubtedly the authors do us a service by attempting to distinguish between an actually existing inter-national economy and a ideal-typical global one, but the retention of the idea of *international* processes and transactions in the ideal-typification is somewhat confusing.

In spite of its narrow focus this is a helpful contribution to the debate about globalization. In opposing the strong version of the *economic* globalization thesis, important issues are addressed, particularly those concerning governance and the role of the nation-state in the increasingly, but not yet, globalized economy. With the injection of some serious sociological insights, this could have been a very valuable book.

*Two Towns in Germany: Commerce and the Urban Transformation.* By Norbert Dannhaeuser. Westport, Conn.: Bergin and Garvey, 1996. Pp. xii+311. \$65.00.

Mark Shanley  
Northwestern University

*Two Towns in Germany* is a comparative case study of the effects of trade concentration in consumer goods distribution on the commercial life of two towns in post-World War II Germany. One town, Hassfurt, is in the former West Germany, while the other, Hildburghausen, is in the former East Germany. Dannhaeuser paints a detailed and interesting pic-

ture of the threat that concentration poses to the vitality of the small businesses that form the traditional center of such towns.

"Trade concentration" refers to the spread of vertically integrated market channels dominated by large enterprises that control retailing through contractual arrangements, such as through dealerships and franchises, or direct ownership, such as branch networks or chains. It also concerns the proliferation of large retail facilities, such as supermarkets, department stores, and shopping malls. Trade concentration lowers average costs for firms, through economies of scale and scope. Vertically integrated firms can offer a wide range of products and services at lower prices.

Small businesses competing against such firms must improve their efficiency, cut their margins, join with other firms in buying groups, and even reorient their businesses towards more specialized products and services for which they do not face cost pressures. Small businesses that do not adjust to increased competitive pressures will stagnate or even fail. Those that do adjust will often lose their familial and social basis in the process of growing, getting better organized, and cutting costs. As more businesses move out of urban centers and into shopping malls and peripheral areas, customers go with them. This makes it increasingly difficult for small firms in the center to survive. Eventually, the survival of the center itself is at risk. Dannhaeuser views this loss of the social basis of urban commerce as a major negative consequence of trade concentration.

Hassfurt is the principal focus of the book, taking up seven chapters and three quarters of the text. After a chapter of historical background, chapters 2 through 4 survey the individuals and firms involved in the town's traditional trade community, the *Altstadt*. Following these descriptions, chapter 5 examines the survival of family firms in depth. Chapters 6 and 7 examine three enterprises associated with Hassfurt's increasing trade concentration.

Chapters 8–10 examine Hildburghausen. Trade concentration here was first imposed by the East German government. The problems of inefficiency, poor coordination, and low productivity associated with centrally planned command economies all came to plague Hildburghausen. Dannhaeuser's account moves through the demise of communism in 1989 and unification of Germany in 1990. In the 1990s, the people of Hildburghausen have experienced both the problems of Communism and the problems of the Western economic system, characterized by the trade concentration that was affecting Hassfurt. It is little wonder that small businesses in Hildburghausen had such problems during this period.

In chapter 11, Dannhaeuser argues that the experiences of these two towns have general applicability to Europe, to Britain, to the United States, and Canada. Indeed, the United States seems to be the source of many of the trends towards vertical integration that were apparent in these cases. Finally, these cases are even relevant to change in the emerging markets of East Asia, such as Thailand or the Philippines.

This is a rewarding study if one is willing to work through the descrip-

tions in the initial chapters and do the conceptual integration independently. Dannhaeuser could have been more effective in motivating this book by employing a theoretical framework for trade concentration. For example, there is an abundant literature on transaction cost economics and scale and/or scope economies that could shed light on how and why trade concentration occurs and what its consequences might be. Dannhaeuser is aware of this but leaves it to his footnotes.

There are also some interesting findings in these studies that I wish had been better developed. One is the consideration of how the combination of trade and handcraft activities in some small firms helps them to survive competitive pressures. Another is the importance of buying groups, which appear to provide a way for small firms to achieve some of the economies of large firms without recourse to vertical integration.

Dannhaeuser is not impartial. He views trade concentration negatively and sees the preservation of the *Altstadt* areas in the face of change as an issue of great importance. It would have been helpful for him to consider the benefits of changes in these business communities. Have changes helped employment? Do residents feel that access to goods and services has diminished or increased with these changes? Are consumers paying higher or lower prices for their goods and services? I am not persuaded by this account that the effects of changes in these towns have been so negative.

*Israel's Changing Society: Population, Ethnicity, and Development.* By Calvin Goldscheider. Boulder, Colo.: Westview Press, 1996. Pp. xix+271. \$55.00.

Haya Stier  
Tel Aviv University

*Israel's Changing Society* provides a demographic and social portrait of Israeli society. Israel has experienced considerable demographic change during its 48 years of independence. Due to mass immigration, the population of Israel increased from 650,000 to over 5 million people within five decades. This dramatic population growth changed the ethnic composition of the country and affected its stratification system. Two issues are at the heart of this book: the first is a description of the demographic processes that have shaped Israeli society; the second is an account of ethnic diversity and inequality between Jews and Arabs and among Jewish groups. Goldscheider reviews the major demographic processes: immigration, fertility, family formation, and mortality and demonstrates their importance to understanding the ethnic differences in access to societal resources and in residential segregation.

Jewish-Arab relationships and the integration of Jewish ethnic groups are central issues in Israeli social and political life. Within the context of the Zionist ideology, the ratio of Jews to Arabs becomes a crucial issue,

and much attention is given to the population dilemma. Arabs have significantly higher fertility levels than Jews, but, based on long-term trends of fertility in the two groups and the massive immigration of Jews, Goldscheider claims that "the continuous fears expressed about the potential 'decline' in Jewish population in Israel is a demographic myth" (p. 219).

Ethnic integration is part of the melting pot ideology that is central to Israeli policy. Goldscheider examines to what extent assimilation of ethnic groups took place, by looking at demographic behavior and socioeconomic standing. His analyses demonstrate the convergence in fertility, family formation, and mortality of all Jewish ethnic groups. However, the demographic similarities, especially among the second generation, are not followed by greater equality. In fact, the ethnic gaps in education, occupation, and income remain as salient as in the past. These gaps cannot be attributed to demographic differences (such as fertility or age at marriage) or the timing of arrival to Israel and can be seen as a product of residential segregation, different opportunity structures, and discrimination.

Goldscheider raises some thought-provoking arguments regarding integration and assimilation throughout the book. For example, he examines the levels of residential segregation and homogamy in marriage as indicators of ethnic assimilation and argues that, paradoxically, a trend toward higher integration may enhance social and economic inequalities in the long run because of a selective mobility of individuals. This argument is supported by the patterns of interethnic marriages, which are more common among the more educated Jewish women of Asian and African origin. The same is true for residential mobility. Consequently, socioeconomic inequalities are not expected to disappear, and ethnic identity may be strengthened.

While the book provides a thorough review of studies in the demography of Israel and a thoughtful interpretation of the social consequences of demographic process, there are some shortcomings. In some cases, the discussion goes beyond the data and seems somewhat stereotypical or even irrelevant. One example is the comparison made between Israeli Arabs and blacks in the United States. Goldscheider emphasizes the importance of the context in which demographic processes take place, but he compares two groups that are different in their historical experience, in their political status, and in their demographic characteristics. The comparison seems random—Arabs could be compared to many other ethnic groups in the United States or elsewhere with a similar success, while other groups in Israel could be compared to blacks. These comparisons are not necessary to understand the social position of Arabs or other ethnic groups within Israeli society. Another problem is the lack of reference to the religious groups within the Jewish community. Religiosity is no less important than ethnicity, demographically, socially, and politically, both within Israel and between Israel and the Jewish Diaspora. Nonetheless, *Israel's Changing Society* is a valuable source of information about

Israeli society, its uniqueness and similarities in comparison to other industrial societies. The distinctive contribution of the book is in the emphasis on the effect of demographic processes and the timing of their occurrence on the emergence of social structures and group relations. This book adds to our understanding of inequality and ethnic integration, emphasizes the uniqueness of Israeli demographic patterns, and inspires future analyses of social structures and processes—as such it is highly recommended.

*International News and Foreign Correspondents.* By Stephen Hess. Washington, D.C.: Brookings Institution, 1996. Pp. xi+209. \$26.95.

Robert L. Stevenson

*University of North Carolina at Chapel Hill*

According to the resident media watcher at the Brookings Institution, the United States is divided into two media societies, one that is rich in information about the world, the other, very poor. "One society is awash in specialized information available to those who have the time, interest, money, and education to take advantage of it," Stephen Hess (pp. 4–5) writes, while for most of us, "the diet of international news offered cannot be adequate to relate the world to the United States and the world's potential importance to their lives."

Hess, author of several books on the media and politics and a former operative in the Eisenhower and Nixon administrations, bases his conclusions on a conventional quantitative analysis of major U.S. media foreign affairs coverage and a curious survey of present and recent foreign correspondents. The survey is curious because he includes the names of all 700-plus respondents and frequently quotes them at length and by name. At the beginning of a very short book (the text is 113 pages), he poses these questions: What information about the world are we given by the mainstream media? How much? How good? By whom? Through what means? The answers are generally negative except for people living in the largest metropolitan areas, although he acknowledges that new technologies have given the rest of us access to a volume and variety of international news beyond anything that may or may not have existed in a past golden age. He quotes mainstream publishers and broadcasters who complain of declining audiences, especially for serious international coverage, but does not let them use it as an excuse for reducing international coverage to a few fleeting seconds or a few column inches devoted to today's one or two hot spots.

While the content analysis of foreign coverage and discussion of it largely reinforce existing research, his portrait of the U.S. foreign press corps, which is actually expanding in size, is interesting because it gives the reader a good sense of the business of covering the world. Today's foreign correspondents, unlike the characters in old movies, tend to be

upper-middle-class graduates of elite schools, usually with good backgrounds in the areas of their assignments; more of them are women and are enmeshed in the joys and frustrations of raising a family overseas, often working as husband-wife teams. The amount of information these correspondents feed into a complex global system dominated by a handful of Anglo-American institutions is staggering in volume and diversity.

The problem comes at the retail level where newspapers, except for a tiny handful that maintain their own pool of correspondents, and even the commercial networks pick one or two blockbuster stories of the day and ignore the rest of the world. The fleeting glimpse of the world most of us get is fragmented, episodic, violent, and devoid of background, context, and interpretation. The admittedly tiny segment of the population seriously interested in the rest of the world must seek out foreign coverage from specialized sources that are probably more readily available outside the New York–Washington corridor than Hess acknowledges. More and more of the media are willing to leave responsibility for coherent, sustained international coverage to the *New York Times*, CNN, and a few fringe publications and cable channels.

While not explicit, an underlying theme is that the United States is less well served by its media than other countries. This may be true—the few comparative figures Hess cites are not encouraging—but it is important to remember that serious newspapers outside the United States, such as the *Guardian*, *Le Monde*, or *Frankfurter Allgemeine Zeitung*, print a few hundred thousand copies for national distribution, while tabloids, like the *Sun* and *Bild Zeitung* (worse than anything published in the United States), have press runs of several millions. European public service broadcasting, traditionally heavy on serious, if dull, foreign affairs programming, is on the defensive both from governments unwilling to continue financing and from publics more attuned to new entertainment-heavy commercial services. The problems Hess describes are not uniquely American.

The conclusions of this book are neither startling nor radical, although Hess is more willing than many critics to argue that a constitutionally protected private industry has a social responsibility that ought to override—at least occasionally—the corporate balance sheets. He has some hope that U.S. global interests will eventually reverse the trend of diminished media and public attention toward foreign affairs and has considerable faith in new technologies to supply information to the minority of the population still interested in the rest of the world. Hess's judicious mixture of anecdotes, statistics, and personal observations helps us understand something about the dim, flickering picture of the world the media create for us every day and something more about the people who bring it to us.



*Who Deliberates? Mass Media in Modern Democracy.* By Benjamin I. Page. Chicago: University of Chicago Press, 1996. Pp. x+167. \$29.00 (cloth); \$10.95 (paper).

Robert P. Snow  
*Arizona State University*

The answer to the question of who participates in public policy debates is mixed. Examining case studies of the prelude to the Persian Gulf War, the Los Angeles riots following the acquittal of officers who beat up Rodney King, and Clinton's failed attorney general nomination of Zoe Baird, the author concludes that, while the elite press still wields considerable power in shaping debates and interpreting policy, they cannot always lead the public. Indeed, the public forum works fairly well, especially when American core values (as in the Zoe Baird case) are at stake.

Benjamin Page, a political scientist, has constructed a carefully crafted analysis of three major media stories, focusing primarily on the role of the elite press (*New York Times*, *Wall Street Journal*, and *Washington Post*) but not excluding other media players including broadcast journalism. In the three case studies, the Persian Gulf story demonstrates how the editorial policy of the *New York Times* mirrored "official" policy to the exclusion of other voices and viewpoints. In short, it was not balanced. On the Los Angeles riots story, the elite press did not accept the official position of the Bush White House and led a crusade to reject a politicized explanation of the riots. In the third case, while Zoe Baird's employment of illegal aliens was downplayed by the media, a public uproar emerged through broadcast media talk shows. In this case, elite print media bowed to emergent public opinion. Central to each case study is the observation that the media forum is dominated by "professional communicators," consisting of journalists, public officials, and experts in that order of prominence. In addition, Page concludes that media initiate and play out roles as "political actors" in the overall policy deliberation process. In this role, journalists do not represent or allow the expression of the attitudes and values of ordinary Americans. Nevertheless, the public sees through the process when it is badly out of line and manages to develop an informed opinion regardless of hype or press bias. Accordingly, the author is not as worried as some media critics about the power of the press, although he spends some time in the final pages giving suggestions on how we can and should be more vigilant.

The book provides good evidence to support the well-established analytical framework that, regardless of the message, we still need to find out the meanings constructed by the audience. On the other hand, Page continues to emphasize content over form and format. As some of us have been arguing for some time, strategies exist for making sense of activity that becomes news, and these strategies are interconnected with the strategies of presenting this news to audiences. An example can be found in all three of Page's case studies.

The framework employed by the press in the prelude to the Persian Gulf War, the Los Angeles riots, and the nomination of Zoe Baird was essentially one of constructing polar extremes or clear-cut contrasts, loosely based on the principle of two sides to a story. Media do not report events as they unfold but provide a framework for seeing contrasts, which are often oversimplified. This becomes a scenario for making sense of further developments. Almost any story can be seen as a tension between A versus B. In the prelude to the Gulf War, it was hawkish saber rattling versus diplomacy by cooler heads. The Los Angeles riots story was politicized as conservative versus liberal urban policy. The most interesting and possibly the most insidious was the Zoe Baird story in which media early on could be seen as "baiting" the public with the suggestion that, on a pragmatic level, Baird's employment of illegal domestic workers should not be a problem. But the press knows full well that when the spotlight is national, practical norms give way to ideal norms and values. While Page argues that, in the Baird case, public opinion overcame elite press stories, he focuses on the content of those stories to the exclusion of the underlying media framework that suggested there was another side to the story. Despite the content of the early stories on Baird, they implied potential conflict, and eventually it was achieved. As we all know, this conflict often manifests through sports metaphors—the horse race, boxing match, and so on.

My read on the role of media in deliberating public policy is that we cannot ignore how media establish and perpetuate particular interpretive frameworks through format strategies. Page is correct that the impact of ideology in the press is exaggerated, and his faith in the public to see through media bias is well founded. He is correct in reaffirming the power of the elite press in influencing how stories are constructed and in stressing that they are a social construction of reality. He also provides excellent descriptions of how media accounts unfolded in the three case studies. In turn, these descriptions afford other students of media the opportunity to explore how other (not competing) theoretical approaches may be supported. This is good science; we need more of it.

*The Persistence of Purgatory.* By Richard K. Fenn. New York: Cambridge University Press, 1995. Pp. viii+209. \$49.95 (cloth); \$16.95 (paper).

Alois Hahn  
*University of Trier*

As Jacques Le Goff (*La naissance du Purgatoire* [Gallimard, 1981]) has shown in his now classic book on the topic, the word *purgatory* and the notion of a third realm between heaven and hell were inventions of the 12th century (though the vague idea of the possibility of penitence after death had already been conceived by authors such as St. Augustine), which accompanied the revolution of the epoch, consisting of a new dra-

matization of the individual, his or her personal guilt, and responsibility for the use of time. Purgatory, then, offered an otherworldly existence for the sinning soul who had to purify herself or himself from the sins not repented for in this life. It was considered an intermediary state belonging not to eternity but to the same "time zone" as the world of the living.

Fenn's portrayal of the medieval view of purgatory relies largely on the work of Le Goff. The originality of Fenn's essay lies in his thesis that a secularized conception of purgatory is one of the sources of modernity and the modern self. Modernity is depicted in a way that owes much to Max Weber's analysis of the Protestant ethic. But whereas Weber saw the roots of the modern obsession with time, punctuality, rationality, professional and economic success, and inner-worldly ascetics in the Puritan belief in predetermination, Fenn traces these modern obsessions back to the idea of purgatory. The *explanandum* is the same, but the *explanans* is radically different.

Fenn's starting point is that the idea of purgatory was not totally abandoned by protestants after the Reformation but transformed and, finally, secularized, just as Weber had conceived of the secularization of the Protestant ethos. Fenn shows, e.g., in a painstaking analysis of the work of Baxter and Locke, that even a Puritan preacher or a liberal Protestant philosopher believed in the existence of a realm of repentance in the hereafter and that the time owed to repentance there might be shortened by a life of repentance in this world. Time, then, becomes scarce because it should be dedicated to the never-ending process of self-purification and self-perfection. The "punctual" and the "reflexive" selves that C. Taylor had detected in Locke's works are, according to Fenn, "purgatorial" selves. The central idea of the secularization of purgatory is that one can begin with purgatorial repentance in this world as a kind of investment for the beyond: "The continuous self-scrutiny that Locke prescribes for the self is just such a form not only of self-reformation but of self-torment. There is a masochistic element in the modern self that has internalized the oversight of authoritative and controlling presences and made that its own" (p. 109).

The same kind of interpretation is then put to American writers of the 19th century, especially to William E. Channing, where Fenn finds the same ideas of endless spiritual growth in this world as a means to reduce endless self-reproach in the life after death. Inner-worldly spiritual growth is seen as a process to prepare and purify the soul for its entrance into heaven (cf. p. 133). Insofar as America was seen as the possibility of the infinite, a new "temporal panic" (p. 180) and a "masochistic social character" seem inevitable, since infinite possibilities can never be realized. In its secularized form, "soul searching for hidden sins has turned into endemic self-doubt and into a chronic hunger for spiritual validation by a secular priesthood of experts on human development" (p. 180). The individual is transformed "from a chronic sinner into a perennial patient. Purgatory has become secularized into a perennial drama" (p. 182).

Fenn's thesis is provocative and shows the scholarly brilliance of an

expert in theology and sociology, but some questions remain: Can one really say that the idea of purgatory is a *root* of modernity in the sense of an historical *cause*? Or is the term *purgatory* merely a fascinating historical metaphor or analogy to describe modernity from a critical point of view, a weaker form of "life is hell"? Of course, the idea of self-discipline and the prospect of otherworldly retribution has played a great role in Christian piety in protestant and Catholic contexts. But, as a sociologist, one would be interested in the institutional devices that translate ideas into habits (e.g., the techniques of the auricular confession and therapeutic practices). It might also have been interesting to compare "purgatorial influences" to the theories of Michel Foucault, where similar forms of self-control and self-surveillance are regarded in the context of the birth of total institutions. Nevertheless, Fenn's essay is a very significant contribution to the theory of the sources of the modern self and will stimulate a vivid discussion.

*Burning the Box of Beautiful Things: The Development of a Postmodern Sensibility.* By Alex Seago. New York: Oxford University Press, 1995. Pp. xxii+234. \$55.00 (cloth); \$29.95 (paper).

Jean-Louis Fabiani

*School for Advanced Study in the Social Sciences, Marseilles*

In the introduction to a recent French edition of Seneca's philosophical works, Paul Veyne, renowned historian of ancient Rome, argues provocatively that, for one century, the sociology of culture has been totally sterile mainly because the discipline has been unable to give any satisfactory account of cultural change in sociological terms (*Entretiens: Lettres à Lucilius* [Robert Laffont, 1993]). Although we may not share Veyne's pessimistic view, we notice every day the explanatory limits of most sociological theories when they apply to the transformations of symbolic forms, either mechanically linked with demographical or institutional change or tautologically related to *zeitgeist* and fashion or considered as a mere output of the endless struggle between the established and the outsiders.

Alex Seago's book is a contribution to the study of change in sensibilities, since he tries to discover and to understand the origins of what he calls "postmodern culture" in Britain. Such an ambitious plan is based upon an analysis of a rather narrow corpus: the issues of *ARK*, published by the Royal College of Art (RCA) Department of Graphic Design between 1950 and 1963. RCA students made their own debuts in this irreverent and innovative publication: some of them became prominent artists. *ARK* had a rather large circulation for a magazine of its type and had a real influence among other British art schools.

Excellentlly documented and richly illustrated (106 black and white plates and 14 color plates give a precise idea about how *ARK* promoted

new trends in graphic design), Seago's book is a real contribution to cultural history. It is neither the study of institutional art nor the usual statements about mass and pop cultures; graphic design, often neglected by cultural historians, appears as a decisive spot where new aesthetic values are produced. Considerations about the anti-institutional or antimandarin mood that characterizes the specific bohemian style of British pop culture in the sixties are not very fruitful if they are not referred to the social settings where symbolic forms are designed. According to Seago, British art schools have played a major role in the process of postwar cultural change. Rather peripheral in the educational system, art schools attracted marginal people who would not otherwise have found their way in higher education. Art schools are strongly opposed in this respect to the art departments of British universities; social recruitment and curricula were clearly different. Art schools attracted mostly children from working-class and lower-middle-class backgrounds. Besides, in the early fifties, art school students were often ex-servicemen attracted by special grants intended to help careers disrupted by war: older and more mature than average students, those men were likely to be outspoken and rebellious and contributed to the unique atmosphere of the postwar art schools, a combination of countercultural mood and of great social expectations. Teaching was unstructured and rather atheoretical; art schools offered a typical student-led education, where performance and satire played a major role. The account of the social and pedagogical features of the schools is indisputably the best part of the book.

Alex Seago also offers interesting insights about the redefinition of the components of British national culture in the postwar era; a debate over English good taste develops in the first issues of the *ARK*. If the first issues show a clear hostility to mass culture and a nostalgic tribute to ruralism and romance, *ARK* is more and more receptive over time to pop culture, particularly through the influence of American graphic design and students from working-class backgrounds who brought in "downtown pop."

Descriptive precision constitutes the great merit of this book, but sometimes it looks like an accumulation of biographical or graphic details that do not really correspond to the author's first ambition: to give an account of cultural change in postwar Britain. There is obviously a gap between the research agenda presented in the first chapter and the empirical study of the *ARK* corpus. Perhaps the difficulty arises from the use of "postmodernism." Alex Seago remarks that it is problematic to talk about an English postmodernism since modernism itself has never been a component of British culture, but he makes no use of this objection. There is another objection that appears from time to time in the book: the students of art schools in the fifties and early sixties had no idea of postmodernism, since the notion was not available yet. Although the author is quite aware of the confusions that arise from the term postmodernism, he goes on using it in a not constantly controlled way. Becoming synonymous with a rather vague spirit of the time, postmodernism might be applied to any cultural

movement or form. The very original history of British art schools, finely analyzed by Alex Seago, deserves more precise concepts.

*Beethoven and the Construction of Genius: Musical Politics in Vienna, 1792–1803.* By Tia DeNora. Berkeley and Los Angeles: University of California Press, 1995. Pp. xi+232.

*The Glory of van Gogh: An Anthropology of Admiration.* By Nathalie Heinich. Translated by Paul Leduc Browne. Princeton, N.J.: Princeton University Press, 1996. Pp. xi+218. \$29.95.

Vera L. Zolberg  
*New School for Social Research*

These studies in the making of genius, each a rich and original contribution to the sociology of culture and the arts, encompass the 19th century, the case of Ludwig van Beethoven at its beginning and that of Vincent van Gogh at its end. In accepting the challenge of interpreting what are arguably the two dominant names in the worlds of serious music and fine art, the authors are skirting perilous terrain. Social scientists who deal with highly valued (or denigrated) phenomena are themselves not likely to be neutral, and their readers, too, have opinions. They are obliged to take special care to avoid allowing their biases to become flaws in what is expected to be dispassionate inquiry. At the same time, they must be prepared to cope with strong reactions, in the case of the arts, from art lovers who suspect social scientists of wanting to reduce significant objects to the ordinary (Heinich, p. xiii). Nathalie Heinich, a sociologist at the Centre National de la Recherche Scientifique in Paris, and Tia DeNora, a lecturer at the University of Exeter, are aware that sociological analyses differ from the sometimes no less scholarly work of art or music critics, historians, aestheticians, or popular writers, and both address these differences.

DeNora's view of Beethoven's genius, as her subtitle implies, is out of keeping with most musicological and aestheticizing discourses. This is not the Beethoven conventionally described as a heroic, irresistible force, a courageous revolutionary. Although DeNora does not deny these qualities and carefully distances herself from the role of debunker (p. xiii), neither does she evaluate the worthiness of his works for entry into the "canon," a task she assigns to "cultural critics." Instead, she provides a carefully thought-out study of the interplay of a unique phenomenon (Beethoven) within the social context in which it emerged.

In her view, social context is not the spongy mass that includes everything not internal to the art work but stipulates little among the mass of surrounding structures. Instead, she specifies the aspects of context that she considers most relevant to her aim, which is "to document the structuration of the Viennese musical field as this structuration occurred in

and through the development of Beethoven's prestige" (p. xii). The critical period of her analysis, barely a decade in length, is one in which the music field underwent a striking transformation. Bounded by a "prehistory" of music identified by historians who constructed the canon and the later career of the composer, that nine-year period encompasses the gestation of his music as high art in embryo. Not only did Beethoven's music come to be defined as "serious," a distinction that existed already, but, in particular, as "great" in the *secular* realm.

Rejecting the mysterious quality of genius for which popular writers and publics have long had a predilection, DeNora looks at it with the sharp eye of the historically aware ethnographer. He may have been unique in his achievements and in his impact on music and taste, but the making of this Beethoven should not be taken as an inevitable unfolding process. Instead, following the strictures of Bourdieu's rules for sociological practice (p. 7), DeNora compares Beethoven to certain crucial predecessors (Haydn, Mozart) in the context of the evolving music institution and, even more important, to contemporary composers and performers, including some who may have been more popular than Beethoven, such as Dussek (chap. 4) and Wölffli (chap. 7). In the process, by analyzing critics and diarists, she studies reception by trying to see (or hear) within the categories of perception of the audiences at the time.

This leads her to distinguish the taste of his early patrons, almost all of them aristocrats (a standing that she carefully breaks down into its several layers), and their relation to changing musical institutions. DeNora meticulously details the rise of an ideology of seriousness in musical taste, promoted by a group of aristocrats, some of the most active being relatively recently ennobled. That Beethoven was the son and grandson of court musicians is a feature of the social capital that he was able to martial, especially in his early foray from the relatively small Bonn music scene into that of prestigious imperial Vienna. Together with a few of his most devoted patrons, they brought about musical "reform," one aspect of which was to replace the frivolity of Viennese aristocratic taste with the taste for the sublime in music, associated with Beethoven's northern German origin. Institutionally, the period coincided with the demise of aristocratic private house orchestras, and the emergence of privately subscribed concerts, which developed into larger semipublic concerts, culminating in the public concerts catering to the middle classes that came to predominate in the latter part of the 19th century. But it was not the middle classes who were Beethoven's patrons during most of the period under consideration.

In addition to his aristocratic connections and his own purported standing (Beethoven allowed the belief to take hold that the "van" in his name actually signified "von"), qualities that his contemporaries, Dussek and Wölffli, lacked, later hagiography attributed to him a further set of charisma-provoking qualities. Beethoven was said to have been handed the mantle of Mozart and Haydn (chap. 5). As DeNora points out, however, despite his musical background and his father's efforts to make him

a child prodigy on the Mozart model, the child Beethoven was far from precocious. As an adult in Vienna, somewhat surreptitiously, he was obliged to study counterpoint to fill gaps in his musical education (p. 104). Despite this, by cannily using his aristocratic patronage and career strategies, he succeeded in bringing his work into a musical canon that went back to Bach.

The strengths that DeNora brings to her analysis include astute use of archival resources and secondary analyses of the emergent musical institutions of the 19th century, in which the commercial world of publishing, instrument manufacturing, and concert performances were intertwined. She integrates musical form and performance features with the social strategizing without which music developments cannot be grasped. Her ethnographic command of the musical field permits her to deconstruct it with remarkable sensitivity, elegance, and ingenuity, but, since her notion of "context" barely surpasses the music world of Vienna, with some comparisons to London and Paris, legitimate as these choices are, she eschews the politics external to the music world. This means that she does not ask why the aristocracy's tastes might be changing in the last decades of the 18th century. In light of the earthshaking events of the French Revolution, the fact that Marie Antoinette was the sister of the Austrian ruler might have had special meaning for the Austrian nobility. Although this does not explain why the musical tastes were beginning to change in the 1780s, it is unlikely that the dynamic of change can be fully understood as an endogenous process.

The work by Nathalie Heinich is dissimilar in most ways from DeNora's. Dealing with a visual artist associated with the "independent," that is, antiacademic artists of late-19th-century Paris, who are still the dominant force in the painting world, it analyzes the posthumous construction of his standing. This does not mean that van Gogh was totally passive in developing his career. Like Beethoven, he left a periphery for the art center (Paris), used his knowledge of the art market (he had worked as a dealer in a firm with which his family had been associated), and sought aesthetic advice both from other artists and his art dealer brother. Heinich is not pursuing these biographical aspects, except insofar as they impinge upon her subject in this work: the construction of the artist as a "saint." Her interest is not the particular individual but what she calls "the van Gogh 'effect'" that has come to be "summed up in him, and no longer involves just one individual destiny, but, more generally, the status imparted to great singular figures" (p. xi).

Singularity, she notes, presents problems for social science, whose vocation is to reconstruct uniqueness into generalizable objects, the task she sets herself. Unlike DeNora, who provides considerable quantitative evidence of concerts, subscriptions, and other aspects of musical life during Beethoven's time, Heinich dispenses tables of figures on sales and publications about the artist after his death (p. 16) and, still later, films (p. 100). More important, her method is less dependent upon such data because she is doing an "anthropology of admiration" in which she treats



the familiar (our own society) as if it were strange. Her "participant observation" (p. xii) involves looking at the behavior of viewers, as well as at documentary evidence—the van Gogh as "preconstructed" by art world participants, such as critics and scholars. She discerns in these sources the construction of "a hermeneutic space" in which van Gogh's name has been taken up, first as an enigma, then, through wider and wider spheres of commentary, as an authentic artist.

What she finds is that a certain kind of legend has become a *sine qua non* for artists, a principal part of which is the necessity of having suffered from the incomprehension of their contemporaries. According to Heinrich, for a large audience, van Gogh is the first artistic hero: poor, depending for support on his brother, failing in his first vocation as a pastor, suffering from mental torment, committing suicide at the early age of 37. She sees this series of stages as akin to stages in the life of a saint: a sense of calling, recognition as an uncommon man, marginality, asceticism, disinterestedness, incomprehension, martyrdom, and fulfillment in posterity (p. 37).

Leaving aside the facts that van Gogh had become integrated into a circle of avant-garde artists and, even though his career in art was short, had actually received a favorable review some months before his death, this hagiography seems mythical indeed. Nevertheless, in popular thinking, the self-sacrifice and suffering, alternating with fits of rage and visits to brothels, came to be accepted as the fault of those who should have succored him (p. 89) and by society as a whole. Pursuing the analogy to sainthood further, Heinrich interprets the steadily increasing prices for his works as a means of atonement by society through the purchase of relics—from millions of dollars for original paintings, to small sums for reproductions—and "pilgrimages" to museums and to his old haunts by a large public. As in the middle ages, art and religion cross each other's path, and van Gogh's role as a Protestant saint becomes established.

Although the authors do not share the same goals, frameworks, or perspectives, each brings strengths to our understanding of how culture is created and maintained, how taste may be used to bolster social standing, not in a simple way, but through the construction of new criteria of value, how social identity and roles are made, how they change or may be perpetuated over the long term, and how aesthetic claims may become authoritative. These volumes are well edited and, in the case of the Heinrich study, lucidly translated.

*Constructing School Success: The Consequences of Untracking Low-Achieving Students.* By Hugh Mehan, Irene Villanueva, Angela Lintz, and Lea Hubbard. Cambridge: Cambridge University Press, 1996. Pp. vii+247. \$49.95 (cloth); \$17.95 (paper).

George Farkas  
*University of Texas, Dallas*

This is a curious book. The authors undertake an interesting and difficult task—to evaluate the success of Advancement Via Individual Determination (AVID), an “untracking” program in San Diego high schools. In a candid section in chapter 2, they state that “we have attributed the success of the program to different factors than the program does. . . . We have raised questions about certain elements of the program . . . and we have discovered unpleasant tensions within it” (p. 24). They discuss the difficulty of “reporting negatives to people who have given you their trust” and say that, “because of these entangled personal relationships and the commitment of our group to educational improvement, we are certain that some will criticize our approach for bias and lack of objectivity.” Yet this early attention to possibilities of bias occurs after a completely biased literature review—one that fulsomely cites the work of Jeannie Oakes, a leading critic of tracking, while ignoring the work of Maureen Hallinan, who, while hardly a “supporter” of tracking, has injected much-needed empirical skepticism into the “untracking” discussion. In particular, Hallinan argues that “the compelling advantage of retaining tracking, at least in certain subjects, is that it facilitates instruction and learning” (see p. 84 of the 1994 exchange between Hallinan and Oakes in *Sociology of Education* [67:79–91]). Mehan et al. begin chapter 3 of their volume by acknowledging that measures of actual learning—test scores and course grades—are important schooling outcomes, along with dropout rates and college enrollment rates. Incredibly, they then declare that they will rely solely upon college enrollment rates to measure the success of AVID. Further, they go on to report study sample attrition rates that cast doubt upon their ability to unbiasedly estimate such rates. They then use these data anyway, reporting first-year rates suggesting a positive effect of AVID. However, these are simple rates, unadjusted for control variables. Second-year and third-year results appear to be less positive. Nevertheless, the authors declare the program a success. Having presented no direct evidence on student learning, they begin chapter 4 by stating that their remaining goal is to determine “how untracking help[s] students improve their academic performance” (p. 55).

Methodological and inferential anomalies continue throughout the remainder of the volume. Chapter 4 uses parental background measures as well as student course grades and test scores prior to AVID to examine which students are enrolled in AVID, yet these are presented as a series of bivariate relationships, with no use of multiple regression to truly separate the effects. We are left with the distinct possibility that nonrandom

selection into AVID combined with differential attrition of study sample members bias the program effects reported. Further, there is no discussion of why, if test scores and grades are available prior to AVID, similar variables from high school are not used to measure the program's instructional success.

Chapters 5–10, which are wholly ethnographic, are more successful. Particularly useful discussions include those concerning the importance of AVID teachers' advocacy for their students (chap. 5), forms of resistance to educational innovations (chap. 6), peer group resistance to educational achievement (chap. 7), difficulties with parental involvement (chap. 8), issues in attempting to duplicate AVID elsewhere (chap. 9), and the potential malleability of cultural and social capital (chap. 10).

Recently, journalist James Traub (*City on a Hill: Testing the American Dream at City College* [Addison-Wesley, 1994]) presented a negative view of the outcomes experienced by ill-prepared students attending college under open admissions. Traub's reporting reminds us that the difficult issue in "constructing school success" for at-risk students involves assisting these students to gain the academic skills necessary to succeed at college-level work. Unfortunately, the present volume gives us little direct and reliable information on AVID's success at this task. My own speculation is that programs like AVID are too little, too late. It is difficult to transform a student's school career when he or she has already experienced daily failure for eight or more years. Instead, advocates for low-income, ethnic minority, and at-risk children should be concerned with providing them with strong reading and mathematics skills from preschool and early elementary school onwards. In addition, where necessary, intervention should be early and powerfully focused on skills and the work habits and self-esteem necessary to sustain and advance them. But where are the volumes arguing for more and better cognitive instruction within Head Start? Where are the efforts to make Title I, which is funded at twice the level of Head Start, fulfill the mission it has failed at during 30 years of operation? Where are the studies exploring the damage done when bilingual education fails to create a transition to English? Where is the outrage that, as a consequence of adopting whole language instruction, California's reading scores fell to next to last among the states? And who among us includes these subjects in their social stratification curriculum?

*Changing the Odds: Open Admissions and the Life Chances of the Disadvantaged.* By David E. Lavin and David Hyllegard. New Haven, Conn.: Yale University Press, 1996. Pp. vii+292. \$35.00.

Barbara Heyns  
New York University

For the better part of three decades, David Lavin has devoted himself to the analysis of equity issues in higher education and, in particular, to

open admissions at the City University of New York (CUNY). He has followed two cohorts of students for a decade or more and coauthored two books as well as numerous reports and papers; in the process, he has become the best known CUNY-ologist in the country. This book summarizes previous results and supplies comparative data for the second cohort of students; the fundamental theoretical question concerns the role of education in the transmission of privilege, but the political and institutional context of reform is described in detail.

The authors trace the impact of open admissions through its various stages, starting with the initial policies (1970–75) prior to the New York City fiscal crisis in the mid-1970s. By comparing the first cohort of students with those who entered in 1980, when retrenchment was firmly in place, the authors contrast the effects of open admissions in the best and worst of times. Three data sets are included in the analysis: first, extensive questionnaire data were merged with the academic records of students in the first three freshmen classes entering CUNY under open admissions in 1970–72, second is a 1984 follow-up survey of 5,000 students drawn from these files, and, the third data set contains questionnaire data and academic records for the class entering in 1980, with their academic progress at CUNY over 10 years.

The differences between 1970 and 1980 reflect both demographic trends and change in institutional policies. Enrollments plummeted, but the bulk of the decline was not among minority youth confronted in 1976 with tuition fees but among white students choosing to enroll elsewhere. Second, fewer students enrolled in baccalaureate programs; in the initial cohort, more than half of the students chose four-year programs; a decade later, only 35% entered senior colleges. Moreover, despite the increasing proportion of minority students, access to these tiers became more sharply defined by race and ethnicity. In part, the change was a result of more stringent standards for admissions in 1980, and, in part, the result of fewer qualified students in the applicant pool. In 1980, 30% of the freshman class had either missing high school records or had earned a general equivalency degree (GED). These students were more likely to be Hispanic and/or immigrants and more likely to study part-time. Within the community college tier, the popularity of vocational programs also increased during the decade, especially among white students. In short, both minority and white students entering CUNY in 1980 were less interested in liberal arts programs and transfer credits and more interested in potential jobs. Not surprisingly, the 1980 cohort was more likely to study part-time, to be assigned noncredit remedial classes, and to lag behind their 1970 peers in both academic credits and completion rates. These differences were especially large for low-income and minority students; family income had no effect on the probability of graduating in 1970, but in 1980, it was significant. As Lavin and Hyllegard note, the evidence suggests that educational opportunity declined substantially between 1970 and 1980. The academic progress of the 1980 cohort seems to have been retarded in part because they were less prepared at entry, in part

because they were forced to juggle demanding work and class schedules, and in part because the actual number of courses offered declined. The authors' discussion of how individual characteristics and institutional policy interact and reinforce changes in educational opportunity is admirably balanced, sensitive, and cogent.

The CUNY program was arguably the most ambitious effort to expand equality of educational opportunity ever launched. Although viewed as affirmative action *par excellence*, open admissions actually benefited more white students than minorities. From the outset, it attracted vehement critics who defined open admissions as a no-win policy. If the program increased the number of graduates, the degree would necessarily be devalued because standards had been compromised; alternatively, if many entering students flunked out because they were unprepared for college work or "did not belong" in higher education, the program failed in its own terms and could be judged responsible for thwarting the hopes of aspiring students. The truth of the matter, naturally, lies somewhere in between. Open admissions at CUNY provided access to credentials that had been previously unavailable to an unprecedented number of poor and minority students; for many, these degrees provided a secure foothold in well-paying jobs. Open admissions did not, of course, end class privilege or the educational and economic advantages of the white middle class. Nor did a sweeping erosion of academic standards occur; the hurdles to receiving a degree at CUNY became more difficult, and the degree became more costly and time-consuming during the decade. The B.A. degree was neither easier to obtain nor devalued and cheapened between 1970 and 1980; if anything, the returns to college attendance have increased since the 1970s, for both minorities and the middle class.

Although open admissions was surely a pioneering effort at CUNY, the prolonged controversy and intense debates are sometimes difficult to understand outside of New York. In other communities, expanding access to public higher education attracted neither the rhetoric nor the enduring opposition aroused by CUNY. Moreover, tuition and reduced financial aid did not alter the attendance of blacks and Hispanics, although white enrollments fell and overall completion rates slowed; moreover, educational budgets were not particularly ruinous. Although open admissions has been celebrated as the most ambitious effort to expand educational opportunity, in retrospect, it merely sped the development of mass higher education and community colleges. It is tempting to analyze the controversy as a product of class and racial politics, but, from another perspective, the debates about equity and excellence should be framed by CUNY's unique legacy and the liberal traditions of New York City. Open admissions cleared the path for educational expansion and for a view of higher education as an entitlement; moreover, as the authors convincingly show, it provided enormous benefits to both students and the city. At the same time, the debates are especially poignant when a progressive institutional legacy and liberal historical traditions are pitted against social change.

*The Ironies of Affirmative Action: Politics, Culture, and Justice in America.* By John David Skrentny. Chicago: University of Chicago Press, 1996. Pp. ix+312. \$48.00 (cloth); \$16.95 (paper).

Keith Reeves  
*Harvard University*

Since the injection of affirmative action into American political discourse, it has been controversial public policy. Why? "Because of its racial beneficiary," answers sociologist John David Skrentny (p. 63). "The interest of . . . mainstream America in resisting affirmative action is largely a moral one: blacks are not seen as deserving of preference," he adds (p. 67). Such is one of many engaging ideas explored by the author in this new, timely, and highly informative work.

In this provocative study, Skrentny steps back from the polemic debate about affirmative action policy to raise fundamental questions that are rarely contemplated. For example, where did the policy (especially regarding equal employment opportunity) come from in the first place? What social, cultural, and historical factors contributed to the implementation of affirmative action during the period from 1964 to 1971? Given that the policy never had strong mass popular support, how did it gain such a foothold in American politics? Lastly, what cultural, historical, and political bases explain conservatives' resistance to affirmative action and liberal's endorsement of it? The author's investigation reveals "a series of *ironies* and unintended consequences, with unlikely heroes—or villains, depending upon one's point of view" (p. 2; my emphasis).

Employing sociological and historical analytical techniques, Skrentny aptly points out what I believe is the most obvious of ironies. Conservatives' opposition to affirmative action policy is largely predicated on the notion that preferential treatment for blacks as a categorical group is fundamentally unfair and antithetical to the principle of individual merit. And yet, *uncontroversial exceptions to merit permeate much of American society*: victims of natural disasters get special consideration, farmers receive subsidies, and the elderly procure discounts of all sorts. What is more, exceptions to merit have pervaded employment hiring and promotion practices in this country historically (e.g., veterans and their relatives or nepotism regarding children of college and university alumni). "Law and policies regularly divide Americans," the author writes (p. 3).

Given such overwhelming evidence of "difference consciousness," any astute observer of American racial politics cannot help but arrive at Skrentny's penetrating conclusion: "Some groups . . . have been constructed as being more deserving than others" (p. 36). Hence, conservatives claim they oppose affirmative action because it is unmeritocratic. Not so, suggests the author. Rather conservatives' resistance can be explained—to a great degree—on the basis of race.

Skrentny's investigation suggests that prior to 1964, it was taboo for politicians, policymakers, and certainly mainstream civil rights leader-

ship to advocate anything other than "color-blind" public policy. The presumption was that the demise of legal race discrimination would bring about "equality of opportunity" for black Americans. More consequential, however, was that cultural resistance deemed "race-targeted" ideas illegitimate and unacceptable, and, until urban rioting in the 1960s, this resistance did not wane.

As such, another "ironic" point is unveiled—although this time it relates to individuals on the left of the political spectrum. *Rioting by blacks prompted government and business elites to advocate large-scale race-targeted proposals.* Consequently, a "crisis management" alliance emerged between Kennedy and Johnson administration officials and the nation's business leadership to promote racial hiring as a means of quelling racial unrest. Indeed, "by emphasizing crisis, the risky issue of moral worthiness was avoided," Skrentny argues (p. 76). In addition, bureaucrats, charged with implementing and enforcing the antidiscrimination laws now on the federal books, were also "led to [race-targeted] affirmative action" (p. 111). In the interest of administrative pragmatism (i.e., the pursuit of cost-effective means to meet one's objectives), tangible results (i.e., how many blacks have been hired) became all the more important (pp. 111–44). The U.S. Supreme Court, too, joined the fray. Acting on the basis of legal precedent, the Court helped to shift political and policy boundaries such that race-conscious affirmative action could be advocated—safely. What is all the more intriguing is that such dramatic developments took place against the backdrop of a politically opposed American public.

This brings us to what is certainly the most fascinating "irony" concerning the history of affirmative action policy in this country. At the most critical juncture of its development, affirmative action "*owes its most advanced and explicit race-based formulation to a Republican President who based much of his campaign on appealing to the racially conservative South*" (p. 177; emphasis added). Why would Richard Nixon, who barely emerged victorious in the 1968 presidential election, sanction a government policy that required federal contractors to show that blacks, in fact, were being hired (the Philadelphia Plan of 1969)? In a nutshell, Nixon's "Southern strategy" was devised to respond both to the urban racial unrest, while simultaneously playing blacks and organized labor against each other; all for the benefit of appealing to disaffected Southern whites. For those desiring a reasoned, crisp, well-articulated understanding of the origins of affirmative action policy in the United States, Skrentny's study will make for a stimulating and thought-provoking read.

*A Town Abandoned: Flint, Michigan, Confronts Deindustrialization.* By Steven P. Dandaneau. Albany: State University of New York Press, 1996. Pp. xix+259. \$19.95.

Martin Kenney  
*University of California, Davis*

The deindustrialization of Flint, Michigan, provides a unique optic with which to examine the effects on communities and workers of the U.S. auto industries' decline in the 1980s. Flint was the second largest auto-producing city in the nation and almost entirely dependent on General Motors (GM). At the end of World War II, its per capita income was 30% higher than the national average, but, by the 1990s, it had become synonymous with deindustrialization.

Steven Dandaneau employs the perspectives and insights developed by critical theorists and postmodernists, including Adorno, Marcuse, Habermas, Harvey, and Jameson, to develop a critique of the ideologies and activities of actors responding to Flint's decline. To accomplish this, he uses documentary materials and interviews. The objective conditions in which Flint is enmeshed are treated as background and only are referred to episodically.

The book is divided into two parts. The first part deals with "class" issues. In the introduction, Dandaneau announces the class ideologies underlying the response to Flint's deindustrialization. These ideologies are divided into three camps representing "solutions" proffered by different classes, and he offers three descriptors for each ideology. The first camp is "enlightenment," or dissent, or critical ideology. The second is "postindustrial," or antihistorical, or technoliberal ideology, and the third type is "enlightened false consciousness," or cynical, or organizational ideology. Each ideological position is illustrated by chapters examining the activities of those espousing that particular ideology. Enlightenment is represented by the New Directions Movement of the United Automobile Workers (UAW). The postindustrial ideology is articulated by the Center for New Work and the GM Great Lakes Technology Centre. The enlightened false consciousness perspective is represented by the local area revitalization program and a joint GM/UAW education initiative. By the descriptors used, the reader can guess the author's favorite.

The second part of the book consists of three chapters meant to decode the meaning of and critique the documentary movie about the deindustrialization of Flint, *Roger and Me*, the GM Great Lakes Technology Centre, and the various Flint and Genesee County economic development programs. Through this study, Dandaneau discovers that all the actors, even those he clearly supports politically, act in ways that indicate they are blinded by their rootedness in the world. They cannot see the possibilities (presumably, revolutionary) that those armed with critical theory can glimpse.

The chapter topics are intrinsically interesting and the examinations



are learned and, often, insightful. However, the book, as in the case of other efforts by postmodernist and critical theoreticians to analyze empirical reality, is a pastiche of chapters strung together with little theoretical or practical justification. Moreover, as is these theoreticians wont, temporal or other organizing frameworks are considered unnecessary. For example, little attention is given to the interaction between the various, roughly contemporaneous initiatives to reshape the nature of work. Further, there is a near-total omission of the changing terrain upon which GM, the various factions of the UAW, and Flint were maneuvering. Due to this lacuna, it appears as though the actors operate on the stage of Flint, Michigan, but in a historical and economic vacuum. The reason Flint is trapped in a seemingly inescapable depression is never explained.

The narrative has a didactic style that eliminates any sense of the dynamic and contingent nature of the unfolding of human events. The Flint experience is treated as static with various participants as uncomplicated, cardboard characters. Those actors that Dandaneau does not approve of are dismissed facilely and, at times, almost flippantly. For example, the admittedly utopian attempts by the Center for New Work, which was created by two professors to include workers and managers in an effort to conceptualize and actualize new work patterns, is dismissed as an attempt to bring "the academic world of sabbaticals, grants, and the free pursuit of ostensibly meaningful projects—to the mundane world" (p. 47). This is a curious critique for an academic advocating the explanatory power of postmodernism and critical theory, both highly abstract and elitist, for guiding change.

It is the three-page epilogue that gives one pause to contemplate the cul-de-sac that those wishing to apply postmodernist and critical theory to studies of the material world face. Ultimately, Dandaneau provides only hints about, but not understanding of, the forces that destroyed Flint. In the last paragraph, Dandaneau avers, "the last best hope for liberation remains primarily vested in the critique of ideology. . . . But even when we cannot see a flicker of hope—for Flint, or for ourselves—negative dialectics remains obstinate in the world and humble before the world's objective possibilities. This is its form of hope" (p. 235). What remains is the purity of the critique or, as T. S. Eliot recommended, prayer.

*Works Councils: Consultation, Representation, and Cooperation in Industrial Relations.* Edited by Joel Rogers and Wolfgang Streeck. Chicago: University of Chicago Press, 1996. Pp. x+425. \$55.00.

Gary Herrigel  
*University of Chicago*

This is a very informative book on a subject of considerable interest and debate in progressive labor circles as well as within the industrial social-

ogy and industrial relations communities. The coeditors suggest that the historical moment is propitious for works councils: "Today, with the larger questions of capitalism, socialism and democracy more or less settled, there are indications in many European countries of a possible renaissance of councils as a workplace-based infrastructure of productive cooperation alongside institutionalized conflict" (p. 19). Indeed, "there may be today a broad if silent movement in the industrial relations of advanced industrial countries toward a new productivist covenant between capital and labor" (p. 19-20).

The larger rationale for the book, very cogently presented in Joel Rogers's concluding essay, is that the editors think that the transposition of works councils to the United States, properly undertaken, could have at least three very beneficial consequences for American workers and for the American economy. First, based in every workplace and mandated by law to be independent of management, they could provide broader and greater representation for workers. This would be especially true in (the many) workplaces in which there is currently no union representation, but, properly constructed, works councils can also improve the quality of representation even in those workplaces currently represented by trade unions. Second, councils could contribute to greater efficiency of firms and production by fostering communication and cooperation among workers and between workers and management. By changing the power relations between workers and management, works councils can encourage workers to share information with management that they otherwise would not and encourage management to consider more precisely and rigorously the costs and benefits of their own decisions and invest more time and money in knowing the details of work and production. Third, legally independent councils would provide a vehicle at the level of the plant to improve the effectiveness of government regulation of the workplace in areas such as health and safety, wages and hours, and workforce training. Rather than relying, as is the case with the current regulatory system, on a centralized bureaucracy to police all workplaces in the American economy, a properly constructed works council system would allow for the devolution of this policing function.

These are large claims for plant-level representative institutions, but, on the whole, the comparative material presented in the various case studies and in a very soldierly synthetic summary of that material by Streeck goes a long way to substantiating (or at least making plausible) these claims. The volume surveys works council arrangements in Germany, the Netherlands, Spain, France, Sweden, Italy, Poland, the European Community, and Canada. Each country situation is presented clearly and informatively by the individual authors (the essay by Michal Federowicz and Anthony Levitas on Poland is especially well done), while Streeck's essay does a nice job of outlining the institutional conditions under which works councils seem to be most effective and have the most beneficial effects for both management and labor.

The comparative data suggest the following major conclusions regard-

ing institutional preconditions. First, legally based councils seem to benefit unitary union movements but weaken divided unions, and unions in competitive systems have a greater interest in councils being union based than do unions in unitary systems (p. 335). Second, mutual trust between labor and management and effective cooperation seem most certainly to arise when there is a division of labor between works councils and trade unions. In particular, the effectiveness of councils increases when, as in the German system, independent trade unions negotiate wages and other distributional issues while works councils focus on plant- and firm-based nonwage issues. Though separation of the mandates of the organizations along this dimension seems very crucial, broad European evidence suggests that close contact between unions and councils on all other matters enhances council performance. Third, legally mandating works council roles and responsibilities can be extremely beneficial (despite the caveat above) because it can "take the main parameters of workplace participation out of contention between management and labor, as well as out of competition between individual firms" (p. 347). Generally, it seems safe to say that, based on the comparative European evidence, when works councils have an independent, preferably legal, status (i.e., are dependent upon neither management nor trade unions), they foster the most effective cooperation, allow for the most productive regulatory policing, and achieve the greatest legitimacy as representative institutions.

In advocating the works council form for implementation in American workplaces, Rogers's excellent concluding essay has no illusions about the transferability of works council arrangements into this country. He admits that it would require a kind of political coalition that at least at present is unlikely to materialize on the American political landscape. Nonetheless, he emphasizes that works councils could, in principle, be integrated into the American scene without harming the already very beleaguered American trade union movement if the proper steps were taken. In particular, he suggests that this could be achieved by limiting the introduction of works councils to nonunionized settings (thus not jeopardizing those workplace representation institutions that already exist), by drawing a "bright line" between works councils and the existing wage bargaining system (perhaps by explicitly limiting council powers to information and consultation), and then finally by building some resource guarantees for their functioning—time off from work, money to hire outside assistance, enforceable rights to elicit information from employers (p. 400).

Such measures would ensure that councils became supplements to, not competitors of or replacements for, existing workplace representation in the United States. Rogers even suggests current legal obstacles to the formation of effective works councils could relatively straightforwardly be overcome by introducing an "employee free choice" exception into section 8 of the Labor Management Relations Act. Such an exception would allow the law to distinguish between labor organizations that are dominated by management (such as business unions) and those, such as works

councils, that would not be—something the current law, dating back to the Wagner Act, is unable to do (pp. 400–401).

All in all, these arguments are very solid, well conceived, and pragmatic. The authors genuinely believe that American workplaces, and American workers, would benefit from the diffusion of works councils, and their case material has gone very far to make this suggestion plausible. The only weaknesses in their case involve the level of analysis on which most of the argument and case material is pitched. I would suspect that most readers of this volume would be interested in the specific ways in which works councils bring concrete benefits to actual workplaces. Yet, for all the empirical material presented, none of the case study authors, nor Rogers and Streeck in their synthetic essays, provides any actual workplace-level examples or situations in which works council involvement could tangibly be seen to be having a beneficial effect on the flexibility or efficiency of a firm or a production process. The authors all bemoan the absence of hard quantitative evidence demonstrating the connection between works councils and efficiency, but they rely mainly on a deductive argument about the benefits of information on productivity and the benefits of works council arrangements on information generation to make their case plausible. Nowhere do they illustrate this set of connections with empirical examples.

This is all the more troubling, given the fact that Walter Mueller-Jentsch, the author of the all-important German case, points out in his conclusion that it is a very open question whether or not the current German works council system will prove to be compatible with the newer forms of team organization in German production processes (p. 75). In this case, the most efficient and flexible forms of work organization are emerging outside of and in some tension with the existing formal system. This kind of shop floor-level problem—one widely experienced in advanced industrial production today—is largely unaddressed by the framework and arguments presented in this volume.

*Environmentalism and Economic Justice: Two Chicano Struggles in the Southwest.* By Laura Pulido. Tucson: University of Arizona Press, 1996. Pp. xxiii+282. \$45.00 (cloth); \$17.45 (paper).

Allan Greenbaum  
York University

This book is well worth the attention not only of those with a specific interest in Chicano struggles or the American Southwest but also of anyone with an interest in analyzing and theorizing social movements and environmental conflict in general. In it, the author sets out to articulate a category of "subaltern environmental struggle" that escapes the received distinction between "old" and "new" social movements (and, I would suggest, some aspects of the newer conventional distinction between "grass-

roots" and "mainstream" environmentalism). She seeks, in so doing, to contribute to linking identity issues with those of political economy in a theoretical framework for a "postmodern left."

The analysis is, as the subtitle promises, empirically anchored by two thoroughly documented case studies of Chicano struggles. The first and perhaps more familiar of the cases is the campaign of the United Farm Workers (UFW) in the late 1960s to improve the working conditions of migrant farm workers in California, particularly with regard to pesticide exposure. The UFW struggle was first and foremost a labor struggle, an effort to organize a subaltern work force under the least conducive of circumstances, in the face of resistance by the entrenched interests of California agribusiness. The UFW succeeded by linking up the issue of agricultural working conditions both with a wider identity-based Chicano movement and with ecological and consumer health concerns about pesticides then emerging among the "Anglo" public. A combination of litigation, political pressure, and (most famously) a consumer boycott of California grapes eventually forced the growers (temporarily) to sign contracts with the UFW giving the union considerable say in decisions about the use of pesticides.

The second case concerns Ganados del Valle, a sheep raising, wool processing, and weaving cooperative in northern New Mexico. The cooperative was organized by members of the impoverished and marginalized Hispano community to provide opportunities for economic development and dignified livelihood based around the occupations of pastoralist and artisan central to traditional Hispano identity. The biggest obstacle Ganados faced was access to sufficient grazing land. Like local Anglo environmentalists, it opposed large-scale commercial development in the area, but its efforts to secure grazing rights in nearby Wildlife Management Areas brought it into conflict with environmentalists, hunters, and government bureaucrats.

In this book, Pulido argues that subaltern environmental struggles cannot be situated within the conventional distinction made between "old social movements," said to be concerned with material interests, class-based, and oriented to the transformation of social totality via the state, and "new social movements" said to be "postmaterialist," concerned with identity and quality-of-life issues and oriented to the local or partial transformation of civil society. In subaltern struggles—struggles for dignity by those subject to multiple forms of structural and institutionalized subordination—what are normally classified as "economic," "identity," and "environmental" issues are inseparably linked with one another and with the fundamental issue of power. Both conflicts examined here involved struggles over relations or means of production in which subaltern protagonists self-consciously mobilized ethnic identity. In California, the UFW made an effort to evoke pride and solidarity among Chicanos; in New Mexico, Ganados took advantage of aspects of Hispano culture admired and romanticized by Anglos, such as fine crafts and a lifestyle perceived to be in harmony with nature.

In the past decade, a second dichotomy—between the “mainstream” and the “grassroots” or “environmental justice” movements—has come to figure prominently in discussions of environmentalism. The mainstream environmental movement in the United States consists of large national organizations dominated by middle-class white males, that lobby central governments to adopt reformist policy measures on a range of quality-of-life and nature preservation issues reflecting the “postmaterialist” values of the well-educated and affluent. By contrast, the grassroots movement is characterized by a multitude of local groups largely made up of women, working-class people, and people of color, who resist and protest threats to their health and other material interests (not to nature) posed, mainly, by toxic wastes.

By and large, Pulido’s analysis confirms the essential elements of this dichotomy, but her account contributes to a broader and more nuanced understanding of environmental justice movements. In several respects the subaltern movements she describes differ from the typical examples of grassroots activism in the literature and were in part chosen for just that reason. Both cases are rural, neither involve wastes, and one does not involve toxics or hazards at all. In both cases, subaltern movement activists appealed to mainstream movement constituencies by pointing out the congruence between subaltern goals and the protection of the natural environment (a move also characteristic of environmental justice campaigns on the part of aboriginal communities). Moreover, she demonstrates that there is a reservoir of good will toward environmental justice struggles in at least part of the mainstream environmental constituency, which has so far gone largely untapped due to organizational imperatives.

*Last Rites: The Work of the Modern Funeral Director.* By Glennys Howarth. Amityville, N.Y.: Baywood Publishing Company, 1996. Pp. vii+224. \$29.70.

Michael C. Kearl  
*Trinity University*

With the growing segregation between the worlds of the living and the dead in Western cultures, there has arisen a growing monopolization of the funerary industry. The modern self, no longer accustomed to the sights and smells of death (and ill equipped to deal with a corpse at home), is more than willing to abdicate all responsibility for the disposal of its dead. Enter the cast of death workers, who have come to dominate—through body possession, mystifying bureaucratization, professionalization, and a “closed network” of hospital workers, casket retailers, hearse and grave marker companies, cemeteries and crematoria, and clergy—the rituals for destigmatizing the deceased and mourners alike. At the apex of their hierarchy is the funeral director, the master conductor

of an elaborate theatrical fiction (replete with fake props and a lifelike corpse) that the audience not only expects but wants.

Glennys Howarth's *Last Rites: The Work of the Modern Funeral Director* is the product of a year of ethnographic observations at a funeral home in London's East End, a cultural backwater site where flamboyant 19th-century funerary rituals survive. Though ostensibly a study of the occupational role of the modern funeral director, the work is as much an analysis of the disempowerment of modern consumers within increasingly secularized capitalist societies. Having largely forgotten the coping rituals refined over many generations, the bereaved are adroitly manipulated to ensure the ceremony occurs with businesslike precision and punctuality. The role of the mortician may well, in fact, be the epitome of service sector commodification of life's private realm: the near total control over the rites of passage of the dead and survivors alike.

I approached this work wondering what more could be said about the mortuary division of labor. Unlike the other major works in the field, this one was not written by an industry insider (e.g., Van Pine's *Caretakers of the Dead: The American Funeral Director* [Irrington, 1975]), nor subsidized by the profession (e.g., Robert Habenstein and William Lamers's *The History of American Funeral Direction* [Bulfin, 1955]), nor is it of the muckraking tradition of Jessica Mitford's *The American Way of Death* (Simon and Schuster, 1963). Though having the benefit of the past 20 years of references (Ariès's works on both death [*The Hour of Our Death* (Knopf, 1981)] and private life immediately come to mind), *Last Rites* goes little beyond Habenstein's, Lamer's, and Pine's pioneering studies of the occupation. While Howarth is the best storyteller of the lot and some new wrinkles are added (especially how reformers' attempts to minimize costs and health threats ironically led to a greater distancing of death from society and thus even greater professional control), the basic plot remains the same: the search for respect by those who perform the highly necessary but equally stigmatizing dirty work of body disposals and how strategies for doing this historically change with urbanization, industrialization, and death's increasingly taboo status.

The book is organized in two sections. The first is a theoretical elaboration of death rituals and undertaking's sociocultural significance. The second is an in-depth analysis of the world of morticians: their socialization and coping mechanisms, the back and front stages of their performances (replete with floor diagram and photographs of funeral home premises—here bodies are held and embalmed in the garage), and their dealings with customers and the broader public. The work culminates with the embalming of a middle-aged woman, a cortege through the old neighborhood, and a crematorium service for an atheist (with music from the movie *Soylent Green*).

Several audiences are addressed beyond thanatologists (and the morbidly curious). For students of occupations and professions, there are few richer case studies than the professionalization of undertakers. For students of gender stratification, there is the story of how women, who were

traditionally responsible for laying out the body, were to be relegated to secondary office roles. For cultural sociologists interested in the power and workings of ritual, the question is addressed "Why, if they accept the realities of death, do bereaved people wish to have the body manipulated by the mortician into a form of icon which symbolizes their own aspirations for life?" (p. 22). And for theorists of the dramaturgical and Durkheimian persuasions (particularly in the tradition of Robert Hertz [*Death and the Right Hand* (Free Press, 1960)] and Mary Douglas [*Purity and Danger* (Praeger, 1966)]), the settings for individuals' final passage are ideal-type domains for illustration.

For this reader, more could have been learned if more explicit comparisons had been drawn between similar British and American establishments. Why did the British undertaking trade (whose 17th-century roots were the industry's precursor) reach its profit-making zenith in the mid-19th century when the peak in the United States was not reached until a century later (and has since become a model for the other)? Precisely how are differences in the countries' class structures revealed in funerary ritual? Why has the cryonics movement had greater success in Britain than in the United States? Further, in addition to the described changes in the mortician role as society shifted from viewing death in secular-unhygienic terms instead of sacred terms, another shift is underway. With the increasing disappearance of an embalmed body in funerary observances (because of increasing rates of cremation and closed coffin services), the "resurrection arts" of the trade are becoming irrelevant. The mortician's role is once again metamorphosing, from guardian of public health to guardian of mental health: enter the "grief expert."

*Society and the Professions in Italy, 1860-1914.* Edited by Maria Malatesta. Cambridge: Cambridge University Press, 1995. Pp. viii+340. \$59.95.

Elliott A. Krause  
*Northeastern University*

The formation of modern professions—in a process that sociologists have traditionally called professionalization—is quite complex, and it varies from nation to nation. In her edited volume, Maria Malatesta brings together an interdisciplinary group of scholars to cover the Italian case, one in which some professions—the legal profession in particular—had an important role to play in state formation, while others (the engineering, notarial, and medical professions) were critically affected by state actions after the formation of the Italian nation, the beginning period for her book. After a general theoretical introduction by Malatesta, the first section covers the production of professionals in Italian universities and then the legal, medical, notarial, and engineering professions. In the second section of the book, the contributions focus on the interaction of profes-



sional groups with their social context, on professional incomes and estates, on the relationship between the Italian aristocracy and professions, on the professions and parliament, and on professions and the form of political relationship summarized by the term "clientalism."

Malatesta's introduction focuses on the difference between the professionalization process in the British and the American case, where it primarily occurs outside the state, and the process on the Continent, where the state's role is much more inclusive. The introduction divides professions by the difference between those in the state's employ and those who are in the private sector. The Italian case is shown as different in turn from the cases of most of northwestern Europe because of underdevelopment and backwardness, which tended to limit the number of clients for those professionals turned out by the university faculties. My only criticism of her essay is the unflattering things she says about the Italian sociologists, who, according to her, have paid little attention to the historical record. But she then later in her own article quotes the work of Tousijn, Olgiati, and Speranza, the very sociologists she is criticizing. In fact, most of the work done by the social historians of the professions was done after the volume edited by Tousijn (*Le libere professioni in Italia* [Mulinò, 1987]) appeared.

Andrea Cammelli's statistical consideration of the university output of professionals revises and updates the work of Barbalgì on the overproduction of professionals by the Italian university—showing that the production was not excessive relative to the Italian population; however she tends to downplay the demand for services, which the other authors in the first part make clear was not excessive given the production of new professionals. Aldo Mazzacane's chapter on the Neapolitan legal profession illustrates the important role that the leading professionals played in the university, the private practice of law, and in politics. Marco Santoro discusses the role of the Italian notary, with the difficulties that the profession had until the time of fascism, when it was given both a numerus clausus and a legal monopoly. Paolo Frascani's consideration of the Italian medical profession, especially the role of the commune-contracted public physicians who were the main element of the Italian profession before the slow and steady building of hospital-based speciality groups and a private profession. He notes that the split between the two segments eventually became political as well, with the commune physicians supporting the socialists. In Michaela Minesso's chapter on engineers, she shows how the Napoleonic era basically set the model for the profession as the Italians borrowed the French model of a state-employed engineering corps and special schools similar to the French *grandes écoles*. With the late onset of industrialization, Italian engineers branched out from the public sector and the private profession grew, ultimately becoming the better-paying one; engineers were instrumental as well in setting up many of the great Italian industrial empires.

The second section of the book takes issues raised in the first into the broader context. Alberto Mario Banti uses the records of estates of profes-

sionals in different regions of Italy to show the massive split between professional elites and the broad mass, earning from one-third to one-tenth of their colleagues—a split more pronounced in Naples than in the North. Giovanni Montroni shows that with only a few exceptions, the aristocracy and the professionals did not mix. Generally it was the younger sons of minor aristocrats who were interested, and most professionals did not intermarry with the great aristocratic families. The early Italian parliaments were composed of a majority of lawyers, which affected the way that issues were framed, but, as Fulvio Cammarino shows, their presence was not used to form any sort of professional project for lawyers per se but rather was used to anchor a set of client relations developed in their home regions. Finally, Luigi Musella elaborates on this, showing how professionals served as brokers between the family, the state, and the private sector of the society.

Malatesta and her colleagues have produced an excellent, scholarly work in the comparative sociological/historical tradition. It will be of major importance for all those interested in state-profession relationships, in professionalization, and in the specific Italian forms of professional development.

*Passing By: Gender and Public Harassment.* By Carol Brooks Gardner. Berkeley and Los Angeles: University of California Press, 1995. Pp. xiii+256. \$40.00 (cloth); \$16.00 (paper).

Lynn S. Chancer  
*Barnard College*

A testament to the evocative power of *Passing By: Gender and Public Harassment* is that, ironically, the very process of reading the book in a number of cafés produced unsought-after remarks from several male strangers who glimpsed its cover. Women, too, initiated conversations, though in their cases it was to tell me stories of being harassed.

This is precisely Gardner's first and most important point: public harassment is common, from a sociological perspective quite remarkably so, even if, as a matter of public policy, it remains little studied and disturbingly overlooked. Gardner defines public harassment as those "groups of abuses, harryings and annoyances characteristic of public places and uniquely facilitated by communication in public" (p. 4). By this usage, she finds that a huge proportion of men interviewed in Indianapolis, Indiana, for her study—90% of the 213 men contacted in her 506 citizen sample (p. 110)—eventually admitted to having engaged in such behaviors themselves (though one wishes Gardner had provided a little more information about the specific character of these behaviors). It is not surprising, then, that Gardner believes public harassment is a problem to which all women are potentially or actually subjected.

Gardner demonstrates her second major point by carefully sidestep-

ping essentialist premises and avoiding the impression that public harassment is *only* a form of invasiveness visited by men upon women. Rather, as Gardner insists, not only men but women too are sometimes guilty of publicly demeaning others on the basis of their race, physical disability, or sexual preferences. In some cases, women may respond reactively through reversing roles with one's usual harassers as a way of striking back. Thus, although the vast majority of Gardner's examples reveals men's actions toward women, she prefers to use the concepts of "situationally advantaged" and "situationally disadvantaged" persons to encourage thinking about the problem in more general (and generalizable) conceptual terms. In Gardner's perspective, then, theoretical precision requires conceiving of gender as a case study in public harassment rather than of public harassment as a manifestation of gender.

*Passing By* succeeds remarkably well in making these points. More and more interesting as one proceeds, it is a work almost sure to inspire lively discussion in graduate or undergraduate classes. The subject of the public sphere has itself increasingly interested American social theorists over the last decade, from the translation of Jürgen Habermas's *Transformation of the Public Sphere* to the interesting work of Nancy Fraser. Moreover, Gardner is excellent—thorough and methodical—in her exploration of different facets of public harassment as a social problem in the contemporary United States. She devotes chapters to particular sorts of harassing behaviors, the characteristics of participants, and to a fascinating summary of strategies used to cope with incidents once they have occurred. In addition, although Gardner's sample of interviewees was confined to Indianapolis, she manages to make a persuasive case that the harassing interactional patterns studied in *Passing By* extend well beyond only that midwestern setting.

A major strength of *Passing By* is therefore to emphasize the astonishing fact that large numbers of "situationally advantaged" persons feel free, undeterred by social/cultural taboos or by fear of legal sanctions, to regularly intrude in public upon the freedom of others. What is less clear from Gardner's book is why this pervasive phenomenon exists and what can be done about it. This comment is aimed not exclusively or even predominantly at Gardner's work. As in many equally rich works written from a symbolic interactionist perspective, there is a tendency to foreground issues of individual behavior and agency; in the process, the sociologist hopes and presumes that the presence of structural factors will be adequately revealed, accounted for, considered, and pointed beyond through the presentation of one's "data." Structural accounts, however, are also needed to account for persistent and repetitive regularities of the social world that can no more be reduced to individually symbolic accounts than structural accounts can themselves afford to sweep away the indeterminacies of individual agency.

In the instance of public harassment, this addendum may be a significant one. For the particularly gendered character of much public harassment suggests an inseparability of this social phenomenon from structur-

ally based vestiges of sexually and racially based domination in American society. Though Gardner provides ample consideration of gender through useful discussions of public etiquette and romanticized interpretations that rationalize men's harassing behaviors, the omission of more structural feminist analyses at the beginning of the book leaves one with less to suggest by way of redress at its end. Thus, Gardner makes the interesting point that better urban architectural designs are needed to protect women; she makes the possibly self-contradictory one that "presence in public places should be restricted to those who can be counted on to use them noncriminally, as one park analyst has already suggested for public parks that are sites of crime toward women and children" (p. 224). Yet, I would argue that changes in urban design and in law—though helpful, not in themselves to be demeaned—would still be insufficient, merely post facto gestures, unless the production and reproduction of gender within a wide range of social institutions, from the family to the economy, were simultaneously targeted as ongoing matters of feminist concern and intervention. The commendable contribution of Gardner's *Passing By* is to tell us that something profound is occurring that allows the "situationally advantaged"—a group whose social morphology is, not surprisingly, disproportionately male, white, and heterosexual—to feel sufficiently emboldened and adequately entitled to regularly make women and others "pawns for street commentary, targets of gaze, subjects of touch, lures for trailing and stalking, dupes of foolmaking—and victims of rape and violent crime" (p. 240). The challenge that remains is to link Gardner's insightful analyses with structural critiques and social movement-initiated redresses that could make of public harassment itself a phenomenon that passes by.

*New Voices in the Nation: Women and the Greek Resistance, 1941–1964.*  
By Janet Hart. Ithaca, N.Y.: Cornell University Press, 1996. Pp. xiii+313.  
\$39.95 (cloth); \$16.95 (paper).

Laurie Kain Hart  
*Haverford College*

The prison on the Greek island of Aegina where, as elsewhere in Greece during the years of active persecution of the left, political prisoners were held, tortured, and executed, is no longer in operation. Derelict in the absence of any plan for conversion and reconstruction, it houses a puppet theater and a shelter for endangered birds; it is as though, from an official point of view, this so visible object has simply ceased to exist. Likewise, Janet Hart observes, the Trikeri political exile camp is now an unmarked picnic spot. If these erasures are the good signs of the end of a reign of terror, they are also testimony to the *modus vivendi* of a "generation of silence." The collective memory of World War II resistance in Greece and of the civil war that followed it has been suppressed both by political

persecution and by the etiquette of coexistence; only recently has there been the possibility, in Greece and abroad, of a new attention to this period. Janet Hart's book, *New Voices in the Nation: Women and the Greek Resistance, 1941-1964* is an insightful contribution to the sparse literature in English on this period.

The book chiefly concerns the causes and nature of the political involvement of women in the resistance and civil war and its consequences for the gender regime in Greece in the long term. This, inevitably, draws us into the question of the nature of the resistance movement itself. Hart's account features, she writes, "an essentially pro-EAM [National Liberation Front] subtext that laments the idea that a nascent popular democracy was crushed during an uncritically repressive era" (p. 81). This position will incite the charge from some quarters that her presentation does not dramatize the costs of "popular democracy" as it was played out at the local level or its ambiguities at the international level; her aim is however not to "prove the truth about the case" (p. 46) but to trace the mode and spirit of a particular mobilization, especially as it is understood retrospectively by the participants themselves.

*New Voices in the Nation* is, first, a presentation of original testimony from women who participated in the resistance, second, an analysis of the nature of narration (oral histories as "managed conversations," p. 17), and, third, an argument for the power of narrative in emancipatory social movements. As an African-American researcher with an eye to forms of mobilization in the U.S. context, the author found the testimony of her informants compelling; she describes what she has written as a "personal allegorical ode to political possibility" (p. 27).

Given that the primary data of the book are individual narratives, Hart's discussion of narrative as a genre of knowledge and self-presentation is particularly important to her analysis. One ends up wishing that the book had been organized in such a way that these theoretical reflections were brought more directly to bear on the oral histories themselves; as it is, the case study material does not make a decisive appearance until midway through the book. The oral histories are at the same time evocative in detail and formulaic in narrative structure. Hart's sample contains a bias toward urban women and toward those who were very young when they joined the partisan movement but that bias raises interesting questions about the role of political vanguards and about the mobilization potential of youth. The interviews reflect a mix of traditional and "progressive" ideas about women, and class, and invoke ideal types of both "forms of life." It is clear from these discussions that resistance organizations, however revolutionary, appealed to the people by means of a common cultural language: they insisted on sexual virtue among partisans, and invoked the value of learning, familial honor, love, and obligation.

Hart devotes considerable attention to resistance leaders Dimitris Glinos and Stefanos Sarafis, in an attempt to link the mobilization of women to strategies of political "modernization" and social reform and to support her interpretation of the character of the resistance. The struc-

tural connection of this "prosopographic" material with the general shape of the movement remains relatively undefined. Her discussion of Gramsci bears most usefully on the matter of leadership and the "national-popular," but her use of Gramsci also suffers from a certain ambivalence. At moments, Gramsci appears as a native voice of the European periphery—constituting a comparative case in the political history of the era—and at other moments as exegete or psychopomp with reference to the Greek case; this combination of functions appears to reflect the author's double positioning of the book as a study of redemptive narratives and, itself, a redemptive narrative.

In the aftermath of World War II, the possibility was raised, however broadly or narrowly, of an everyday life carried out on the principles of the new social program forged during the period of defensive nationalism. The violent backlash was, as Hart shows, both political and sexual. Her oral histories give evidence of vicious popular as well as official repression, and yet, as Hart persuasively shows, the received rhetoric of EAM was less Marxist than broadly nationalist and populist. The problem with the final chapters—on the period 1945–64 as an extended "demobilization" by means of repression—is simply that this is a terribly complex period to review so briefly.

The Greek case is set beside a series of comparative examples of the potentials and hazards of political mobilization—the American Civil Rights movement, the Spanish Civil War, Gramsci's Italy, Nicaragua, the American New Left, Iran, and Algeria. While these comparisons are interesting, they are too various to constitute a persuasive argument about anything in particular. Where the author is at her best is in her clear presentation of rich material on Greece itself, as well as in her thoughtful, and self-aware, reflections on political ideals. She manages to provide adequate background for those unfamiliar with Greece and her book makes accessible both the general subject of political mobilization and the particular case of the Greek resistance.

*The Roads of Chinese Childhood: Learning and Identification in Angang.*  
By Charles Stafford. Cambridge: Cambridge University Press, 1995. Pp.  
xv+213. \$54.95.

Hill Gates  
*Stanford University*

This gracefully written study of life in a small, contemporary Taiwan village will have two readerships, though each set of specialists will get only a little more than half a book. Those who assume from the title that Stafford focuses on childhood as a developmental stage in a particular culture will learn most from the first part of Stafford's ethnography. The others, reading from a more general curiosity about things Chinese/Taiwanese, will quickly find themselves deep in his original reinterpretation

of some familiar aspects of folk ritual as "mother-child religion" (p. 145). The book is a welcome addition to both literatures, adding too to our knowledge of how Taiwan villagers conceptualize space and how they move between the "morality of the school" versus the "morality of the community" (p. 170). As a coda, Stafford offers us some comparative observations drawn from fieldwork in China's Dongbei province.

What the reader will not find in abundance is something we might particularly anticipate: children's commentary on their own lives. Old Taiwan hands will not be surprised at this lack. Most of us, if we are not born in Taiwan, find ourselves peering into the suite of Chinese cultures from two rather limiting positions: the honored guest, immobilized by courtesy, or the troublesome stranger, warned off politely for our own, and everyone else's, good. Stafford discusses both of these with considerable insight, but they remain hard seats from which to interview children.

Stafford's organization of much of his material in spatial terms—how children's bodies are moved and manipulated, how "chaotic" body movement is contradictorily disciplined and admired, village layout, the physical hierarchy of schools that some children climb as they leave Angang—gives cohesion to the study and brings into focus an aspect of social reality that currently draws much attention from anthropologists. He misses a trick, however, in failing to follow up on his hints about the world of work that distributes these children's elders around Taiwan's industrialized landscape. Angang may be relatively remote—it lacks good public transportation—but it is far from isolated. A great many of the community's young adults, including many children's parents, are either migrants to or sojourners in more prosperous towns and cities. They return often, knitting the outside world firmly into the spatial reality that Angang children grow up oriented toward. Older brothers and sisters in middle schools and military service do so as well. More reportage both on the work and study ladders and on how children and parents view them would be very welcome. This would be all the more relevant because of Stafford's careful and convincing emphasis on the material exchanges that make up the core of the parent-child relationship (e.g., p. 79).

In discussing migration, Stafford emphasizes the well-established image that home is safe while strangers are dangers. Yet he says little beyond this about what children learn or are taught to prepare them to leave Angang. When Angang people tell Stafford that their area is culturally unique (p. 41), they appear to me to describe things common to most poorer Taiwan communities that must send out laborers to survive. Like so many poor people, do Angangnese take the mismatch between themselves and those they encounter in the towns as "cultural" in the regional sense, or do they recognize a commonality of class with other, similarly situated migrants? My informants often gloss class as educational or occupational level and seem much more alert to those identities than to local ones. Yet Angang parents, if they wish to reap the nurturance legitimately expected from grown children are well served by emphasizing the home village identity.

Many *AJS* readers will wonder, as does this anthropologist, why Stafford shows so little interest in confirming his many speculations about what it is that children actually learn from their parents, their schools, and the ubiquitous spirit mediums whose connection with children he explores in such interesting ways. His interpretation of the last (esp. pp. 140–42) is a spiral of “implies,” “perhaps,” and “might.” He neither pries answers out of the children he knows nor raids the work of others for clues. Looking at the bibliography, one would not know that Bernard Gallin, Arthur P. Wolf, and Norma Diamond—to mention only three of the first postwar anglophone ethnographers of Taiwan—had written a good deal about children.

Stafford is to be congratulated for bringing original insights into the key question of what Taiwanese children learn and how they learn it. Taiwan/China specialists, comparative students of childhood experience, and those seeking a clear, lively recent survey of working-class life in rural Taiwan will find this an attractive book. Hard-nosed developmental or educational empiricists will find many more questions than answers.

*Crossing Frontiers: Gerontology Emerges as a Science.* By W. Andrew Achenbaum. New York: Cambridge University Press, 1995. Pp. xiii+278. \$59.95 (cloth); \$18.95 (paper).

Helena Znaniecka Lopata  
*Loyola University Chicago*

Andrew Achenbaum is a major historian of gerontology. *Crossing Frontiers* is an excellent, thorough, and richly documented history of gerontology from the time that “old age becomes a ‘problem’ worth investigating scientifically” (pt. 1) to when “gerontology takes shape in the era of Big Science” (pt. 2). I particularly liked the introduction in which Achenbaum plays with the ideas of the title, defining gerontology as the “study of aging from the broadest perspective” (p. 1) and explaining his metaphoric use of frontier. The major thesis of the book is the multidisciplinary and risk-taking nature of this field in which new knowledge crosses boundaries, often of territories “already disciplined and contained,” causing competition and conflicts.

Actually, Achenbaum does not consider gerontology to be a science since it lacks “intensified collective consciousness” and “because senescence remains a ‘secondary problem’ in most disciplines and professions” so that it is largely ignored by “Big Science” (p. 11). It is not even a profession since it does not claim a monopoly on knowledge and does not extend “rights, rites and privileges of a profession” (p. 13). In fact, it “may already have outlived its influence and its usefulness as a self-contained endeavor” (p. 14), being mainly a relatively new field of study with a variety of exploratory themes.

Achenbaum’s history of gerontology, just like the field, is complicated,



arising from an ancient interest in aging till the turn of the 20th century when it developed in two directions, medical and psychosocial. In the process of development, it has struggled internally with setting boundaries, establishing networks, and trying for a unified, though interdisciplinary gerontologic science. It has continually worked to attract maverick starts, funding, and public policy. Much of the research has been dependent on extensive funding, at first primarily philanthropic, then governmental, as it moved from isolated, independent researchers to big science. The creation of these agencies, in turn, had been facilitated through the influence of leaders such as Clark Tibbits, Wilma Donahue, Nathan Shock, and James Birren.

The problems of democracy and collegiality created by interdisciplinarity and change in gerontology are illustrated by the history of the Gerontological Society of America (GSA). Membership percentages shifted from 1956 to 1991 as follows: biological sciences down from 16% to 7.3%, clinical medicine down from 37% to 23.5%, social research, planning, and practice up from 18% to 21.7%, and behavioral and social sciences up from 15% to 45%. The percentage of members who refused to take sectional memberships, sticking with the multidisciplinary identification, went down from 14% to 2%. Members come from a variety of institutions and undertake a variety of activities. In other words, the membership interests of GSA have broadened from a strictly scientific, biological, and clinical medicine emphasis to a social and psychological approach and to the more applied orientation of health care professionals (p. 134).

A major part of *Crossing Frontiers* is devoted to the political and academic problems of developing research institutes and centers, especially of the University of Michigan's Institute of Gerontology, with which Achenbaum has extensive familiarity. Its history illustrates the importance of factors such as multidisciplinary, collective risk taking, efficient establishment of priorities and resources, and a strategic placement in a major research university. The connection between gerontology and knowledge sponsors and consumers is also exemplified by its relations with American veterans programs.

I find three main problems with *Crossing Frontiers*. One is its emphasis on big science, which is mainly biological and longitudinal. Except for a few pages devoted to the University of Chicago's scholars and the Kansas City Study of Adult Life, a brief presentation of Matilda Riley's program at the National Institute on Aging, and reference in the conclusion to Robert Butler and Betty Friedan as the most influential "outsiders" making gerontology known to society, there is very little about the contributions of sociologists and related researchers (to a sociologist, writing for a sociology journal, this omission is striking). In *Crossing Frontiers*, gerontological knowledge is generally limited to the biological and medical, focused on individuals isolated from each other, the community, and the social structure.

My second concern is with the assumption that no significant knowledge or recruitment to gerontology exists outside of the big institutes.

There is no mention of ways other scholars have been pulled into gerontology, such as the effective and enjoyable interuniversity program of the Midwest Council for Social Research on Aging.

Finally, Achenbaum does not, in spite of his introductory statement, present us with a historical analysis of ideas but with the methods by which big science is made possible through highly funded, institutionalized centers. I would have liked to know more about the content of the field.

*The Masada Myth: Collective Memory and Mythmaking in Israel.* By Nachman Ben-Yehuda. Madison: University of Wisconsin Press, 1995. Pp. xxi+401. \$60.00 (cloth); \$22.95 (paper).

Barry Schwartz  
*University of Georgia*

In A.D. 66, the people of Jerusalem simmered in thoughts of rebellion against Rome. So outraged were they, however, at their own Sicarii, a band of Jewish assassins and thieves, that they drove them from their city. The tyrannical Elazar Ben-Yair led his 1,000 Sicarii followers to a seemingly safe mountain fortress, Masada. From there they sustained themselves by pillaging nearby villages. The Romans, after destroying Jerusalem in A.D. 70, laid siege to Masada. Instead of fighting to the death, as had Jerusalem's defenders, Sicarii men chose suicide for themselves, their wives, and children. "Miserable men indeed they were" (p. 41). Such was the account of Josephus Flavius, sole historian of Masada's fall.

Few Jews had ever heard of Josephus's story until a vital Jewish community formed in Palestine during the early decades of the 20th century. The narrative that then arose, however, bore little resemblance to Josephus's. In this one, Ben-Yair leads his "Zealots" to resist the Roman attack against Jerusalem, is overwhelmed, and retreats with them to Masada to make a last stand. For three years, Masada's defenders raid Roman outposts and resist Roman assaults against their fortress, but they are finally worn down. Ben-Yair and his followers choose suicide—death in freedom over life in slavery and dishonor.

It will be a long time before anyone documents this myth-making process with more thoroughness and precision than Nachman Ben-Yehuda. His book, *The Masada Myth*, is rich in substance, but its questions are simple and direct: Who revised the original Masada story and why? When did this revision peak and decline? What was its consequence? Ben-Yehuda answers these questions by drawing on an immense array of sources—textbooks, historical monographs, encyclopedias, newspapers, children's books, dramatic art, literature, television, military brochures, and tourist guidebooks. Each source reveals a different facet of the narrative's institutional supports. Moving deliberately, often repetitively, he follows the theme of the Masada chronicle from depravity to heroism.

To turn around ancient stories takes dedicated entrepreneurs backed up by powerful institutions. Shmaria Guttman, a 1912 immigrant to Palestine, believed that Josephus, a turncoat Jew who had sided with the Romans, had distorted a story of true valor. During World War II, Guttman arranged pilgrimages to Masada and, working through Jewish youth organizations, convinced an entire generation to adopt Masada's "defenders" as model ancestors. Guttman succeeded not because he had discovered new historical evidence but because his version of the Masada story encompassed the predicament of the day. This is the basis of Ben-Yehuda's argument. A small community of Jews was hemmed in by Vichy French troops in Syria at the north and Rommel's Afrika Korps rapidly approaching Cairo from the west—no story prefigures this situation better than the Roman siege of Masada. No story better articulates the fear that Jewish Palestine—Israel, as its residents thought of it—would be the last stand of the Jewish people. The revised Masada story formulated the mood of the Jews in time of mortal danger, and formulating it transformed it into a powerful mobilizing force for self-defense and statehood. Two decades later, the Masada myth's second great entrepreneur, native-born Yigal Yadin, conducted archeological excavations that revealed the fortress's original structures. Yadin's achievement was stunning, but its significance, Ben-Yehuda shows, rested on the commemorative infrastructure that Guttman had established during the earlier years of peril.

Besides Guttman and Yadin, there were many revisionists. Ben-Yehuda's work, in fact, documents hundreds of deviations from Josephus's text and explains them all in terms of the rumor-making processes of leveling, sharpening, and assimilation. The argument is neat and coherent but not entirely compelling. How can deviation from Josephus's account be dismissed as a myth if Josephus himself is untrustworthy? Ben-Yehuda asserts that the validity of Josephus's story is irrelevant to the question of how new versions of it emerged; yet, he assumes that Josephus is more credible than his revisionists. This assumption is essential to a "contextual constructionist" argument that, although credible, preempts systematic discussion of the grounds on which Guttman and the other revisionists challenged Josephus. To make Josephus's version of the story the "analytical docking anchor" restricts rather than promotes analysis.

After the 1967 Six Day War, Masada's appeal deteriorated. A newly skeptical intelligentsia, the transformation of Israel from an insular society to an outwardly looking society, the demise of the generation in which the Masada cult originally developed—these converted Masada from a sacred shrine for pilgrims to a profane site for tourists. Ben-Yehuda's explanation of this change, however, does not go far enough. The decade in which Masada deteriorated was one in which the "grand narratives" of many Western democracies eroded. *The Masada Myth* itself appears as a new generation of Israeli historians, many educated in the West, are defining their nation's wars as products of racism, political rigidity, and indifference to Palestinian rights. The diminishing of Masada is part of a global diminishing of traditional beliefs.

Whatever the imperfections of Nachman Ben-Yehuda's book, they cannot obscure its massive erudition, methodological rigor, and sustained and plausible analysis. That the object of *The Masada Myth* should be situated beside the sites of Maurice Halbwachs's *The Legendary Topography of the Gospels in the Holy Land* (1941) is fitting, in this regard, for it shows how far our understandings of collective memory have progressed over the past 55 years.

*The Nation of Islam: An American Millenarian Movement.* By Martha F. Lee. Syracuse, N.Y.: Syracuse University Press, 1996. Pp. vii+144. \$14.95 (paper).

Laurence Mamiya  
*Vassar College*

Martha Lee has made an important contribution in applying the theory of millenarian movements to the history of the Nation of Islam, the most enduring of the black militant groups in the United States. The core of the book originated as a master's degree thesis. She has persuasively demonstrated that the millenarian perspective can lend a special insight into the group's behavior and ideology. Elijah Muhammad's doctrine of the "Fall of America" is integrally connected to the Nation of Islam's central myth of Yakub, the black mad scientist who created whites as a devilish race and allowed them to rule over blacks for 6,000 years. But that rule is coming to an end, according to the Nation of Islam's apocalyptic vision, derived from the book of Revelation, that blacks and whites will engage in a final cataclysmic battle. In the midst of the destructive chaos, the mother space ship, a version of the prophet Ezekiel's vision of the wheel, will emerge to save black people and return them to rule over the natural order in the new millennium. The task of Master Fard's prophets, previously Elijah Muhammad and now Louis Farrakhan, is to read the signs of the times in street riots, assassinations, and the appearance of UFOs and to prepare Allah's chosen people. Screenwriters for an action movie could not do any better.

In chapter 1, Lee gives an overview of the various millenarian theories and selects some aspects for her own application. She does not develop her own theory. What is crucial to Lee's view is the "failure of prophecy," particularly when the religious prophet specifies a date or time when the millennium is to arrive and the group's members react in either severe disappointment (and leaving) or in reinterpreting the prediction of final events. Chapter 2 presents a summary of the early history and doctrines of the Nation of Islam. In chapter 3, "The Fall of America," Lee presents her main argument that Elijah Muhammad had specified the years of 1965 and 1966 as the time of the apocalypse, citing as evidence quotes from the nation's paper, *Muhammad Speaks*. However, much of the evidence for that chapter is concerned with Malcolm X's split with Elijah

Muhammad, departure from the nation, and assassination, which I would contend was more critical for the history of the Nation of Islam than any of Elijah Muhammad's numerous warnings that continued beyond 1966. These warnings were compiled in Elijah Muhammad's *The Fall of America* and published in 1974. Chapters 4 and 5 deal with the emergence of the new leaders, Wallace D. Muhammad (Imam Warith Deen Muhammad), who led the Nation of Islam to Sunni Islam in 1975, and Minister Louis Farrakhan who created a new faction of the Nation of Islam in 1978. The last chapter is a summary of millenarianism in the history of the Nation of Islam.

The major problem with Lee's analysis is a lack of adequate interviews with the followers of Elijah Muhammad to confirm or disconfirm their reactions to the alleged failure of prophecy. According to my own sources, the most critical event for the Nation of Islam in the 1960s was Malcolm X's death since it affected its growth, especially among young people. Secondly, the research stops in 1986, apparently with the master's thesis research, which is somewhat problematic for a new book published in 1996. Finally, a focus on the millenarian doctrine of the nation, which was always taken with a grain of salt because most members tended to be "this worldly," almost obscures the real appeal of the Nation of Islam in its psychology of black consciousness and its doctrine of economic uplift.

*Culture of Honor: The Psychology of Violence in the South.* By Richard E. Nisbett and Dov Cohen. Boulder, Colo.: Westview Press, 1996. Pp. xviii+119. \$59.95 (cloth); \$12.95 (paper).

Steven F. Messner  
State University of New York at Albany

Nisbett and Cohen traverse well-traveled terrain in *Culture of Honor*. For more than a century, scholars in a variety of academic disciplines (e.g., history, sociology, criminology, and cultural geography) have noted the comparatively high rates of homicide in the South and have offered theoretical explanations for this phenomenon. In addition, a fairly large body of research has accumulated over recent decades that attempts to test the general thesis of a Southern "culture of violence." The question that naturally arises, then, is whether or not this book by two social psychologists has anything new to offer to the long-standing debate over the causes of high levels of violence in the South. In my view, the answer is decidedly yes.

Nisbett and Cohen argue that the South is characterized by a cultural orientation common among herding societies—a "culture of honor," which puts a premium on defending one's reputation in response to insults. Insults, especially when directed at males, must be punished aggressively lest the target appear to be weak. Because of this acute sensibility

to attacks upon honor, the potential for violence is high in encounters involving members of this culture, which ultimately leads to high homicide rates. Nisbett and Cohen explain the origins of the culture of honor in the South with reference to the background of the original settlers (primarily Scotch-Irish) and features of Southern political economy (lands suitable for herding and the absence of a strong controlling government).

These arguments about a culture of honor will be familiar to those who have followed the literature on Southern violence (see esp. Grady McWhiney, *Cracker Culture: Celtic Ways in the Old South* [University of Alabama Press, 1988]). Nisbett and Cohen's original contribution is to make the empirical case for the existence of such a culture in the contemporary South. The authors begin by summarizing the facts about regional differences in violence. Here, they rely on census data and official crime statistics. They employ the standard regression framework to demonstrate that "Southernness" is significantly related to homicide rates, despite statistical controls for structural variables, when these rates are disaggregated by race, city size, and circumstances in a way consistent with theoretical arguments. Next, the authors examine regional differences in attitudes and values supportive of violence. They cite survey data indicating that Southerners do not exhibit a general preference for violence. Rather, respondents from the South regard violence as legitimate only under special circumstances that would be expected given the logic of the culture of honor thesis (e.g., for protection of self, family, and possessions, for responding to insults, and for socializing children).

The most original evidence in support of this thesis comes from experimental research. The authors report the results of laboratory studies of college students with different regional backgrounds to demonstrate physiological, cognitive, and behavioral differences in reactions to insults. For example, Southerners are more likely to experience stress in response to insults, as reflected in increased cortisol levels, and to exhibit physiological preparation for aggression, as reflected in increased testosterone levels. The authors also provide experimental evidence showing that these physiological changes are linked with aggressive cognitions and actual aggressive behaviors.

Nisbett and Cohen conclude their empirical analysis by examining the institutional manifestations of regional differences in attitudes and value orientations. They identify systematic differences between the South and other regions with respect to a number of laws pertaining to violence for protective purposes and for the defense of honor. They also report the results of field experiments examining institutional behavior. One such experiment demonstrates that prospective employers in the South are more receptive to a hypothetical job applicant with a record of honor-related violence, and another reveals that reporters for college newspapers in the South are more likely to depict an offender in an incident of honor-related violence in more favorable terms than reporters in other regions.

In sum, Nisbett and Cohen have produced a well-written and carefully

argued monograph on the cultural underpinnings of violence in the contemporary South. Critics will undoubtedly be able to find fault with each of the pieces of evidence considered in isolation. However, the authors' impressive use of diverse data sources and methods (archival, survey, laboratory experimental, field experimental) makes their overall case for a culture of honor in the South compelling. At the very least, Nisbett and Cohen effectively shift the burden of proof to those who would deny any meaningful differences between regions in cultural orientations related to interpersonal violence.

*Justifying Ethics: Human Rights and Human Nature.* By Jan Gorecki. New Brunswick, N.J.: Transaction Publishers, 1996. Pp. vii+154. \$32.95.

Jean Bethke Elshtain  
*University of Chicago*

Using the idea of human rights as his entry point, Gorecki traverses the minefield of contemporary moral discourse by posing his inquiry as a provocative query, namely, is "the idea of human rights justified?" (p. xi). He segues immediately into two broad options for justification he calls the objective and the subjective. Noting the vagueness of these alternatives, he nonetheless finds them indispensable as markers for moral inquiry and justification. The objective camp, he claims, deals in the morally compulsive nature of certain facts that lead, in turn, to norms. These he calls "norm-making facts"; one such (and the most important in the history of ethical thought) is human nature. A second such fact is the location of categorical norms in an outside authoritative source, whether "God or social contract" (p. xi). Gorecki subjects each of these norm-making options to critical examination. This proceeds clearly and briskly, and the upshot, he insists, is that neither human nature, nor God, nor the fiction of a social contract suffices as the ground for justification. Human nature is too polyphonic; God and the social contract are endlessly and essentially contestable. The subjectivist route, however, is not really viable either, leading, in our own time, to postmodern justification that is really no justification at all as expediency and power determine what we "believe" to be true at any given moment. Gorecki locates his own approach in a modern, not postmodern, problematic, one that relies heavily on critical human reason, that accepts the "classical, correspondence understanding of truth, and the testability of propositions as the demarcation line between scientific knowledge and metaphysics" (p. xiv). Metaphysics, he argues rather anemically as if it were simply a given, flows from human anxieties and hopes: basically a spin on Feuerbachian thought, although he does not call it such.

Believing that one can decisively separate propositions from evaluative judgments, Gorecki offers up an interrelated set of propositions. First, he acknowledges the undeniable power of human rights, despite the inherent

ambiguity of the idea. It is worth noting here that, in his drive to follow a continuous train of argument, he unacceptably conflates the formulas embraced by the American Declaration of Independence and the French Rights of Man. He does this via a curious ellipsis, simply omitting "nature and nature's God" from the Declaration's proclamation of eternal truths. For example, he truncates one quote by adding an ellipsis immediately after "they are endowed" and picking up again with "certain unalienable rights," but human beings are endowed "by their Creator" in the Declaration's words. The French revolutionaries, on the other hand, located the justification for rights in the nation as "the source of all sovereignty" (p. 1). Between these two formulations there can be no reconciliation. This is an important point Gorecki overlooks.

Second, Gorecki treats moral rights (of which human rights is a subcategory) as flowing from a strong "moral feeling" or "experience" that is acquired "in the process of contagious social learning" (p. 5). This seems rather indistinguishable, at first glance, from the subjectivist route he strongly resists, but he argues that building in the social dimension guards against mere subjectivism. Morally relevant stimuli activate norms "stored in our brain" (p. 6). Thus, unsurprisingly, it is more common than not for those who witness a starving child to want to do something—to experience the "feeling of duty to feed it" (p. 7). Rights as an experience, then, locate us in a world of others: rights are transitive, they must be recognized. Acknowledging that he seems to reduce human rights to individual expression, Gorecki spends the bulk of his sustained essay building in barriers to subjectivism, or mere subjectivism. In this effort he moves through cognitive psychology, intuitionism, universal needs and wants, Kantian thought, utilitarianism, sociobiology, divine will, legalism, and the enforcement of human rights norms internationally.

How well does he succeed in offering a nonsubjectivist justification for the norm-making fact of human rights? The argument, admirably clear in its articulation, nonetheless flounders, in part because Gorecki is so deeply committed to a set of categories that predetermine outcomes too predictably. Are our alternatives really between subjective feelings and objective grounds? Are we simply stuck in a moral world in which the political possibilities must be posed as the "morality of tribalism" versus that of universal "humanism" (p. 34)? Because human nature is "polymorphous" does that mean it should be given no explanatory weight? Perhaps thinking about a "human condition" rather than a predetermined teleology of human nature might add nuance to the argument. He also falters in his treatment of that ground for justification called God by reducing the *imago Dei* to Divine Will, basically a post-Occam formulation, not the only understanding of the *imago Dei* available and certainly not the one that was, and is, drawn upon by human rights thinkers within the Christian tradition as most central to their own understanding.

In other words, Gorecki winds up stalemated in part because he seems unaware of the fact that justification within moral philosophy, even at its most sophisticated, does not map precisely onto the political world.



What counts as an adequate, indeed compelling and powerful, political justification for a defense of human rights may not satisfy various analytical criteria, particularly a correspondence theory of truth. As Gorecki himself understands and writes movingly about, referring to the terror his own family experienced (he and his mother were rescued and sheltered by Christian Poles and escaped extermination in the terrible years 1942–45), what compels individuals to act morally in situations of extreme danger is often a powerful given that flows from what might be called character. Gorecki calls this “sacrificial altruism.” But those Christians who rescued them saw it rather differently—not as an act of moral supererogation but as something one did because one’s moral and ethical formation insisted on loving both God and one’s neighbor. This is something much sturdier—though never sturdy enough when the chips are down—than “moral contagion.” Aware as he is of the importance of justification, Gorecki seems less aware than he should be of the centrality of ethical formation. What authoritative institutions help to create persons incapable of committing horrible deeds? How can we guarantee that such institutions flourish rather than falter? That is a task that goes much beyond arguments in moral philosophy—as vital as these are—to the heart of our social and political arrangements. Here one of Gorecki’s heroes, Gustav Radbruch, a great legal philosopher, is an important interlocutor for Gorecki. Radbruch, having “witnessed the unheard of horrors committed in the name of positive law,” came to the conclusion in the post-Nazi era that a “struggle against positivism” alone would help to guarantee that wrong remains wrong and that “universally binding moral norms as prerequisite for the validity of positive law” remains essential (pp. 132–33). Gorecki cannot follow him in this, but it is clear that this strong justification haunts his own project.

*The Time of the Tribes: The Decline of Individualism in Mass Society.* By Michel Maffesoli. Translated by Don Smith. London: Sage Publications, 1996. Pp. xii+176. \$69.95 (cloth); \$23.95 (paper).

Ronald N. Jacobs  
Rice University

In this ambitious and provocative book, Michel Maffesoli explores the persistence and the centrality of an affective, solidary space of sociality that emerges from the small-group contexts of everyday life. Combining Simmel’s formal sociology, Durkheim’s concern with the sui generis reality of the social group, as well as a careful attention to space and the multiplicity of urban experience, Maffesoli’s main argument is that the greatest vitality of social life is to be found in the multiplicity of local “tribes” to which any individual belongs.

The book is primarily theoretical in its orientation, and centers around the two core concepts of *puissance* and *proxemics*. *Puissance*, translated

roughly as "vitalism," is distinguished from *pouvoir*, or power. Whereas "extrinsic" power is eminently political—directed toward "the social," toward the structural transformation of "history," and toward the philosophical structure of the "ought"—"intrinsic" puissance is beyond politics: that is, directed by a rationality outside the scope of politics. Puissance directs the multitude of tribes to live for their own sake; while they may have a goal, or a future orientation, what is important about them is "the energy expended on constituting the group as such" (p. 96). Furthermore, according to Maffesoli, puissance contains a founding power that the more centralized and bureaucratic forms of social power lack. As Maffesoli says, "Every time an era begins, a city flourishes or a country is epiphanized, it occurs as a result of a popular puissance. Only later is there confiscation by a few who appoint themselves managers, owners, and clerks of legitimacy and knowledge" (p. 107).

The second theoretical concept upon which Maffesoli's argument relies is that of proxemics. Contemptuous of abstract social-structural categories and demographic statistics, Maffesoli envisions a social world of overlapping networks and relations, grounded at the quotidian level of everyday existence and made meaningful through "day-to-day histories" (p. 123). This implies not only social relations between individuals but also relations to a landscape, a city, an environment, a *topos*. It is here, in the spatialization of contemporary neotribes, where Maffesoli sees the central difference from the tribes of antiquity and also from the putative atomized mass subject of modernity. In the neotribes of contemporary urban life, the "affectual nebula" of the tribe is characterized by a fleeting temporality and a transient, more symbolic or "virtual" spatial grounding. Contemporary neotribes are constituted by those who share the same subway, the same neighborhood bar, the same television program, or the same bulletin board on the internet. These neotribes organize their territories (even if they are virtual or symbolic territories) and their identities through shared practices and a dionysiac "being-together," even if only for a fleeting time, and then adjust themselves to the force of circumstance—in large part because any one individual belongs to multiple and overlapping tribes, to which she gives varying and fluctuating intensities of commitment (pp. 108–10). In the neotribes of the contemporary scene, according to Maffesoli "the interplay of the proxemic is organized into polycentric nebulae" (p. 145).

It is here that Maffesoli's theory becomes somewhat vague and problematic. On the one hand, Maffesoli values the affective dimension of tribes and neotribes because of the way they allow for a recentering of politics: away from a universal and transcendental reason, based on the individual *I*, and toward aesthetics as a form of situational ethics, grounded in a network of collective *we*'s. This is an important difference from the more interactionally guaranteed ethical vision of, say, communitarianism, the focus of which centers around the construction of shared substantive values or the cognitive dimensions of solidarity. Instead, Maffesoli favors the dionysiac and sensual "being-together" of the tribe and

believes that the "polycentric nebulae" of tribes in today's postmodern era can produce a cultural blending characteristic of a new era, based on a combination of segregation and tolerance (p. 145). The problem is that, by favoring the aesthetic over the cultural dimension of solidarity, Maffesoli fails to consider the darker side of neotribalism: namely, that the dionysiac formation of local and particularized identities can tear asunder the possible grounds for intersubjectivity and reflexivity between groups. Unless the power and politics disappear altogether, replaced by a completely inward-looking localism, this is a real problem.

As the reader may have discerned by now, this is a difficult book, written in a style characteristic of French theory. The intellectual gymnastics that substitute for an introduction, the superfluous attacks on empiricism, and the complete lack of attention to contemporary American sociology (even in those areas directly related to Maffesoli's theoretical argument) are likely to enrage many readers. All the same, this is a book that should provoke challenging and fruitful questions for those studying civil society, community, religion, or the "new social movements."

*Society and Psyche: Social Theory and the Unconscious Dimension of the Social.* By Kanakis Leledakis. Oxford: Berg, 1995. Pp. viii+226.

Ian Craib  
*University of Essex*

This book does not come directly from the recent boom in psychoanalytic studies in Britain, but it does emerge from the intellectual culture of which the boom is a part. Its particular focus is social theory on a grand level, and perhaps the best way of describing Leledakis's project is to say that it is approximately similar to Talcott Parsons's attempt to use psychoanalytic ideas as a bridge between social structure and social action. This is as far as the similarity goes: whereas Parsons eventually produced what Dennis Wrong called an oversocialized concept of man (and presumably of women too), Leledakis's concern is to build into social theory an understanding of what he regards as the essential indeterminacy of action, of social change, and of history itself.

There seems at the moment to be two ways of working at this level of grand theory. One I would call the soup approach: various theoretical ingredients are peeled, cored, diced, and thrown into the pot and boiled, and the resulting meld is seen as an overarching synthesis. This is Anthony Giddens's way. The other way is the jigsaw approach. The theoretical pieces are picked up and carefully fashioned and sanded down so they might fit each other. This goes on until there is only one piece left, and the author picks up his or her perfectly cut piece, fits it in, and shouts "Eureka!" The picture is completed. This is Leledakis's approach, and his final piece is psychoanalysis.

His assumption is that good social theory must allow for indeterminacy

at the level of social change and at the level of individual action; it must not allocate a transhistorical, essential role to any of its concepts or presumably to the realities that those concepts are attempting to understand. We then start a long journey. Marxism sets the problem of reconciling creativity and determinism and fails through allocating a transhistorical role to labor and production; positivism and Hegelian idealism fare no better, but Wilhelm Dilthey, Edmund Husserl, Alfred Schutz, and George Herbert Mead all add to the picture through being "suggestive of a theorisation of a level of the social as a structured field of meaning not coextensive with conceptual thought though partially accessible to it" (p. 46). Talcott Parsons, Max Weber, and Louis Althusser all fall under the accusation of developing transhistorical categories and running into the consequent difficulties.

In contrast to Althusser, Giddens gives too much to individualism; Pierre Bourdieu has a more interesting conception of the social but no theory of individual action. Jacques Derrida, Ernesto Laclau and Chantal Mouffe, and Cornelius Castoriadis (a French psychoanalyst and social theorist) all take us forward on Leledakis's journey, but they either fail to develop a theory of subjectivity or they maintain transhistorical categories. None of them reach the target of theorizing "a certain 'openness' and indeterminacy of the social [which] could be located *within* the processes of production and reproduction of the social through the individual" (p. 121).

Thus we come to the final piece: psychoanalysis. Psychoanalysis enables us to provide an understanding of this indeterminacy on which Leledakis insists: it does so in three ways. First, it is impossible to predict what objects will be cathected or internalized. Second, when the internalization takes place, it is subjected to an unpredictable transformation in the chaos of the unconscious; and, third, all attempts at rational thought are always disrupted by id impulses.

What are we to make of all this? My first point is that the attempt is honorable; sociological theories of agency are, I think, profoundly overrational, and psychoanalysis, if taken seriously, does offer a sophisticated hermeneutic that has a place in social theory. I think the problem with this book is that while the third part, which deals directly with psychoanalysis and social theory, is well worth reading—in fact as good an introduction to the area as I have found—the first two parts remain too close to a Ph.D. format. They are more concerned with convincing the reader of the author's abilities and range of knowledge than with the clear demonstration of issues. There are some interesting points in these sections, but they could be developed at much less length and leave space for two notable omissions. The first is Jürgen Habermas, who needs much more serious attention than he gets here (a longish footnote), more so than several who get a chapter devoted to them; the second is that Leledakis does not deal with object relations theory—the area of psychoanalysis where creativity is most central and where complex conceptions of subjectivity and agency are waiting to be developed.

*Nietzsche, Politics and Modernity: A Critique of Liberal Reason.* By David Owen. Newbury Park, Calif.: Sage Publications, 1995. Pp. x+180. \$65.00 (cloth); \$19.95 (paper).

Stjepan G. Mestrovic  
Texas A&M University

Owen begins: "Why Nietzsche now? Reflecting on the contemporary intellectual scene, it appears as if Nietzsche is rapidly becoming as central to cultural debates in our *fin de siècle* as he was to cultural critics at the end of the nineteenth century" (p. ix). The question is reasonable: Over 30 books on Nietzsche were published in 1995 alone. What can Owen or anyone else teach us about Nietzsche that is not known already? Owen claims that his book is "on Nietzsche's critique of the philosophical commitments of liberal reason and his recommendation of an alternative ethical schema structured around the ontological thesis of will to power, the law of eternal recurrence, an ascetics of self-overcoming, and the *telos* of the Overman" (p. 132).

Owen compares and contrasts Nietzsche with Alisdair MacIntyre, Charles Taylor, Richard Rorty, and John Rawls. Other philosophers who appear in this crowded argument include Descartes, Hume, Kant, and Schopenhauer. The argument moves at a dizzying pace. For example, in finding a tenuous link between Rorty and Nietzsche, Owen focuses on Nietzsche's anti-Cartesian argument that the activity of knowing is rooted in a person's affective constitution. Truth involves many perspectives, but perspectivism does not entail relativism. In criticizing the epistemological foundations of liberalism, Nietzsche's thought "does not entail an irrational relativism but . . . on the contrary, his thought expresses an erotic commitment to truth, reason and human freedom" (p. 52).

In a subsequent chapter, Owen tries to put a positive, liberal spin on Nietzsche's nihilism: "Nietzsche argues that the ascetic priest solves the problem of nihilism by providing a *telos* for human existence in the form of an idealisation of the ascetic life as the good life which requires the quest to realise one's rational self by overcoming one's irrational bodily and cultural desires" (p. 100). In addition, Owen reinterprets Nietzsche's representation of the Overman as a utopian ideal of integrity and the Last Man as the "freedom to pursue desires and freedom from suffering" (p. 122).

With these new interpretations of Nietzsche's old concepts in place, Owen proceeds to reshape his image. For example, "Nietzsche shares Rawls' recognition that modernity is characterized by an irreducible pluralism concerning the character of the good life" (p. 159). One reads that both Nietzsche and Rawls regard citizens as free and equal and that "for Nietzsche, tolerance for other views, a willingness to engage with them in an open and fair-minded way, is a condition of claiming to hold one's own beliefs to be true" (p. 161). However, Rawls and Rorty cannot account for the foundations of pluralism whereas, according to Owen, the

just society modeled on Nietzsche's teachings on the Overman would recognize "the equal right of perspectives to claim epistemic authority without necessarily granting them equal authority" (p. 167). The last sentence in the book reads, "Thus, it is my contention that Nietzsche's agonistic politics is more conducive to human flourishing than liberal politics, whose emphasis on state neutrality and the public/private (or political/non-political) distinction undermines the conditions requisite to the production of a noble humanity" (p. 169).

I take the author at his word, that he wants the reader to take seriously his claim that Nietzsche offers a cogent critique of liberalism. It is ironic that Owen seeks to make Nietzsche seem more liberal in many respects than liberal philosophers such as Rorty and Rawls. But I must confess that I do not recognize Owen's version of Nietzsche as the philosopher of tolerance, pluralism, self-actualization, and equality. No amount of argumentation can undo Nietzsche's aristocratic hardness, his contempt for equality, and his mockery of compassion.

This leads me back to the question with which Owen opens his book and which I used to begin this review. Why Nietzsche now? Despite the surface rhetoric of liberalism and compassion extant in many Western societies, in practice, one encounters an unquestionably harsh and pitiless attitude toward the have-nots. It was Nietzsche's self-acknowledged "master," Arthur Schopenhauer, not Nietzsche, who dominated the previous fin de siècle, while the pupil has overshadowed Schopenhauer in the present fin de siècle. As noted by Georg Simmel in his book, *Schopenhauer and Nietzsche* (University of Massachusetts Press, [1907] 1986), compassion and its derivatives were central to Schopenhauer's philosophy, but Nietzsche mocked these kind-hearted virtues. Perhaps Nietzsche's widespread popularity now, coupled with the nearly complete cultural amnesia concerning Schopenhauer, suggests something disturbing about the present age that Owen and others would not care to analyze.

*Reading Marx Writing: Melodrama, the Market, and the "Grundrisse."*  
By Thomas M. Kemple. Stanford, Calif.: Stanford University Press, 1995.  
Pp. xviii+274. \$35.00.

Kevin Anderson  
*Northern Illinois University*

Since the 1940s, we have experienced, among others, existentialist, Freudian, structuralist, Hegelian, and analytic Marxisms. Today we are beginning to see the emergence of a deconstructionist Marxism, most notably in Derrida's *Specters of Marx* (Routledge, 1994). Although apparently published too soon to respond directly to Derrida's reading of Marx, Thomas Kemple's book shares some similarities to that work. Kemple also offers us some original and important insights into Marx's thought. He does so by way of a literary, sociological, and philosophical decon-

struction of parts of a key Marxian text, the *Grundrisse*, the much-discussed series of notebooks that Marx wrote in 1857–58 as a summary of his economic studies to date. From the outset, Kemple makes clear that his will not be a literalist reading of Marx, but one that allows for stretching and reworking the meaning of Marx's texts and for drawing nonobvious relationships to key literary texts of Marx's era.

For example, he is so bold as to deconstruct the word "Grundrisse" itself, usually rendered into English as notebooks, outlines, or architectural plans. When Kemple splits the word "Grundrisse" into its two component parts, however, with "Grund" carrying as a possible meaning "the ground or reason" for something and with "Risse" having that of "breaks or tears," we are given a very new rendering of the word "Grundrisse" as "ruptures in reason" (p. 2). Of course, in deconstructing this word, Kemple still faces the problem that the title *Grundrisse* was applied nearly a century later by Marx's editors to an untitled set of notebooks, but he acknowledges this issue and at least partially overcomes it by citing an 1857 letter in which Marx did use the word "Grundrisse" to refer to his economic notebooks.

Viewing not only the *Grundrisse* itself but also Marx's thought more broadly as a process of "ruptures in reason," as Kemple does, has some important ramifications. It is an interesting and new way of suggesting what many have referred to as the fundamentally negative and critical character of Marx's thought. It also seems to relate to Marx's notion of social development as a series of breaks and contradictions. To be sure, Marx's work is both historical and logical, deeply rooted in a tradition stretching from the ancient Greeks to Hegel, but it is at the same time a sharp break or rupture within that same Western intellectual tradition. As Kemple writes, here evoking Roland Barthes: "I have approached Marx as a kind of logothete, a founder of a new language" (p. 7). Kemple is careful here, however, to evoke the notion of a rupture in (within), but not with, the Western rationalist tradition.

Early on, Kemple presents a very imaginative account of a possible relationship between Marx's reference to the "monstrous" (p. 30) character of machinery in the *Grundrisse* and one of the opening scenes of Goethe's *Faust*. Later, he draws out an even more intricate set of relationships between Marx's work and that of Balzac, building here both on textual analysis and on Marx's own frequent statements of admiration for Balzac's work. While he refers to Lukács on Marx and Balzac, Kemple develops his argument in a substantially different direction.

These and other juxtapositions of the *Grundrisse* to great literary texts that Marx read enable Kemple to capture in a new way what many before him have also referred to as the dramatic character of much of Marx's work—one thinks for example of the opening pages of the *Communist Manifesto* or the closing pages of *Capital*'s first volume.

This is an erudite work, steeped in original texts in both French and German. The book also includes some trenchant critiques of prominent commentators on Marx, such as Habermas. More surprisingly, given the

author's theoretical orientation, there are also some extremely sharp critiques of Althusser and even of Baudrillard. There is also evidence of a major intellectual debt to both Lyotard and Marcuse.

Unfortunately, however, the book exhibits some unevenness (one hesitates to use such a word as a criticism in these "decentered," postmodern times) at crucial junctures. First, Kemple is sometimes so anxious to stretch or deconstruct Marx in order to show a relationship to Goethe or Balzac that Marx's own text gets nearly lost in the process. Second, he gives surprisingly short shrift to Marx's explicit references in the *Grundrisse* and elsewhere to literary texts, and he never even refers to standard works such as S. S. Prawer's *Karl Marx and World Literature* (Oxford, 1976). Third, the treatment of Hegel is cursory and often dismissive, even though more discussion of him might have informed and deepened the notion of ruptures in reason that is so central to the book. As a whole, however, this book is an example of a fruitful dialogue between social theory and deconstruction.

*Really Existing Nationalisms: A Post-Communist View from Marx and Engels.* By Erica Benner. Oxford: Clarendon Press, 1995. Pp. ix+266.

Roman Szporluk  
*Harvard University*

Erica Benner writes that Marx and Engels have been misunderstood on the question of nationalism: "Too many people have misread Marx and Engels in the light of political movements associated with their names." Benner wants to rescue the authentic ideas of Marx and Engels from those erroneous interpretations and attributions. Thus, for example, Lenin was wrong when he said that "Marxism cannot be reconciled with nationalism," and the contemporary scholar, John Schwartzmantel, who thinks Lenin was correctly interpreting Marx, is also mistaken. By identifying "some neglected strands of thought that are less easy to dismiss than the views usually attributed to them," Benner aims to correct what we are told has been—if she is right—a monumental historical misunderstanding, one moreover continuing to this day, of Marx, Engels, and nationalism.

The bulk of this book consists of an elaborate, meticulous, and often subtle exegesis of many well-known and some other less known Marxian and Engelsian dicta. Benner wants to demonstrate that Marx and Engels held a much more positive, much more "nuanced" view of nationalism than is generally thought. Her task, unsurprisingly, is not easy because Marx and Engels do not always cooperate and then have to be reprimanded. Thus, their joint statement (1879) that for almost 40 years they had "emphasized that class struggle is the immediate driving power of history" is called by Benner "an unusually doctrinaire passage" that is to be explained, she says, by "their propensity to theoretical bull-



headedness." She encourages us not to take such statements too seriously and supplies quotations revealing a less "doctrinaire" position, one consistent with her argument.

Unfortunately this particular instance of Marx's alleged theoretical bullheadedness contains one of Marxism's fundamental ideas on history and society and cannot be explained away with quotations to the contrary. Benner is so deeply committed to her thesis that she seems to forget that Marx was a communist and a proletarian revolutionary, after all, and that the interests of the proletarian revolution (as he understood them) defined his position, for or against, in specific cases where one or another nationalism was involved. Benner makes some good points, for example, when she discusses the evolution of Marx's stand on Ireland. She treats the Irish question, rightly, as an issue on Marx's real agenda—his concern with the future course of the international proletarian revolution. She provides no evidence that Marx regarded nationalism as anything but a bourgeois point of view and as such incompatible with the proletarian position. Indeed, in some cases, such as that of the Czechs, or of Eastern Europe's "nonhistoric" nations in general, Marx and Engels went beyond opposing nationalism. They denied certain emerging nations—such as the Czechs—the right to exist, to be nations. When Benner notes Marx's 1858 comment that "the Czechs" were adding "to the ferment of our movement," she comments that in this way Marx was questioning Engel's earlier, wholly negative view of the Czechs. But she does not explain why, if this indeed was the case, Marx nonetheless still insisted, as she reports, that *the Czechs* were "indeed counter-revolutionary." The answer is clear: The Czechs were counterrevolutionary because (and insofar as) they were *Czechs*.

Rather than being a product of some anti-Slavic racism, this position was consistent with Marx's understanding of class relations in that region of Europe, specifically with his and Engels's view of the place of the peasantry in the transition from feudalism and in the conflict between the proletariat and the bourgeoisie. In the Marxian vision of history as the story of class struggle no provision was made, no rational justification could be invoked, for the emergence of modern nations out of Czech- or Slovak-speaking peasantry. If the author does not make this clear it is because in her book she goes far too far in downplaying what made Marx Marx. Thus, Benner attributes to Marx the view that the proletariat was supposed to work "to break down the division between state and civil society," and in another place she seems to think that, according to Marx, "the divide between state and civil society reinforces rampant egotism." In fact, Marx wanted to *abolish* civil society just as much as he opposed the state, which was, in his view, an instrument of class domination based on private property, and private property was the foundation of civil society as Marx saw it.

The concluding chapter, oddly titled "The Revenge of Nations, 1870—?" in fact deals with contemporary questions and has virtually nothing to say about the Communists and their treatment of nationalism or of *nations*

between 1917 and 1989/1991. Benner doubts "whether the founders of 'Marxism' would have endorsed the centralist and authoritarian brands of internationalism which were discredited by their association with communist regimes." ("In politics Marx was a ferocious anti-authoritarian; and 'really existing' communism was, in its gentlest incarnations, an authoritarian system of government.") Since Marx-the-communist interests her little, Benner does not speculate on whether the project to abolish private property and to build a classless society, undertaken in 1917, would have nonetheless found Marx's approval. As for the phenomenon of postcommunist nationalism, "nothing in his writings suggests it would have surprised him," and Benner believes that her reconstruction of Marx's thought on nationalism can help to produce a better understanding of nationalism after the fall of Communism.

*The Disunity of Science: Boundaries, Contexts, and Power.* Edited by Peter Galison and David J. Stump. Stanford, Calif.: Stanford University Press, 1996. Pp. 567. \$65.00 (cloth); \$24.95 (paper).

Sal Restivo

*Rensselaer Polytechnic Institute*

Historically, the rhetoric of unity is a stock feature of the "journey to the east" in search of "truth," "wisdom," God, and moral order. The term "Unity" is the first word and the first sentence in Peter Galison's introduction to this important contribution to science studies. The first names we come across in the text are Otto Neurath, Rudolf Carnap, and Charles Morris, the editors of the *Encyclopedia of Unified Science*. A few pages later, we are reminded of Patrick Suppes's 1978 article on "The Plurality of Science" (*PSAyear* 2:3-16). Galison draws our attention to the disunity/unity axis and what its "wandering valence" can tell us about science as a historical realization and about the history of culture. The editors would like readers to reach or at least consider the conclusion underscored in David Stump's afterword: that science studies is moving away from the realism/relativism debate and becoming focused on the complex details of local scientific practice.

Stump's afterword makes explicit the fact that this volume represents a philosophical project and philosopher's interests. The aim of this collection has been, we learn in Stump's very last paragraph, to initiate "a real integration of philosophy into science studies." The contributors include 11 philosophers (J. Rouse, N. Cartwright, A. Fine, D. Stump, A. Wylie, J. Cat, H. Change, R. Creath, A. Davidson, J. Dupre, and I. Hacking), five historians or philosopher/historians (M. Biagioli, P. Galison, E. F. Keller, T. Lenoir, and S. Schaffer), one sociologist (K. Knorr-Cetina), one philosopher/sociologist (S. Fuller), and two representatives from cultural studies (D. Haraway and C. L. Ross). The book is organized in three main sections: boundaries, contexts, and power. The editors and contrib-

utors have worked hard and successfully to cross-fertilize each other's chapters. This tends to give the volume a certain unity. But, as we might expect given the theme of this volume, there are also signs of disunity: Schaffer argues for a contextualized history of canonical texts, Biagioli opposes this with a prescription for locating the author of historical texts; Rouse argues for a Foucauldian critique of the "sovereign" conception of power, which he finds represented in the works of Galison, Lenoir, and Ross, and other science studies scholars; Fuller is critical of Latour's conception of interpretation as translation. There is an underlying, but certainly not unified, opposition to social constructionism as a reductive strategy.

There are two basic foils for the arguments put forth in this book—one, looking backward, is the Unity of Science movement; the other, looking to the present, is science studies as it has come to be dominated by social constructionists and actor-network theorists. Most of the contributors assume that there is something local about science, but Galison's description of this assumption as an "intuition" is revealing. As a sociologist/anthropologist of science, I am more inclined to ground this assumption in the outcomes of studies such as Knorr-Cetina's study of scientific practice. This trivial-sounding semantic issue reveals, however, the major disunity in this volume, and that is the abyss between sociology/anthropology and philosophy. The unifying agenda of the editors and at least some of the contributors would dismiss the realism/relativism/rationality debates in science studies and concentrate on exploring the philosophical implications of contextualizing science, the temporality of science, and social explanations of scientific practice. I have no quarrel with this agenda in the wider arena of science studies. However, I find criticisms of social constructionism as "reductionist," attempts to save epistemology from becoming the province of social science, and the defense of the illusory notion that "the subject" and "human agency" are not thoroughly sociocultural phenomena illustrative of a widespread failure among historians and philosophers of science (not to mention some sociologists of science) to comprehend the meaning of "social construction of science." The biggest error here is to treat "social constructionism" as a philosophical idea subject to the traditional linguistic and logical tools of philosophical argument. This is part of a larger failure (not limited to philosophers) to comprehend the nature and achievements of sociology as a theoretical and empirical enterprise. I cannot pursue this disunifying theme here, but I cannot be as optimistic as David Stump about ending intellectual turf conflicts nor as certain that this would promote better understanding of science and scientific knowledge as social and cultural phenomena.

Specific criticisms and praises aside, this book opens an important window on the contemporary science studies landscape and should be read by anyone interested in the issues that continue to drive this multidisciplinary field as well as by science and culture wars watchers and participants.

# Essential Reading

## Masters of Bedlam

The Transformation of the Mad-Doctoring Trade  
Andrew Scull, Charlotte MacKenzie, Nicholas Hervey



Through an examination of the fascinating lives and careers of nineteenth-century "mad-doctors," *Masters of Bedlam* provides a unique perspective on the creation of the modern profession of psychiatry. Drawing on an extensive array of sources, the authors vividly re-create the often colorful lives of seven "masters of bedlam." Sensitive to the idiosyncrasies and peculiarities of each man's personal biography, the authors offer fully rounded portraits of their struggles and successes, and they underscore the complexities of nineteenth-century psychiatry as it emerged into a profession.

"Long subject to controversy, the rise of British psychiatry is now fleshed out through this series of well-researched and elegantly written lives of the pioneering 'mad doctors.' *Masters of Bedlam* transcends biography to give a lively, individual view of the making of the madhouse system."—Roy Porter

Cloth \$35.00 ISBN 0-691-03411-7

## The Order of Terror

The Concentration Camp  
Wolfgang Sofsky

*Translated from the German by William Templer*

During the twelve years from 1933 until 1945, the concentration camp operated as a terror society. In this pioneering book, the renowned German sociologist Wolfgang Sofsky looks at the concentration camp from the inside as a laboratory of cruelty and a system of absolute power built on extreme violence, starvation, "terror labor," and the business-like extermination of human beings.

Sofsky shows convincingly that the German concentration camp system cannot be seen as a temporary lapse into barbarism. Instead, it must be conceived as a product of modern civilization, where institutionalized, state-run human cruelty became possible with or without the mobilizing feelings of hatred.

"Wolfgang Sofsky dares the near impossible: he gives us a rational description of the concentration camp without losing sight of the human suffering, which the use of terror brought with it. . . . Sofsky exposes the potential of immorality that modern times carries within itself, and how the ordinary can transform itself into terror."—Ralf Dahrendorf

Cloth: \$29.95 ISBN 0-691-04354-X

# Social Science and the Quest for a Just Society<sup>1</sup>

Immanuel Wallerstein  
*Binghamton University*

Behind the current passionate debates about structure versus agency lies the ancient debate of determinism versus free will. The article reviews the debate and its dilemmas in theology, philosophy, revolutionary theorizing, and social science. The epistemological issues have been reopened and redefined by complexity studies in the natural sciences and cultural studies in the humanities, both of which involve a turn toward some central premises of social science. It is argued that social science will be epistemologically central to all knowledge and that utopistics will be a central responsibility of social scientists. It is also argued that science is at its very beginnings, and that the future will be built on going beyond the antinomy determinism/free will.

*Macro* and *micro* constitute an antinomy that has long been widely used throughout the social sciences, and indeed in the natural sciences as well. In the last 20 years, the antinomy *global/local* has also come into wide use in the social sciences. A third pair of terms, *structure/agency*, has also come to be widely adopted and is central to the recent literature of cultural studies. The three antinomies are not exactly the same, but in the minds of many scholars they overlap very heavily and, as shorthand phrases, they are often used interchangeably.

Macro/micro is a pair that has the tone merely of preference. Some persons prefer to study macrophenomena, others microphenomena. But global/local and, even more, structure/agency are pairs that have passions attached to them. Many persons feel that the only framework of analysis that makes sense is one cast at the global level or, conversely, one cast at the local level. The tensions surrounding structure/agency are, if anything, stronger. The terms are often used as moral clarion calls; they are felt by many to indicate the sole legitimate rationale for scholarly work.

Why should there be such intensity in this debate? It is not difficult to discern. We are collectively confronted with a dilemma that has been

<sup>1</sup> Send correspondence to Immanuel Wallerstein, Fernand Braudel Center, University of Binghamton, P.O. Box 6000, Binghamton, New York 13902-6000.

discussed by thinkers for several thousand years. Beneath these antinomies lies the debate of determinism versus free will, which has found countless avatars within theology, philosophy, and science. It is therefore not a minor issue, nor is it one about which, over the thousands of years, a real consensus has been reached. I believe that our inability to find a way beyond this opposition constitutes a major obstacle to our collective ability to create a form of knowledge that is adequate for what I expect will be a quite transformed world in the coming century and millennium. I therefore propose to look at how this long-standing debate has been conducted within our community, that is, within the framework of that very recent construct, "social science." I intend to argue that the way the problem has been posed heretofore has made it insoluble. I intend also to argue that we are today at a point where we may be able to overcome the social constructions of the 19th century in ways that will allow us to move forward constructively, and collectively, on this question.

Let me start with determinism and free will in theological discourse. The concept that everything is determined seems to derive quite directly from the concept of the omnipotence of God, which is central to all the monotheistic religions at least. If there is an omnipotent God, then everything is determined by the will of God and to suggest otherwise would seem to be blasphemous. On the other hand, the churches of the world are in the business of regulating moral behavior. And determinism provides an easy excuse for the sinner. Has God indeed determined that we shall sin? And if so, should we try to counter the will of God? This is a conundrum that has plagued theologians from the beginning. One way out is to argue that God has bestowed upon us free will, that is, the option to sin or not to sin. It is however too easy a solution. Why would it have been necessary or desirable for God to have done this? It makes us seem like God's playthings. Furthermore, it does not provide a logically tight argument. If God has given us free will, can we exercise it in unpredictable ways? If so, is God omnipotent? And if not, can we really be said to have free will?

Let me say once again how impressed I have always been with the astuteness of John Calvin's attempt to resolve this dilemma. The Calvinist argument is very simple. Our destinies are indeed not predetermined, not because God could not predetermine everything, but because if humans assert that everything is predetermined, they are thereby limiting God's ability to determine. In effect, Calvin is saying, perhaps *we* cannot change our minds, but God can, or else God is not omnipotent. Still, as is well known, Calvinists were not persons to countenance immoral behavior. How then could humans be induced to make the necessary effort to behave according to the norms which Calvinists believed they ought to observe? Remember, Calvin was part of the Reformation attempt to refute

the doctrine of the Catholic Church that good deeds are rewarded by God (a view that, by derivation, justified the sale of indulgences). To get out of the box, Calvinists resorted to the concept of negative grace, which is in reality a familiar and very modern device of science, the concept of disproof. While we could not have foreknowledge of who was saved, since that would limit God's decisions, we could have foreknowledge of who was *not* saved. It was argued that God displays the prospect of damnation in the sinful behavior of humans, as sinful behavior is defined by the church. Those who sin are surely *not* saved, because God would not permit the saved so to act.

The Calvinist solution is so astute that it was subsequently adopted by its successor expression, the revolutionary movements of the 19th and 20th centuries. The analogous argument went like this. We cannot know for sure who is advancing the revolution, but we can know for sure who is *not* advancing it, those who act in ways that are sinful, that is, in ways that run counter to the decisions of the revolutionary organization. Every member is a potential sinner, even if the militant has behaved appropriately in the past. Members are thus continuously subject to the judgment of the revolutionary authorities as to whether or not they have gone against the will of God, that is, against the will of the revolutionary organization.

Nor was it only the revolutionary organizations that adopted the Calvinist solution. Essentially, modern science adopted it as well. We can never know with certainty whether a scientist has reached truth, but we can know when the scientist has sinned. It is when he has failed to follow the norms of appropriate scientific methods, as defined by the community of scientists, and therefore has ceased to be "rational," that is, when the scientist has stooped to politics, or to journalism, or to poetry, or to other such nefarious activities.

The Calvinist solution is astute, but it has one enormous drawback. It confers inordinate power on those humans—church authorities, revolutionary authorities, scientific authorities—who are the interpreters of whether or not other human actors are showing signs of negative grace. And who will guard the guardians? Is there then a remedy to this drawback? The consecrated remedy is to proclaim the virtue of human freedom. That good Calvinist, John Milton, wrote *Paradise Lost*, a marvelous poem extolling this remedy. There are many readers who have said that, behind Milton's ostensible vindication of God, his real hero was Lucifer, and that Lucifer's rebellion represented humanity's attempt to rise up against the constraint of the will of an unseeable and unknowable God. But the remedy seems almost as bad as the malady. Shall we praise Lucifer? After all, in whose interests does he act?

I have come to bury Caesar, not to praise him.

\* \* \*

Consider the Enlightenment. What was the sermon? It seems to me the essential message was anticlerical: humans were capable of rational judgment and hence had the ability to arrive at both truth and goodness directly, through their own best efforts. The Enlightenment represented the definitive rejection of religious authorities as judges of either truth or goodness. But who was substituted for them? I suppose one has to say the philosophers. Kant was anxious to take away from the theologians the right to judge either truth or goodness. He found it easy enough to do this for truth, but more difficult to do for goodness. Having decided that one cannot prove laws of morality as though they were laws of physics, he might have conceded goodness to the theologians. But he insisted that here too the philosophers could offer the answer, which for Kant was located in the concept of the categorical imperative.

However, in the process of secularizing knowledge, the philosophers enshrined doubt, and this proved to be their own subsequent undoing. For along came the scientists to proclaim that the philosophers were merely disguised theologians. The scientists began to challenge the right of philosophers as well as of theologians to proclaim truth, asserting very stridently that scientists were *not* philosophers. Is there anything, the scientists asked, that legitimates the speculations, the ratiocinations of the philosophers, anything that allows us to say that they are true? The scientists asserted that they, on the contrary, possessed a firm basis for truth, in which empirical investigation leads to testable and tested hypotheses and then to those provisional universals called "scientific theorems." The scientists, however, wiser or perhaps less courageous than Kant, wanted nothing to do with moral laws. They laid claim therefore to only one-half of the task the philosophers had inherited from the theologians. Scientists would search only for truth; goodness, they suggested, was not an area of interest since it was incapable of being an object of knowledge as science was defining that term.

The claims of the scientists that science represented the unique path to locate truth gained wide cultural support, and they came to be the preeminent constructors of knowledge in the course of the late 18th and early 19th centuries. However, at the beginning of this period, there was a small happening called the French Revolution, whose protagonists claimed to be acting in the furtherance of goodness. Ever since, the French Revolution has served as the source of a belief system at least as powerful as that provided by the rise to cultural predominance of science. As a result, we have spent the last 200 years trying to reunite the search for truth and the search for goodness. Social science, as it came to be established during the 19th century, was precisely the heir to both searches, and in some



ways offered itself as the ground on which they could be reconciled. I must however admit that social science has not been very successful in its quest; rather than reunifying the two searches, it has itself been torn apart by the dissonance between them.

The centrifugal pressure of the "two cultures" (as we now call the two searches) has been impressively strong. It has provided the central themes of the rhetoric of public discourse about knowledge. It has determined the structures of the universities in the course of their being rebuilt and reinvigorated in the 19th century. Its continuing strength explains the persistently high degree of passion about the antinomies to which I referred. It explains the fact that social science has never achieved true autonomy as an arena of knowledge nor ever acquired the degree of public esteem and public support to which it aspires and which it believes it merits.

The gulf between the two cultures was the deliberate construction of Newtonian-Cartesian science. Science was very sure of itself in this struggle. This is well illustrated by two famous declarations of the Marquis de Laplace. One was his *bon mot* in replying to Napoleon's query about the absence of God in his physics—"Sire, I have not found any need for that hypothesis" (cited in Koyré 1957, p. 276). The other was his unyielding statement about how much science could know: "The present state of the system of nature is evidently a resultant of what it was in the preceding instant, and if we conceive of an Intelligence who, for a given moment, embraces all the relations of beings in the Universe, It will be able to determine for any instant of the past or future their respective positions, motions, and generally their affections" (cited in Hahn 1967, p. 15). Triumphant science was not prepared to admit any doubts or to share the stage with anyone else.

Philosophy, and, more generally, what came to be called in the 19th century "the humanities," fell in public esteem and retreated to a defensive stance. Unable to deny science's capacity to explain the physical world, philosophy abandoned that domain entirely. Instead, it insisted that there existed another quite separate domain—the human, the spiritual, the moral—which was as important as, if not more important than, the domain of science. That is why, in English at least, philosophy assumed the label, "the humanities." From this human domain philosophy sought to exclude science or, at the very least, to relegate it to a very secondary role. As long as the humanities engaged in metaphysics or literature, science was quite willing to allow itself to be excluded, on the deprecatory grounds that these were nonscientific matters. But when the subject matter was the description and analysis of social reality, there was no accord, not even a tacit one, between the two camps. Both cultures laid claim to this arena.

A cadre of professional specialists on the study of social reality emerged

slowly and, be it said, unsurely. In many ways, the most interesting story is that of history. Of all the fields that constitute what we today call social science, history has the longest lineage. It was a concept and a term long before the 19th century. But the basis of the modern discipline of history was the historiographical revolution we associate with Leopold von Ranke. And the modern version of history, which Ranke and his colleagues called *Geschichte* and not *Historie*, was extraordinarily scientific in its fundamental premises. Its practitioners asserted that social reality was knowable. They asserted that such knowledge could be objective; that is, that there were correct and incorrect statements about the past, and that historians were obliged to write history "as it really happened," which is why they gave it the name of *Geschichte*. They asserted that scholars must not intrude their biases into the analysis of the data or its interpretation. Hence they asserted that scholars must offer evidence for their statements, evidence based on empirical research, evidence subject to control and verification by the community of scholars. Indeed, they even defined what kind of data would be acceptable evidence (primary documents in archives). In all these ways they sought to circumscribe the practices of the "discipline" and eliminate from history anything that was "philosophical," that is, speculative, deductive, mythical. I have called this attitude "history in search of science" (Wallerstein 1996).

But historians proved in practice to be timid scientists. They wished to stick extremely close to their data and to restrict causal statements to statements of immediate sequences—immediate particular sequences. They balked at "generalizations," which is what they called either inductions of patterns of behavior from specific instances or assertions of causal sequences in which two variables were less immediately linked in time and space. We may be generous and say that the historians did this because they were sensitive to the thin basis the collected empirical data in the 19th century afforded them for sound inductions. In any case, they were haunted by the fear that to generalize was to philosophize, that is, to be antiscientific. And so they came to idolize the particular, the idiosyncratic, even the unique, and thereupon to shun, for the most part, the label of social science, despite the fact that they were "in search of science."

Other practitioners were more audacious. The emerging disciplines of economics, sociology, and political science by and large wrapped themselves in the mantle and the mantra of "social science," appropriating the methods and the honors of triumphant science (often, be it noted, to the scorn or despair of the natural scientists). These social science disciplines considered themselves "nomothetic," in search of universal laws, consciously modeling themselves on the good example of physics (as nearly as they could). They had, of course, to admit that the quality of their data and the plausibility/validity of their theorems were far beneath the level

achieved by their confreres in the physical sciences, but they defiantly asserted optimism about future progress in their scientific capacities.

I should like to underline that this great *Methodenstreit*, as it was called, between idiographic history and the nomothetic trio of "real" social sciences was in many ways huff and puff, since *both* sides of this disciplinary and methodological debate fully acknowledged the superiority of science over philosophy. Indeed, science might have won the battle for the soul of the social sciences hands down were the natural scientists not rather snobbish in refusing to accept the importuning social scientists into full membership in the fraternity.

History and the nomothetic trio remained up to 1945 very much social sciences of the civilized world, by the civilized world, and about the civilized world. To deal with the colonized world of what were called "primitive peoples," a separate social science discipline was constructed, anthropology, with its separate set of methods and traditions. And the remaining half of the world, that of non-Western, so-called high civilizations—that is, China, India, the Arabo-Islamic world among others—was left to a special group of persons engaged in something that was given the name of "Oriental studies," a discipline that insisted on its humanistic character and refused to be considered part of the social sciences. It is obvious today why a cleavage between a social science of and for the civilized world and a second social science of and for the rest of the world seemed so natural to 19th-century European scholars and why it seems so absurd today. I shall not dwell on this issue (see Wallerstein et al. 1996). I wish merely to note that both the anthropologists and the Orientalist scholars, by virtue of the logic of engaging in a social science about the others/the nonmodern world/the barbarians, felt very much more comfortable on the idiographic side of the *Methodenstreit*, since the universalist implications of nomothetic social science seemed to leave no place for what they wanted to say.

In the 19th century, the idiographers and the nomothetists were in great competition as to who could be more objective in their work, which had a strange consequence for the macro-micro distinction. If one looks at the earliest works and major figures in each of these emerging disciplines, one notices that they wrote about very large themes, such as universal history or stages of civilization, and the titles of their books tended to be all-encompassing. This fit very well with the turn that modern thought was taking in that century, the turn to evolution as the fundamental metaphor. These books were very "macro" in the sweep of their subject matter, and they described the evolution of mankind. They were seldom monographic. But this macro quality of the research did not seem to last very long.

In the interests of creating corporate structures, the various social science disciplines sought to control the training and career patterns of those

who would enter the fraternity. They insisted on both originality and objectivity, and this turned them against macroscholarship. Originality required that each successive scholar say something new, and the easiest way to do that was to divide up the subject matter into subjects of ever smaller scope, in terms of time, of space, and of variables under consideration. The process of subdivision opened up endless possibilities of not repeating the work of earlier scholars. And by circumscribing the scope, social scientists believed they were making it more possible for scholars to be careful in their collection and analysis of data. It was the mentality of the microscope, and it pushed scholars to using ever more powerful microscopes. It conformed well with a reductionist ethos.

This microscopization of social science reinforced the gulf between idiographic and nomothetic social science. The two camps were equally in search of objectivity, but pursued diametrically opposite paths to achieving it, because they singled out opposite risks of subjectivity. The idiographic camp had two principal fears. They saw the danger of subjectivity deriving on the one hand from inadequate contextual understanding and on the other hand from the intrusion of self-interest. Insofar as one was dependent upon primary documents, one was obliged to read them correctly, not anachronistically or from the prism of another culture. This required considerable knowledge of the context: the empirical detail, the definition of boundaries, the use of the language (even, in many cases, the handwriting), the cultural allusions in the documents. The scholars hence sought to be hermeneutic, that is, they sought to enter into the mentality of persons and groups who were remote from them in order to see the world as the persons under study saw it. This required long immersion in the language and culture under observation. For the historians, it seemed easiest therefore to study their own nation/culture, in which they were already immersed. For the anthropologists, who by definition could not follow this path, it required so great an investment to know enough to study a particular group of "others" that it seemed sensible to devote one's life work to the study of one such people. And for the Orientalist scholars, doing their philological exercises well required a life-long improvement of difficult linguistic skills. There were thus, for each field, objective pressures which led scholars to narrow the scope of their research and to attain a level of specialization at which there were, at most, a few other persons in the world who had a matching profile of skills.

The problem of noninvolvement was also a serious one for idiographic scholars. The historians solved it first of all by insisting that history could not be written about the present and then by ending the "past" at a point relatively distant from the present. The argument was that we are all inevitably committed politically in the present, but that as we move backward in time we may feel less involved. This was reinforced by the fact

that historians made themselves dependent upon archives, and the states that provided the materials for the archives were (and are) unwilling to make the documents available about current happenings, for obvious reasons. The Orientalist scholars ensured their neutrality by avoiding real intercourse with the civilizations they studied. Theirs being primarily a philological discipline, they were immersed in reading texts, a task they could and largely did conduct in their study. As for the anthropologists, the great fear of the discipline was that some colleagues would "go native" and thereby be unable to continue to play the role of the scientific observer. The main control employed was ensuring that the anthropologist did not stay out in the "field" too long. All of these solutions emphasized remoteness as the mechanism of controlling bias. In turn, validity was guaranteed by the interpretative skills of carefully trained scholars.

The nomothetic trio of economics, political science, and sociology turned these techniques on their head. In order to avoid bias, they emphasized not remoteness but a very particular kind of closeness. Objective data were defined as replicable data, that is, precisely data that were *not* the result of an "interpretation." The more quantitative the data the easier it was to replicate them. But data from the past or from remote parts of the world lacked the infrastructural basis for the necessary guarantees of quality, or "hardness." Quite the opposite: the best data were the most recent and those which had been collected in the countries with the best infrastructure for the recording of data. Older or remoter data were necessarily incomplete, approximate, perhaps even mythical. They might be sufficient for the purposes of journalism or travel reports but not for science. Furthermore, even newly collected data rapidly became obsolete, since the passing of time brought ever-increased quality of data collection, especially in terms of the comparability of data collected in two or more sites. So the nomothetic trio retreated into the present, even into the immediate and instantaneous present.

Furthermore, insofar as one wanted to perform sophisticated operations on quantitative data, it was optimal to reduce the number of variables and to use indicators about which one could collect good data, hard data. Thus, reliability pushed these social scientists into constantly narrowing the time and space scope of the analyses and into testing only carefully limited propositions. One might wonder then about the validity of the results. But the epistemological premises solved this problem. Insofar as one believed that there existed universal laws of human behavior, the locus of the research became irrelevant. One chose sites of data collection according to the quality of the data it was possible to obtain, not because of the sites' superior relevance.

I draw from this the conclusion that the great methodological debates that illustrated the historical construction of the social sciences were sham

debates, which distracted us from realizing the degree to which the "divorce" between philosophy and science effectively eliminated the search for the good from the realm of knowledge and circumscribed the search for truth into the form of a microscopic positivism that took on many guises. The early hopes of social scientists that they could be modern philosopher-kings proved totally vain, and social scientists settled into being the handmaidens of governmental reformism. When they did this openly, they called it "applied social science." But for the most part they did this abashedly, asserting that their role was merely to do the research, and that it was up to others—the political persons—to draw from this research the conclusions that seemed to derive from this research. In short, the neutrality of the scholar became the fig leaf of their shame in having eaten the apple of knowledge.

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As long as the modern world seemed to be one long success story of technological triumph, the necessary political base to maintain a certain equilibrium in the system continued to exist. Amidst the success, the world of science was carried from honor to honor within this system, as though it were responsible for the triumph. The social sciences were swept along in the tide. No one was seriously questioning the fundamental premises of knowledge. The many maladies of the system—from racism to sexism to colonialism as expressions of the manifestly growing polarization of the world, from fascist movements to socialist gulags to liberal formalisms as alternative modes of suppressing democratization—were all defined as transitory problems because they were all thought to be capable of being brought under control eventually, as so many turbulent deviations from the norm in a world in which the trajectory always returned to the curve of linear, upward-moving equilibrium. The political persons on all sides promised that goodness was coming at the end of the horizon, a prospect presumably guaranteed by the constant progress in the search for truth.

This was an illusion, the illusion bred by the separation and reification of the two cultures. Indeed the separation of the two cultures was one of the main factors pushing the trajectories far from equilibrium. Knowledge is in fact a singular enterprise, and there are no fundamental contradictions between how we may pursue it in the natural and in the human world, for they are both integral parts of a singular universe. Nor is knowledge separate from creativity or adventure or the search for the good society. To be sure, knowledge will always remain a pursuit, never a point of arrival. It is this very fact, however, that permits us to see that macro and micro, global and local, and above all, structure and agency are not unsurpassable antinomies but rather yin and yang.

There have been two remarkable intellectual developments of the last

two decades that constitute an entirely new trend, signs that the world may be now in the process of overcoming the two cultures. These trends are only marginally the doing of social scientists, but they are wonderfully encouraging about the future of social science. I refer to what has been called "complexity studies" in the natural sciences and what has been called "cultural studies" in the humanities. I am not going to review the now immense literature in each of these two fields. Rather I shall try to situate each of these fields in terms of their epistemological implications for knowledge and their implications for the social sciences.

Why are complexity studies given that name? It is because they reject one of the most basic premises of the modern scientific enterprise. Newtonian science assumed that there were simple underlying formulas that explained everything. Einstein was unhappy that  $E = mc^2$  explained only half the universe. He was searching for the unified field theory that would in an equally simple equation explain everything. Complexity studies argue that all such formulae can at best be partial and at most explain the past, never the future. (We must of course be careful to distinguish between the dubious belief that truth is simple and the sound methodological injunction of Occam's razor, that we ought always to try to eliminate logical curlicues from our reasoning and include in our equations only the terms necessary to stating them clearly.)

Why is truth complex? Because reality is complex. And reality is complex for one essential reason: the arrow of time. Everything affects everything, and, as time goes on, what is everything expands inexorably. In a sense, nothing is eliminated, although much fades or becomes blurred. The universe proceeds, it has a life in its orderly disorder or its disorderly order. There are of course endless provisional orderly patterns, self-established, holding things together, creating seeming coherence. But none are perfect, because of course perfect order is death, and in any case enduring order has never existed. Perfect order is what we may mean by God, which is by definition beyond the known universe. So the atoms, the galaxies, and the biota pursue their paths, their evolution if you will, until the internal contradictions of their structures move them further and further away from whatever temporary equilibria they enjoy. These evolving structures repeatedly reach points at which their equilibria can no longer be restored, at points of bifurcation, and then new paths are found and new orders established, but we can never know in advance what these new orders will be.

The picture of the universe that derives from this model is an intrinsically nondeterministic one, since the aleatory combinations and the number of small decisions are too many for us to predict where the universe will move. But it does not follow that the universe can therefore move in any direction whatsoever. It is the child of its own past, which has created

the parameters within which these new paths are chosen. Statements about our present trajectories can of course be made, and can be made carefully, that is, can be stated quantitatively. But if we try to overdo the accuracy of the data, the mathematicians tell us we get unstable results.<sup>2</sup>

If physical scientists and mathematicians are now telling us that truth in their arenas is complex, indeterminate, and dependent on an arrow of time, what does that mean for social scientists? For, it is clear that, of all systems in the universe, human social systems are the most complex structures that exist, the ones with the briefest stable equilibria, the ones with the greatest number of outside variables to take into account, the ones that are most difficult to study.

We can only do what the scientists can only do. We can search for interpretative patterns, of two sorts. We can search for what might be called *formal* interpretative patterns, of the kind that state, for example, that all human social systems are historical social systems, not only in the sense that they follow a historical trajectory, but in the sense that they are born or emerge at certain times and places for specific reasons, operate according to specific sets of rules for specific reasons, and come to a close or die or disintegrate at certain times and places because they are unable any longer to handle their contradictions for specific sets of reasons. Such formal interpretative patterns are of course themselves subject to a finite relevance. One day, a given particular formal pattern may no longer operate, though for the moment this day may seem remote.

We can also search, however, for what might be called *substantive* interpretative patterns, such as the description of the rules of a particular historical social system. For example, when I term the modern world-system a capitalist world-economy, I am laying claim to the existence of a particular substantive pattern. It is of course a debatable one, and it has been much debated. Furthermore, like a series of boxes within boxes, there are substantive patterns within substantive patterns, such that, even if we all agree that the world in which we live is a capitalist world-economy, we may nonetheless differ about whether it has had discernible stages, or whether unequal exchange has been its norm, or about endless other aspects of its functioning.

What is crucial to note about complexity studies is that they have in no sense rejected scientific analysis, merely Newtonian determinism. But in turning some premises on their head, and in particular by rejecting the concept of reversibility in favor of the concept of the arrow of time, the

<sup>2</sup> The crystal has been shattered, Ivar Ekeland (1988, p. 73) tells us: "The qualitative approach is not a mere stand-in for quantitative methods. It may lead to great theoretical advances, as in fluid dynamics. It also has a significant advantage over quantitative methods, namely, stability."



natural sciences are taking a giant step in the direction of the traditional terrain of social science, the explanation of reality as a constructed reality.

If we now turn to cultural studies, let us start with the same question. Why are they called cultural studies? For a group of scholars so taken with linguistic analysis, to my knowledge this question has never been posed. The first thing I note is that cultural studies are not really studies of culture but studies of cultural products. This is the consequence of their deep root in the humanities and explains in turn their deep attraction to the humanities. For the humanities, in the division of the two cultures, were attributed above all the domain of cultural products.

The humanities were also attributed the domain of goodness, but they were very reluctant to seize hold of it. It seemed so political, so uncultural, so fleeting and unsolid, so lacking in eternal continuities. The personal path of Wordsworth from poet of the French Revolution to poet of poetry illustrates the repeated flight of the artists and the scholars of cultural products to the surer ground of "art for art's sake," an aesthetic turning inward. They comforted themselves with Keats's lines in *Ode on a Grecian Urn*, "Beauty is truth; truth beauty"—that is all/Ye know on earth, and all ye need to know."

To be sure, there were always those who asserted that cultural products were a product of the culture, and that this could be explained in terms of the structures of the system. Indeed, cultural studies as we know it today originated in England in the 1950s with persons who were arguing this long-standing theme. They were, let us remember, in search of a workers' culture. But then cultural studies took what has been called a linguistic turn or a hermeneutic turn, but which I think of as a 1968 turn. The revolutions of 1968 were against the liberal center and put forward the argument that not only was the Old Left part of this liberal center, but also that this liberal center was as dangerous as (if not more dangerous than) the true conservatives.

In terms of the study of cultural products, it meant that the enemy became not merely those who would study cultural products according to conservative, traditional aesthetic norms (the so-called canons) but also those (the Old Left) who would analyze cultural products in terms of their presumed explanations in the political economy. An explosion followed in which everything was deconstructed. But what is this exercise? It seems to me the core of it is to assert the absence of absolute aesthetics, to insist that we have to explain how particular cultural products were produced, when they were produced and why in that form, and then to proceed to ask how they were *and are* being received by others and for what reasons.

We are clearly involved here in a very complex activity, one in which equilibria (canons) are at best transient and one in which there can be no determinate future, since the aleatory elements are too vast. In the process,

the study of cultural products has moved away from the traditional terrain of the humanities and onto the terrain of the social sciences, the explanation of reality as a constructed reality. This is of course one of the reasons why so many social scientists have been receptive to it.

The move of natural scientists toward the social sciences (complexity studies) and the move of scholars in the humanities toward the social sciences (cultural studies) have not been without opposition within the natural sciences and within the humanities. The opposition has in fact been ferocious, but it seems to me that it has been largely a rearguard operation. Nor have the proponents of complexity studies or the proponents of cultural studies defined themselves as moving into the camp of the social sciences. Nor have all (or even most) social scientists analyzed the situation in this way.

But it is time that we all call a spade a spade. We are in the process of overcoming the two cultures via the social scientization of all knowledge, by the recognition that reality is a constructed reality, and that the purpose of scientific/philosophical activity is to arrive at usable, plausible interpretations of that reality, interpretations that will inevitably be transitory but nonetheless correct, or more correct for their time, than alternative interpretations. But, if reality is a constructed reality, the constructors are the actors in the real world and not the scholars. The role of the scholars is not to construct reality but to figure out how it has been constructed and to test the multiple social constructions of reality against each other. In a sense, this is a game of never-ending mirrors. We seek to discover the reality on the basis of which we have constructed reality. And when we find this, we seek to understand how this underlying reality has in turn been socially constructed. In this navigation amidst the mirrors, there are however more correct and less correct scholarly analyses. Those scholarly analyses that are more correct are more socially useful in that they aid the world to construct a substantively more rational reality. Hence the search for truth and the search for goodness are inextricably linked the one to the other: We are all involved, and involved simultaneously, in both.

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In his latest book, Ilya Prigogine says two things very simply: "The possible is richer than the real. Nature presents us in effect with the image of creation, of the unforeseeable, of novelty." And "science is a dialogue with nature" (Prigogine 1996, pp. 83, 177). I should like to take these two themes as the basis of my concluding remarks.

The possible is richer than the real. Who should know this better than social scientists? Why are we so afraid of discussing the possible, of analyzing the possible, or exploring the possible? We must move not utopias,

but utopistics, to the center of social science. Utopistics is the analysis of possible utopias, their limitations, and the constraints on achieving them. It is the analytic study of real historical alternatives in the present. It is the reconciliation of the search for truth and the search for goodness.

Utopistics represents a continuing responsibility of social scientists. But it represents a particularly urgent task when the range of choice is greatest. When is this? Precisely when the historical social system of which we are a part is furthest from equilibrium, when the fluctuations are greatest, when the bifurcations are nearest, when small input has great output. This is the moment in which we are now living and shall be living for the next 25–50 years.<sup>3</sup>

If we are to be serious about utopistics, we must stop fighting about nonissues, and the foremost of these nonissues is determinism versus free will, or structure versus agency, or global versus local, or macro versus micro. It seems to me that what we can now see clearly is that these antinomies are not a matter of correctness, or even of preference, but of timing and depth of perspective. For very long and very short time spans, and from very deep and very shallow perspectives, things seem to be determined, but for the vast intermediate zone things seem to be a matter of free will. We can always shift our viewing angle to obtain the evidence of determinism or free will that we want.

But what does it mean to say that something is determined? In the realm of theology, I can understand it. It means we believe that there is an omnipotent God, and that he has determined everything. Even there, we get quickly into trouble as I have suggested. But at least, as Aristotle would have put it, we are dealing with an efficient cause. But if I say that the possibility of reducing unemployment in Europe in the next 10 years is determined, who or what is doing this determining, and how far back shall I trace it? Even if you were to convince me that this had some analytical meaning (and that would be difficult), does it have any practical relevance? But does it follow then that it is merely a matter of free will, and that, were Dutch or German or French politicians, or entrepreneurs, or trade-union leaders, or someone to do specific things, I could then assure you that unemployment would in fact be reduced? Even if they, or I, knew what these things were, or believed we knew, what would motivate us to do them now when we did not do them previously? And if there were an answer to this, does that mean that our free will is determined by something prior? And if so, what? This is an endless, pointless, sequential chain. Starting with free will, we end up with determinism, and starting with determinism, we end up with free will.

<sup>3</sup> I have no place to argue this here but I have articulated this elsewhere (see Walzerstein 1995).

Can we not approach this another way? Let us agree that we are trying to make sense of the complexity, to "interpret" it usefully and plausibly. We could start with the simple task of locating seeming regularities. We could also try provisionally to assess the relative strength of various constraints on individual and collective action. This task we might call locating structures of the *longue durée*. I call this a simple task, but of course it is not at all an easy task. It is simple rather in the sense that it explains little, and also in the sense that it is a prior task, prior, that is, to other more complex tasks. If we do not have the structures clearly in mind, we cannot go on to analyze anything more complex, like for example so-called microhistories, or texts, or voting patterns.

Analyzing structures does not limit whatever agency exists. Indeed, it is only when we have mastered the structures, yes, have invented "master narratives" that are plausible, relevant, and provisionally valid, that we can begin to exercise the kind of judgment that is implied by the concept of agency. Otherwise, our so-called agency is blind, and if blind it is manipulated, if not directly then indirectly. We are watching the figures in Plato's cave, and we are thinking that we can affect them.

This brings me to Prigogine's second apothegm: "Science is a dialogue with nature." A dialogue has two partners. Who are they in this case? Is science a scientist or the community of scientists or some particular scientific organization(s) or is it everyman insofar as he or she is a thinking being? Is nature a living entity, some sort of pantheistic god, or God omnipotent? I do not think we know for sure who is engaged in this dialogue. The search for the partners in the dialogue is part of the dialogue itself. What we must hold constant is the possibility of knowing more and of doing better. This remains only a possibility, but not an unattainable one. And the beginning of realizing that possibility is ceasing to debate the false issues of the past erected to distract us from more fruitful paths. Science is at its very earliest moments. All knowledge is social knowledge. And social science lays claim to being the locus of self-reflection of knowledge, a claim it makes neither against philosophy nor against the natural sciences, but at one with them.

Much as I think that the next 25–50 years will be terrible ones in terms of human social relations—the period of disintegration of our existing historical social system and of transition toward an uncertain alternative—I also think that the next 25–50 years will be exceptionally exciting ones in the world of knowledge. The systemic crisis will force social reflection. I see the possibility of definitively ending the divorce between science and philosophy, and, as I have stated above, I see social science as the inevitable ground of a reunited world of knowledge. We cannot know what that will produce. But I can only think, as did Wordsworth about

the French Revolution in *The Preludes*: "Bliss was it in that dawn to be alive,/But to be young was very Heaven!"

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# Between Markets and Politics: Organizational Responses to Reform in China<sup>1</sup>

Douglas Guthrie

*University of California, Berkeley*

In China's economic transition, firms diversify assets through investments in the rapidly expanding service sector in response to organizational uncertainty. Using data from a random sample of firms in Shanghai, the author shows that there are two situations that cause this uncertainty: economic instability, where weak firms struggle to survive in the rapidly changing market system, and administrative instability, where large firms that were the most protected are now being forced to handle the responsibilities that were previously handled by the state. The result is that both types of firm seek stability by spreading out risk through investment in low-risk, fast-return markets, revealing much about the economic reforms.

The overall strategy of a firm reflects, at any given point in time, important organizational facts. . . . Any decision to fundamentally alter the deployment of internal resources represents a major structural change. These kinds of changes do not occur often; when they do, they provide us with an opportunity to examine the conditions under which actors can alter their social structures. (Neil Fligstein)

## INTRODUCTION

Study of organizational strategies and practices offers a way to examine the institutional and structural circumstances under which large-scale economic actors operate. While Fligstein (1985, 1987, 1990, 1991) studied the

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structural circumstances of changes in American capitalism, similar organizational-level analyses are also fitting for study of the institutional changes of economic transitions from planned to market economies. Indeed, one of the central problems with many studies of economic transitions—many of which are based upon individual-level data—is that studies based upon individual and household income say a lot about income distribution and changing patterns of stratification, but they reveal little about the actual structure of markets, the changing institutional environments in which economic actors are operating (e.g., hardening budget constraints), and how these changing institutional environments influence practices and decisions in the economic transitions. Despite the fact that studies of this genre claim to show evidence about the “transition from redistribution to market mechanism,” and the general “shift from hierarchies to markets” (Nee 1989, p. 663), they show no direct or concrete evidence about the fate of the hierarchy of former command economies. Making a similar point in a recent symposium on economic transitions, Anthony Oberschall (1996, p. 1035) quipped, “Can [variables of income and income inequality] give us any clue about the shape of emerging institutions? . . . There are many questions on institutional change for which these surveys have no answers.” At the same time, transition studies that have taken a more state-centered or institutional approach to the economic reforms, focusing on changes in industrial structure, on changing institutions, and on state-market relations, often begin with evidence derived from aggregate data on increasing productivity across sectors and regions. In these studies, aggregate changes of the economic transitions are often not connected to the decisions and practices of actual economic actors in the transforming marketplaces.

Direct observations of organizational actions—and the institutional environments that define those actions—can contribute significantly to the study of economic transitions. From the new institutional economics to the new institutionalism in organizational sociology, there is a large body of theoretical and empirical research pointing the direction for research on organizational action in economic transitions. To some extent, the study of organizational practices in transforming industrial societies is an approach that has already yielded valuable fruit. Stark's (1996) work on industrial change in Hungary, also a contribution to the above-mentioned symposium, is an important piece of work in its focus on organizational decisions and practices and the institutional changes that drive them. Similarly Walder's (1992a) work on the structure of property rights and the impact of administrative hierarchies on organizational action under China's redistributive system focuses on what organizational actions can tell us about institutional environments. Walder's (1995) more recent theoretical work on organizational characteristics and institutional environ-

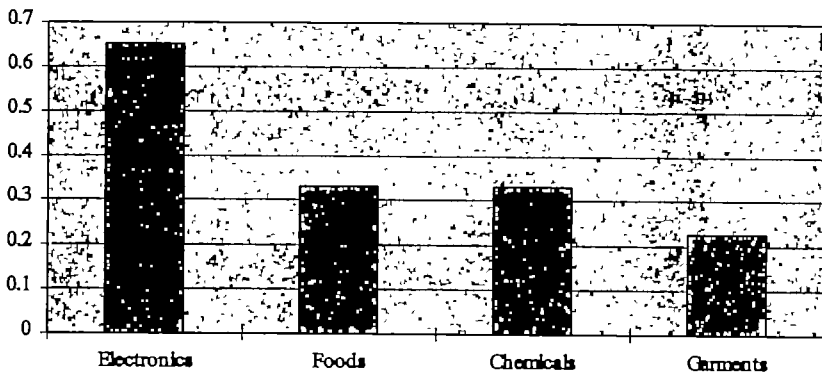


FIG. 1.—Proportion of firms adopting the service sector diversification strategy in stratified random sample of 81 firms from four sectors in industrial Shanghai, 1995.

ments in China's economic transition is also an important place to begin for any organizational and institutional study of transition. Yet we are in need of more research that begins with organizational analyses of economic transitions.

Following these approaches, I direct my focus to the actions of industrial organizations in China's transforming economy to study the effect of broader institutional reforms on organizational action in emerging markets. My assumption is that organizational decisions and practices can illuminate a great deal about the transformation in China's economic transition. In this study, I examine one economic practice that illuminates important structural and institutional changes in China's economic transition. Service sector diversification, a practice where organizations choose to invest in the low-risk, fast-return markets of the rapidly expanding service sector economy, is a strategy that a significant number of Chinese firms are adopting in the economic transition. Figure 1 shows the proportion of organizations that have adopted this practice by sector, based on a sample of industrial organizations in Shanghai.<sup>2</sup>

The service sector is an important area of research if only because it is one of the most rapidly expanding economies in China. Nationally, average daily transactions in the service sector rose over 700% between 1985 and 1995 (China State Statistical Bureau 1996, p. 5). In addition, this sector now comprises a significant portion of urban labor forces: in Shang-

<sup>2</sup> The proportions reported in fig. 1 are based on data gathered in 1995 from a sample of 81 organizations in Shanghai. The sample was randomly generated and stratified by sector, so it is fairly safe to infer these proportions back to the broader sectors from which the samples were drawn.



hai's urban districts in 1993, 43% of the labor force was located in the service sector economy (nationally the number is 26%; China State Statistical Bureau 1994, pp. 111, 108). Some firms are choosing to take advantage of this rapidly expanding economy by diversifying assets through investments into the service sector. While it appears in figure 1 that there may be a sectoral dimension to the adoption of this practice in China's industrial economy, the analysis below will show that there are deeper structural and institutional factors that are at the root of a firm's decision to adopt this diversification strategy. An examination of service sector diversification will help illuminate the structural and institutional situations that give rise to specific organizational decisions and practices in periods of economic upheaval and transformation. Thus, much as Fligstein (1991, p. 317; see also 1990) explored the "structural historical conditions under which diversification takes place" in American capitalism, in this study, I explore the structural and institutional conditions under which a specific type of diversification is adopted as an economic strategy in China's transforming industrial economy.

The practice of diversification is one that has been widely researched in economics and sociology. Diversification in established market economies is generally a risk-spreading, growth-oriented strategy among powerful firms. While service sector diversification in China is a strategy of risk aversion, the practice is also directly linked to an organization's economic health, as well as to its structural location in China's administrative hierarchy. An important aspect of service sector diversification in the Chinese case is that it is a risk-spreading practice that is adopted less by powerful firms and more by firms that are experiencing extreme uncertainty in the emerging markets of the economic transition. Thus, in the Chinese context, this economic practice is not a growth-oriented strategy as much as it is a lifeboat strategy; it is a strategy to create stability in a world that has become suddenly uncertain. Essentially, an analysis of this practice shows that there are two types of uncertainty that increase the likelihood that a firm will invest resources in the rapidly expanding service sector in China's transforming economy; that is, there are two types of organizations that are experiencing extreme uncertainty in the economic transition. First, there is economic uncertainty: firms that are weak economically are likely to adopt the diversification strategy. Economically weak firms are struggling to survive in the transition, and in the face of the overall tightening of fiscal constraints in the transition away from the planned economy, these firms are seeking stability through investments in low-risk, fast-return markets. The second type is administrative uncertainty: firms that are located under the highest level of governmental administration are also significantly more likely to diversify into the service sector. As administrative and economic responsibilities are pushed down the hierar-

chy of the former command economy, the industrial organizations that are at the highest levels of this hierarchy are being asked to assume the greatest amount of responsibility in the transforming economy. The result is that, like the economically weak firms, these previously privileged firms are experiencing an uncertainty that pushes them to invest assets in low-risk, fast-return markets. This second type of uncertainty, I will argue, suggests that budget constraints are in fact being hardened for these upper-level firms, contrary to recent predictions that these firms continue to dip freely into state coffers in the economic reform. Whereas firms at the highest levels of the administrative hierarchy were the most protected under the redistributive system (they had the most access to state resources), it is now precisely these firms that are experiencing the most uncertainty under the economic reform because they are suddenly being set adrift by the state; they no longer have open access to state funds, and they are receiving little guidance from state administrative offices in the economic reform.

The analysis that follows will begin with a theoretical discussion of the practice of diversification and what analyses of this practice might reveal about institutional and economic structures. Within this section, there is a brief discussion of research on economic transitions and the strengths and weaknesses of past research in this area. Following that, I present an empirical analysis of the adoption of the diversification strategy by organizations in a sample of firms in the industrial city of Shanghai. Finally, a discussion of this analysis centers around the institutional changes of industrial structure that are defining China's transition to a more market-oriented system. Throughout the analysis, I work from the perspective that the decisions and practices of organizational actors serve as a window into the changing institutional environments in which economic decisions are being made. They are thus a window into the institutional and structural changes of China's economic transition.

## THEORETICAL ISSUES

### Studies of Diversification

Diversification has long been a part of market societies.<sup>1</sup> As an economic practice of business organizations, diversification can be traced at least

<sup>1</sup> Some scholars of diversification define the practice narrowly as the number of manufacturing markets served by a given manufacturing organization's output (Gort 1962, 1964, 1984). Others (Luffman and Reed 1984, p. 3), however, define the term much more broadly as "development based on [an organization's decision] to move into a completely new area of business in which the company has few (if any) skills." It is this latter, more broadly defined, view of diversification that will be the focus of the current study.

as far back as Jakob Fugger's firm in the 16th century (which was involved in mining, real estate, banking, and trading) and the famous trading companies of the 18th century (Gort 1984). In the industrial West, diversification has been a fairly widespread practice of large manufacturing firms since the early part of the 20th century (Fligstein 1990, 1991). Study of this practice has become an important part of understanding industrial structure in the West because it is intimately tied to economic constraints, market environments, and the risks that organizations face (Gort 1984; Rumelt 1986; Scott 1993). In addition, other scholars have pointed out that the spread of diversification as a practice was intimately tied to organizational structure—specifically the multidivisional firm—and management practices (Chandler 1962; Fligstein 1985, 1987), and it was also tied to the changing institutional environments in which firms were situated over the course of the 20th century (Fligstein 1985, 1987, 1990, 1991). Thus, study of diversification has not only given us an understanding of why firms choose to diversify, but it has also provided clues about the institutional structure of the period during which firms were adopting this practice.

While many scholars have been interested in the relationship between diversification and economic performance (Luffman and Reed 1984; Scott 1993; Rumelt 1986; Markides 1995), that is, the *effects* of diversification, others have paid more attention to the *causes* of diversification. The central questions that guide research on the causes of diversification are, What types of firms diversify? What are the structural circumstances that lead to diversification? And what does the practice of diversification illuminate about the historical circumstances and institutional environments in which diversifying firms (and their nondiversifying counterparts) are situated? Williamson (1975) and Gort (1984) have argued that diversification is fundamentally a risk-spreading practice that firms adopt to attenuate the unpredictable changes of profitability in different industries and sectors. Firms diversify because the fate of different industries and markets can be tenuous, and the ability to spread production across multiple markets reduces the extent to which organizations are threatened by the variable profitability of a given market. While diversification may be a risk-spreading strategy (adopted by risk averse actors), a review of diversification practices in the West shows that it is typically large, stable, and economically healthy organizations that branch out into other industries and sectors. In many cases, diversification begins with merger movements, and often diversifiers emerge as leading producers in their new industries (Gort 1984; see also Eis 1978). Thus, although the organizational actors adopting this practice may be risk averse, they are by no means economically weak or unstable. As Gort (1984, p. 27) points out regarding firms in the first half of the century in the United States, firms that were diversi-

fied across multiple industrial sectors accounted for less than 1% of the total number of firms in the six primary sectors of the economy, yet these firms accounted for a large share of the production in those sectors.<sup>4</sup>

More broadly oriented studies offer similar insights about the types of firms that diversify, but they also view the actions and decisions of organizations in the historical and institutional environments in which they occurred. Fligstein's (1985, 1990, 1991) organizational and institutional view of diversification presents a much deeper explanation of the phenomenon than other market-driven explanations of diversification (e.g., Chandler 1962; Rumelt 1986; Williamson 1975). In Fligstein's analysis, the decisions and practices of firms offer important insight into the institutional and structural circumstances of a given period. In linking organizational strategies to institutional environments, Fligstein shows that individual actors within firms, the economic and structural circumstances of firms, and the institutional environments in which firms are located were all important for the decisions and practices of firms, especially in periods of rapid transformation or general reorganization. In Fligstein's analysis, two institutional factors significantly shaped the emergence of diversification as a widespread practice in the United States. First, politically defined institutional environments shaped the practices of firms through antitrust laws and policies. Second, as organizations adopted these practices, the practices spread across organizational fields through mechanisms articulated in DiMaggio and Powell's (1983) concept of institutional isomorphism. Thus, while Fligstein has no argument with earlier assertions that large, economically powerful firms adopted this strategy, he also argues that the practice itself is integrally linked to the institutional environments in which firms are situated. Fligstein studied organizational strategies and practices as a window into the institutional arrangements of American capitalism, but this approach is also useful for study of periods of economic upheaval and change, such as that during the transition from a planned to a market economy.

#### Transition Studies and China's Economic Reforms

Several studies have addressed the issues of economic transition through analyses of individual-level data.<sup>5</sup> One problem with studies of economic

<sup>4</sup> The six sectors Gort refers to here are those that were covered in the 1954 economic census: manufacturing, extraction and minerals, wholesale trade, retail trade, service trade and public warehousing.

<sup>5</sup> The current study is primarily about the relationships among institutional change, economic instability, and the organizational practices that reflect these circumstances. With respect to the literature on economic transitions, I am mainly interested in the gaps that a study of organizational practices can fill. Therefore, while I identify some of these gaps in the brief literature review on economic transitions here, a substantial

transition that begin with individual-level data is that the data often do not fit the research question. Some individual-level studies have focused on research questions that fit well with individual-level data, such as returns to education (Peng 1992; Xie and Hannum 1996; Hannum and Xie 1994; Gerber and Hout 1995) or the relative returns to bureaucratic position in the economic transition (Róna-Tas 1994). Other studies have taken individual-level data much further to make broad sweeping claims about the emergence of markets, the transition from redistribution to market mechanisms, and the fate of the hierarchy of the former command economy (Nee 1989, 1991, 1996).<sup>6</sup> The central problem with this type of research is that these studies make inferences and draw conclusions about markets, institutions, and social structures, with few direct observations of these factors (for a discussion, see, esp., Walder [1996]; see also Oberschall 1996, pp. 1028, 1035, 1037; Parish and Michelson 1996, p. 1042; Fligstein 1996a). If the goal is to draw conclusions about markets and institutions in economic transitions, then direct observations of the decisions and practices of market actors and the institutional structures in which they are operating (as opposed to the income of the market actors) may make more sense for adding to the debate on market transitions.

Industrial organizations were central to the planned economy and the redistributive system; focusing on the changing practices and the changing environments in which industrial organizations are situated is fundamental to understanding economic transitions from plan to market. Walder's work has been crucial in this area of research not only because of his direct focus on institutions (e.g., property rights and fiscal systems) and institutional structure but also because of his focus on organizations. Organizations are a fundamental part of industrial economies, and understanding organizational structures and practices is crucial for understanding industrial economy and economic change (Stark 1996; Walder 1986, 1992a, 1992b, 1995; Lin and Bian 1991; Bian 1994; Lincoln, Gerlach, and Ahmadjian 1996; Groves et al. 1995). Nevertheless, we still lack adequate empirical observations of organizational action in urban industrial China

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review of the transitions literature is beyond the scope of this article. For more extensive reviews of this literature see Róna-Tas (1994), Nee (1996), Walder (1995), and Lin (1995).

<sup>6</sup> While Nee (1996) appears to see his research as being in direct dialogue with the likes of Róna-Tas, it should be noted that Róna-Tas's claims remain much more in line with his data. Róna-Tas employs individual-level data to draw conclusions about the economic advantages of elites in Hungary's economic transition. Nee, in contrast, employs his data—which are direct observations of stratification mechanisms—to draw conclusions about institutions and markets. For Nee, changing patterns of stratification are employed to draw inferences about institutions such as labor markets, the redistributive hierarchy, and market distribution of products

in the economic transition. In the study of China's industrial economy, one of the few projects that has systematically analyzed organizational practices is Walder's (1992a) study of organizational action under the redistributive system.<sup>7</sup> Walder's (1995) later work focuses on organizational structure and institutional arrangements in the economic transition, but this study is primarily theoretical in nature and lacks any systematic data on comparative observations of economic actions and practices across organizations.<sup>8</sup> Below I present an approach to the study of economic re-

<sup>7</sup> It should be noted that even this study of organizational practices is based on individual-level data for an urban area (Tianjin); the data were used as representative of the organizational level for the purposes of the study. Organizational-level data were based on characteristics that each individual respondent revealed about her or his workplace: organizational size, overseeing governmental office, sector, and nonwage benefits. Thus critical organizational-level data, e.g., profits, turnover, etc., were not available for the study. It should also be noted that, although this study (Walder 1992a) has been viewed by some scholars (e.g., Nee 1996, p. 943) as informative with respect to the period of economic transition, it is fundamentally about the economic structure of the redistributive system in prereform China (Walder himself makes no claims about the study's applicability for the period of economic transition). The reason for this has to do with the indicators: although the data were collected in 1986, the dependent variables of the study are the practices of organizations in the form of nonwage benefits that organizations offer to their employees, and since the majority of these practices were either institutionalized or awarded (in the case of housing distribution) before the economic reform began, it would be problematic to draw conclusions about the economic transition based on these indicators. Organizations were offering housing and other nonwage benefits before the economic transition, and if the *i*th respondent individual in Walder et al.'s (1989) survey was allocated housing before the economic transition began (likely as the survey was conducted in 1986, only eight years after the start of the economic transition, two years after the reforms turned to urban areas) the dependent variable would then reveal nothing about how that organization's resource availability changes in the face of economic changes. This is especially true given the fact that the average age of the individuals in Walder's study was 39.1 years old (Walder et al. 1989, p. 20). When people are allocated housing, it is generally done when they begin working at an organization or when they get married. With a mean age of 39.1 years old and relatively low job mobility in China (at least this was still the case in 1986 [see Walder 1992a, p. 526]), I think it is safe to assume that many of the people in the sample had been allocated housing (or not) before the economic transition began; most people have either been awarded housing or not by the age of 31 (eight years before the study was conducted), and it is generally only allocated once. In situations for which that is the case, the allocation of housing to the respondent indicates nothing about slack resources *during the economic transition*. If a respondent is living in work unit housing, we do not know when they were awarded the housing, and therefore, we do not know what, if anything this says about the period of economic transition.

<sup>8</sup> There is a significant amount of state statistical data employed in Walder's (1995) discussion, but while this type of data tells us broadly of industrial organization, it should not be mistaken for systematic comparative observations on firms' practices. Studies that have employed organizations as the units of analysis in the study of the economic transition are Groves et al. (1995), Jefferson and Xu (1991), and Reynolds (1987); see also Rawski (1994).

forms that focuses on the economic decisions of organizational actors. I break from past analyses in that I do not begin with the assumption that growth in output, performance, and entrepreneurial activity are the best indicators of economic reforms. In fact, as I argue below, focus on these types of indicators may cause us to miss many other things going on in economic transitions, particularly for the cases of faltering firms.

### Organizational Uncertainty in Economic Transitions

A general problem with much of the research on economic transitions is that there has been a lopsided focus on the cases where productivity, changes in economic performance, and entrepreneurial activity are taking place. In many analyses, the focus is often on "the pursuit of power and plenty by economic actors in society" (Nee 1996, p. 945). For scholars who are focused more on state-market relations in the industrial economy, the studies often begin with the puzzle of explaining differential gains in industrial output or productivity (Jefferson and Rawski 1994; Chen et al. 1988). For these studies, the evidence of economic transition begins with performance and the economic arrangements that have led to increasing productivity. Even Walder (1995), who has addressed many institutional issues directly, presents an argument that is based on variation in aggregate measures of productivity. Walder's (1995, pp. 268–69, 294; see also Walder 1994, p. 63) hypothesis that soft budget constraints remain intact in upper levels of the administrative hierarchy is rooted in the observation that the township and village public sectors have experienced significant gains in industrial performance (based on total factor productivity) while the urban industrial sector has developed at a much slower rate in terms of output and productivity. But how do we know that budget constraints are not being hardened for large-scale urban industrial enterprises? Hard budget constraints will not *necessarily* lead to performance gains, and, therefore, slower rates of change in these areas cannot be taken as evidence that budget constraints are not being hardened in the urban industrial economy.<sup>9</sup> In Walder's transition study, no direct organizational-level evidence is presented to support the assertion that budget constraints are not being hardened in the higher levels of the administrative hierarchy,

<sup>9</sup> An argument could be made that we should actually expect the opposite: hardened budget constraints would drastically alter the production patterns and protection that firms came to expect under the command economy, and it would not be surprising that many firms would be at risk of market failure (as was the case in the textiles sector in Shanghai in 1994–95). If that was occurring, aggregate measures of productivity would reveal nothing about these changes. The point here is that slow gains in productivity are not sufficient evidence to conclude that changes in budget allocation have not occurred.

aside from the comparatively slower growth in output and productivity in that sector of the economy. The hardening of budget constraints in these sectors is an empirical question that slower growth rates cannot suffice as evidence to answer.

Behind this type of argument lies a line of reasoning very similar to that of neoclassical economic theories: reforms—loosely defined as a tightening of fiscal constraints—will breed efficiency, and if we observe a lack of economic efficiency (measured in productivity), then we can assume that reforms have not been enacted. But this may not be the case. It could also be the case that reforms are being enacted (i.e., budget constraints *are* being hardened), but economic actors are failing to adapt to the necessities of market environments. If we are going to draw conclusions about the hardening of budget constraints—an issue that has direct implications for theories of marketization—rather than observe productivity (or a lack thereof) a better tack would be to draw a sample of firms and observe their economic situations, their economic practices, and the ways their actions have changed over the course of the reform (see Stark 1996; Fligstein 1996b; see also Walder 1996). If soft budget constraints were remaining intact for organizations in the upper levels of China's administrative hierarchy, we would expect there to be no organizational responses to the institutional changes of the reform.

Gauging the progress of an economic transition as a function of aggregate gains in industrial performance and entrepreneurial activity ignores the fact that economic reforms may be taking hold, but this may be true irrespective of increased productivity, profitability, entrepreneurial activity, or higher incomes. The problem with this type of analysis lies in the assumption that market reforms will breed efficiency and vitality, and a lack of efficiency and vitality indicates a lack of reform. But where else would we look for evidence of the reforms? Theoretical and empirical insights of economic sociology provide us with some tools for answering this question. If reforms are happening in areas of the economy that are not growing, it is safe to assume that there will be a great deal of uncertainty for firms that formerly dipped—with few limits—into state coffers. And past research has shown that organizational actions and decisions are often adopted in response to uncertain market environments and situations (e.g., Stark 1996; Fligstein 1990, 1991). An empirical examination of organizational actions in the face of uncertainty may be a necessary supplement to studies based on aggregate increases in productivity or vitality of the private sector in analyses of economic transitions.

Fligstein's (1996b) theoretical treatise on the study of "markets as politics" offers several critical insights for research on societies that are engaging in the political project of market construction and transformation. In Fligstein's view, the state, large-scale organizations, and the preexisting



institutional structure of the economic system all become important factors in the political project of market construction. While Fligstein's emphasis on preexisting institutional structures links his work to studies that emphasize "path dependence" (Stark 1992, 1996; Walder 1995), more important, his perspective offers a framework for understanding economic action in unstable markets. According to Fligstein, economic actors are not necessarily the profit maximizers that classical economic models have posited them to be. Actors may still be rational but what they do is strive to create stable worlds. "In markets, the goal of action is to ensure the survival of the firm. No actor can determine which behaviors will maximize profits (either *a priori* or *post hoc*), and action is therefore directed toward the creation of stable worlds" (Fligstein 1996*b*, p. 659). Fligstein's view of market action is not a matter of rational action in worlds of perfect and imperfect information (in the manner of Stiglitz and Weiss 1981); rather the focus is on linking survival and profit-maximizing strategies in one general model of market action. For dominant actors in the market, the drive for stability usually amounts to profit-maximizing strategies of market domination (and thus a solidification of their dominant market position). But for "challenging" actors, whose positions are not inherently stable in the market, their worldviews may lead to strategies of stability that do not always make profit-maximizing sense. Challenging actors may be more likely to adopt strategies that emphasize short-term survival in market situations. Thus actors that adopt lifeboat strategies have one central thing in common with market actors that are adopting strategies that appear to make more sense in conventional, profit-maximizing terms: both types of actors are seeking to create stable worlds. While the focus in market analyses is typically on successful action in markets (measured by productivity, profits, etc.) we should also be able to observe firms that are adopting lifeboat strategies of survival and stability, particularly in precarious market situations. Through observing these firms that are adopting strategies of short-term survival, we may be able to infer that these firms are experiencing unstable worlds, and by linking these firms to their institutional and organizational environments, we can ask how or why their worlds have become unstable.

Fligstein's view of market activity is critical for making sense of the processes of marketization that are occurring in the Chinese urban industrial economy, especially at the upper echelons of the administrative hierarchy. The fact that we do not observe rapid performance gains in these areas may mean one of two things: (1) that this sector of the economy is still plagued with problems of softened budgets and still operates much like the planned economy did, or (2) that reforms are being enacted but the responses of firms have not been as successful as they have been in the smaller-scale rural areas. If the latter is the case, we need to view

the economic actions of these firms more broadly as stability-seeking and survival strategies and incorporate those alternative strategies alongside productivity and successful management in an analysis of the economic reforms. Under the redistributive system, firms' worlds were inherently stable: there was never a question that the state would cover costs with funds from state coffers—that is the nature of the redistributive system. In this type of situation, there would be no incentive for firms to adopt stability-seeking or survival strategies. It follows from this view that, if firms' worlds have changed significantly, if budget constraints are being hardened, we should be able to observe firms adopting stability-seeking strategies in response to those changes, even if they are not becoming more efficient economically.

The intersection of Fligstein's theoretical perspective and the research approaches of Walder and Stark provides a foundation for further research on organizational decisions and practices in China's economic transition. As legally standardized accounting systems are set in place and the state attempts to standardize taxation and begins the process of hardening budget constraints, there is a state-induced crisis that has caused various responses by firms as they seek stability in the market (Fligstein 1996*b*, pp. 668–69; see also 1991, pp. 314–15; 1990). Below I apply this theoretical lens to the study of China's economic transition. My account begins with the fact that the state has changed the "rules" of society (North 1990) but then goes on to ask what these changes have actually meant for economic actors.<sup>10</sup> As a result of these rule changes, some firms adopt strategies to deal with these changes. Central questions for the analysis are, Will we observe firms diversifying assets and spreading out risk, as Stark found was the case in Hungary? If firms are diversifying, do they tend to be unstable firms seeking lifeboat strategies for survival or are they large healthy firms adopting growth-oriented strategies, as is the case with diversifying firms in advanced Western societies? What types of institutional and administrative positions do diversifying firms occupy in China's industrial hierarchy? Will the broad-based institutional changes—

<sup>10</sup> Though an extensive discussion of these state-level changes is beyond the scope of this study, it should be noted that the Chinese government has changed the "rules" in a number of ways. From internal policy changes such as the Enterprise Self-Responsibility System (see the discussion section below) to many legal changes (e.g., PRC 1984, 1985, 1986, 1988, 1992, 1994, 1995), the state has potentially altered the environments in which firms operate. This study is not about the politics or history of these changes *per se*. Rather the empirical project behind this study is to explore whether or not these changes have significantly altered firms' environments and how we would measure this fact. Some scholars have argued that gains in productivity are the best measure of the meaning of these changes. I argue that, even where we cannot see gains in productivity, evidence that these changes have been meaningful can be found through observations of organizational decisions and practices.

the state-induced crisis—of the economic transition be mediated by institutional and political environments? Will diversification patterns fall in line with Walder's predictions that firms at higher levels of this industrial hierarchy are the least marketized and therefore the least likely to experience uncertainty in the reforms? The empirical analysis below will address these questions directly.

## THE EMPIRICAL CASE OF DIVERSIFICATION IN INDUSTRIAL SHANGHAI

### Data

The quantitative data on organizational structure and practices were gathered through face-to-face on-site interviews with managers or factory directors of 81 industrial firms randomly sampled across four industrial sectors. The interviews were conducted in 1995 between the months of February and July. The sectors were chosen based on parameters of theoretical and substantive interest.<sup>11</sup> Comparative information on these sectors is presented in table A1 in the appendix.

Within each of the four sectors, organizations were randomly selected from the sector lists of organizations in the *Chinese Directory of Organizations and Institutions* (Directory 1993). The *Directory* is a 4,500-page list of over 160,000 organizations that are registered in Beijing, Shanghai, Tianjin, Inner Mongolia, Hebei Province, Shanxi Province, Liaoning Province, and Jilin Province. There are roughly 8,800 organizations listed in the Shanghai section of the directory. Each geographical section of the directory is divided into 19 large sectoral categories, which are subdivided into a total of 75 smaller, more specific sectors. With the rapid change and development that is occurring throughout China, it is impossible that this 1993 directory—containing information that was finished being updated on October 10, 1992—is a *complete* list of organizations in the given

<sup>11</sup> Selection of the sectors for the study was based on a  $2 \times 2$  matrix cross-referencing state presence (high/low) with asset/technological intensity (high/low). The sectors chosen for the study were: chemicals (petrochemicals; high/high), foods (high/low), electronics (low/high), and garments (low/low). Determination of these sector characteristics was based upon interviews with governmental officials and managers from the pilot study. For each of the sectors, the 20 organizations in the sample of the sector roughly represent a 10% sample of the producing industrial organizations in that organizational field. In terms of sample size by sector, this sampling is similar to other organizational studies. Dobbin, Sutton, Meyer, and Scott's recent study (see Dobbin et al. 1993; Sutton et al. 1994), e.g., was based on 279 (responding) organizations spread across 13 sectors, which breaks down into about 20 organizations per sector (assuming even response rates across sectors; Dobbin et al. [1993] sampled equally across the sectors, as I did).

geographical areas in China. However, this list may be among the closest we can come to a complete list of organizations in Shanghai. The sector lists in the *Directory* are far more complete than those found in the phone book, for instance, the only other semicomplete public list of organizations by sector to be found in China at the time this study was conducted. Further, as I am interested in the dynamics of institutional practices of relatively large organizations (more than 50 employees; see Edelman 1990, 1992; Dobbin et al. 1993), we can be even more certain that this list is among the most up-to-date lists of large organizations available.<sup>12</sup>

The study was limited geographically to the city of Shanghai, as opposed to Shanghai Municipality. This means that all organizations located in counties (*xian*) were eliminated from the study, while only those located in city districts (*qu*) were included. Thus the study is one of organizations across four sectors in the nine districts in the city of Shanghai. There are 233 organizations that make up the universe of organizations in the electronics sector (*diansi hangye*; *Directory* 1993, pp. 482–92); 204 organizations comprise the universe of organizations in the “light industry” food stuffs sector (*shipin hangye*; *Directory* 1993, pp. 347–62); there are 176 organizations in the petrochemicals sector (*huaxue gongye*; *Directory* 1993, pp. 413–23); and there are 301 organizations in the garments sector in the city of Shanghai (*fushuang hangye*; *Directory* 1993, pp. 386–400). The only requirements for an organization to be included in the sample were that the organization employ at least 50 individuals and that it be a production-oriented industrial unit in the appropriate sector (i.e., some portion of the factory turnover had to be derived from sales of manufactured products).<sup>13</sup> The overall response rate for the study—the rate of sampled organizations that agreed to be interviewed—was 90%.

<sup>12</sup> I also cross-checked this assertion with many of the governmental and organizational informants that were interviewed in the pilot and preliminary stages of this study. Obviously, bureau officials have complete lists of the firms in their sectors (for tax purposes, etc.), but I wanted to conduct this study—and particularly the interviews—independent of governmental control or influence. Virtually all of the informants affirmed that this source would yield the most complete sector lists of any source. Other complete lists are likely to exist, e.g., postal code lists (*yousheng bianma*), but these lists are not divided by sector, and sector divisions were necessary due to the stratified nature of my sampling. The size of the overall universe of organizations reported in the *Directory* is compatible with the count of Shanghai municipal enterprises reported in the *Statistical Yearbook of Shanghai* (SMSB 1994, p. 139), which is 8,948. Aldrich et al. (1988) point out that this type of source is likely to underrepresent new organizations, an issue that is likely to be exaggerated in my study, given that the most recent version of the *Directory* was published in 1993.

<sup>13</sup> The criterion of production orientation turned out to be an important one with respect to the *Directory* because the lists also included research institutes and organizations that would fall under the rubric of commerce for the given sector. While both of these types of organization would fit into the broad sense of “organizational field”

## Variables

*Dependent variable.*—The dependent variable for this study is the adoption of the diversification strategy by firms in China's emerging markets. A number of organizational strategies and practices were surveyed for each firm: the adoption of service sector diversification emerged as a strategy that is intimately tied to an organization's success and stability in the emerging markets of China's economic transition.<sup>14</sup> Managers when

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as authors such as DiMaggio and Powell (1983) define it, they are fundamentally different from the other industrial production organizations in the sectors on critical organizational criteria such as fixed assets, turnover, profits, etc. This criterion was determined prior to contact, as the lists of products and services for each organization are given in the *Directory*; the reported size of the universes was adjusted appropriately. However, the determination of the size criterion (i.e., at least 50 employees) could only be made after first contact, and, since only a sample of organizations in the universe was contacted, the universe sizes may also include some small-scale organizations. This problem is minor, though, because in all of the organizations contacted for the study, I only encountered three organizations (4%) with fewer than 50 employees.

<sup>14</sup> Study of the adoption of this specific diversification strategy is of interest not only because of the rapid expansion of this sector or because of the ways in which managers view this practice, but it is also of interest because it is tied to changes in China's economic transition more generally. The expansion of the service sector economy, e.g., is tied to the enigmatic findings of earnings inequality reported in such studies as Xie and Hannum (1996), namely the lower returns to human capital in rapidly growing urban areas. While Xie and Hannum's interpretation that these results indicate the lack of a true labor market in urban China seems plausible, it does not fit with the reality of job allocation in urban China. The emergence of the "talent market" (*rencai shichang*) service organizations—private organizations that serve the purpose of finding individuals' jobs—which are cropping up in large numbers throughout urban China, and the labor bureaus' decreasing participation in job allocation indicate that labor markets are in fact emerging in urban China. In the sample of firms upon which this study is based, over half of the firms do not receive workers from the labor bureaus any longer; managers from those firms reported that they rely on the "talent market" services, putting ads in newspapers, or sending scouts to universities to find talented workers. Further, many of the managers in this sample of firms reported difficulties keeping workers because talented workers would often get offers from other organizations. Fitting with the themes of this study, a significant factor that likely underlies Xie and Hannum's findings is not the absence of labor markets but rather the rapid expansion of the service sector economy in urban areas. The most educated students—those with higher education degrees in engineering or industrial business management—are often siphoned off into large industrial firms; these are still the high prestige jobs in urban China (Lin and Bian 1991). As the majority of these industrial firms are experiencing some form of financial difficulty, at least in terms of cash flow, they offer their workers additional remuneration through noncash benefits such as housing and worker vacations (Walder 1992a). However, they also pay their workers considerably less (in terms of salary and bonus) than many other organizations in the economic transition. The contrast is most striking with organizations in the service sector. The workers in the service sector often have low educational attainment (two years of college or less seems to be the norm); they work for organizations that give no noncash benefits, but these organizations pay sometimes two and three times as much (salary

interviewed often spoke of this strategy in relation to issues of market prominence and economic success in their home sector. One manager in Shanghai's electronics sector explained it in the following way: "Things have been difficult for this factory. . . . [Service sector diversification] is the only way we are making any money. Investments in this sector are great for us because we can make a quick return at a high rate on any investment. We don't need to wait for a product to be produced and then for the market to decide if it is going to sell or not. We just make money through our investments in the service sector. As soon as we invest, we can start making money" (translation of this and other quotes from the survey are mine). Another manager, also in the electronics sector, offered another view of this strategy when interviewed: "In Chinese business we have a saying: 'every sector is like a mountain' [*gehang ru geshan*]. The meaning is that just because we've made it to the top of this sector, doesn't necessarily mean we'll be successful in another sector. That would be like climbing an entirely different mountain. . . . You won't see our company running a restaurant or a hotel or a store. We do well in the electronics sector, and we don't see a need to do anything else."

While these managers' views on the adoption of the diversification strategy are interesting, a more systematic exploration of this practice will show the net effects of exogenous factors associated with this organizational practice. It will be important to keep in mind throughout the analysis that, inasmuch as this economic practice is a short-run investment strategy, I view this type of diversification as an indicator of a firm's desire to create a stable world; firms that are adopting this strategy are experiencing some degree of uncertainty in the emerging Chinese markets. In the analysis, we should therefore expect to see variables such as poor economic performance associated with the adoption of this strategy.

I view the general model of adopting the service sector diversification strategy as

$$\log[d/(1 - d)] = \Omega_0 + \Omega_1$$

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and bonus) as industrial organizations. While no systematic research has been conducted on service sector organizations, during the period for which the research for this study was conducted, I never encountered a service sector employee that was making less than 15,000 *yuan* annually. This is in contrast to the average annual income among the organizations in my sample that was 9,930 *yuan*. This sector is expanding most rapidly in urban areas, precisely where Xie and Hannum find lower returns to human capital. The point is that service sector diversification and the growth of the service sector economy more generally is an important area of research not only in terms of the strategies that firms are adopting in China's economic transition but also in the ways the expansion of this sector can influence labor markets and returns to education.

where  $d$  is a dichotomous variable representing the decision (yes/no) to adopt the diversification strategy,  $\Omega_i$  is a vector representing structural and economic organizational characteristics, and  $\Omega_i$  is a vector representing the governance environment in which the organization is situated. From this general model, I estimate four specific logistic regression models for the organizational-level observations that predict the likelihood that the  $i$ th organization will adopt the diversification strategy,  $d_i$ .

*Structural situation of the firm.*—The specific models include the following variables. Those controlling for sector are included in the equations as chemicals, electronics, and foods; garments is the omitted category for comparison of sectoral location. Organizational size is measured as the natural log of the number of active workers employed by the organization. The term is logged because I expect that the effect of size increases at a diminishing rate. A composite measure of overall organizational health is calculated based on turnover, salary of the workers, and the constant rate that organizations are required to pay into the new official pension fund for retired workers (25.5% of overall salary budget).<sup>15</sup> This variable gauges the organization's health in terms of the overall turnover (gross income) and costs (money covering labor costs). If an organization is doing well in terms of sales *in relation to its labor burden*, the value of this variable will be large; if an organization is in the red, or making little money relative to labor costs, the value for this variable will be negative or small. If diversification is indeed a strategy that unstable or uncertain organiza-

<sup>15</sup> The equation for calculating organizational health is  $O_i = T_i - (S_i W_i + .255 S_i W_i)$ , where  $T_i$  = turnover (gross revenues) for the  $i$ th organization;  $S_i$  = average salary of workers in the  $i$ th organization; and  $W_i$  = the number of active workers. The final term  $(.255 S_i W_i)$  represents the amount every organization must pay into the new pension fund system (*yanglao jin*). The pension fund was set up to level the playing field between organizations that have large retired employee populations and those that have few or no retired employees. It could be argued that this term should be logged because it may be an effect that increases at a diminishing rate. However, logging the value was impossible because some of the organizations are operating at deficits, giving  $O_i$  a negative value that is undefined for the natural log function. We could assign the negative values a small positive value and log the term from there, but I think this would be unwise theoretically, as many of the important cases are the ones that are running deficits. While this measure is a better overall measure than the employee ratio figure, it may be problematic because the measure is incomplete: ideally this equation should include measures for the cost of medical insurance for both active and retired employees—a factor that has not yet been shifted to a general social system. The full equation should be  $O_i = T_i - [(S_i W_i + .255 S_i W_i) + (M_i W_{ri} + M_i W_{ri})]$ , where  $M_i$  = average cost of medical insurance for workers and  $W_{ri}$  = the number of retired workers. However, my data on average cost of medical insurance were too sparse, and I had low confidence in the data that were provided by factory managers for this parameter for it to be included in this variable. (Many managers could only offer guesses as the organizations often did not keep accurate figures on this.)

tions are adopting, organizational health should be negatively associated with the adoption of the diversification strategy (i.e., healthy organizations will be less likely to adopt the strategy). The employee ratio of the organization is another measure of the burdens that organizations have been faced with; the measure is the ratio of active employees to retired employees (all of which the organization has been responsible for paying salaries in the years prior to the implementation of the new pension system). A low value indicates that an organization has to pay a significant amount of wages to nonproductive (retired) employees while a high value indicates that the organization's burden with respect to retired workers is relatively small.<sup>16</sup> The economic strength of an organization is likely to have an impact on decisions to branch out into other markets, and I have included several variables to capture this effect. The profit margin of an organization is based on the 1994 profits relative to the number of employees in the organization; this term thus observes profits as a function of the size

<sup>16</sup> While the organizational health variable may be the better overall measure of wage burdens than the employee ratio measure, the problem with that measure is that the pension system is a completely new system. Since we are dealing with the period of the past five years (the time when organizations started diversifying), the employee ratio may tell us more about the burdens an organization has experienced during the period that is most salient for the dependent variable. It is believed by many managers in the Chinese industrial economy that the burden of retired workers is the largest problem that industrial organizations face. As one manager put it, "The biggest problem our state-owned companies have is the retired workers. We are taking care of so many people in comparison to other private companies. We can't compete with them, in terms of development. They take all of their profits and put them back into the company; we take all our profits and use them to take care of workers who are no longer working here. And many of these retired people are now working at other companies, but they still come here every month to get their pay." For organizations that had no retired employees, they were assigned a value of one retired employee to avoid undefined terms, for these organizations, their employee ratio is equal to their size. The value was then logged to pull in the outliers. Including both organizational health and employee ratio in the same equation was not problematic because the two are not significantly correlated ( $r = .059$ , with  $O_{\text{org}}$  and  $r = .015$ , with  $\ln(O_{\text{org}})$ ). Substituting  $O_{\text{org}}$  for  $\ln(O_{\text{org}})$  does not alter the equations, but the transformed variable ( $\ln(O_{\text{org}})$ ) is significantly correlated with diversification ( $r = .249$ ;  $P < .05$ ), so it was the one included in the models. Both the employee ratio variable and the organizational health variable are important for getting at the demographics of the firm. Currently there is an oversupply of urban labor, and this problem is exacerbated by the dissolution of the job replacement system. Under that system, workers could be convinced to take early retirement on the grounds that their children would get their jobs. With the dissolution of that system, workers are not retiring as early as they once were, and they are holding onto their retiree benefits longer than they did in the past. These variables that observe the relationships among organizational demographics, diversification, and gross revenue (in the case of organizational health) thus may also implicitly test for changes of urban employment policies.



of the organization.<sup>17</sup> Two loss terms were included in the equations as dummy variables: "loser," indicating whether or not an organization lost money in 1994, and "loss trend," indicating whether or not the real profits of the firm declined in 1994 relative to 1990 (both loss variables coded "1" if yes).<sup>18</sup>

*Governance environment of the firm.*—Finally, it may be the case that the governance structures and institutional environments in which firms are situated are critical for the economic decisions they make. In China's planned economy, firms were organized into a "nested hierarchy" in which different levels of government controlled firms at different levels of this hierarchy. Figure 2 shows an ideal typical view of this nested hierarchy of governing organizations and firms.<sup>19</sup>

Walder (1992a) showed that the governing environments at different levels of this administrative hierarchy had a significant impact on the practices adopted by organizations under the redistributive system. Organizations that were at higher levels of the administrative hierarchy had more access to slack resources and funds from state coffers than organizations at lower levels of the hierarchy. Walder's (1995) more recent predic-

<sup>17</sup> Other profit margin measures were observed in the equations including (profits/turnover) and (profits/fixed assets). Neither of these measures was significant and neither changed the overall results of the models.

<sup>18</sup> For both loss variables and the organizational health variable, data from 1990 would be better than data from 1994. I use the latter because there is a significant number of cases with missing data for 1990. Data from 1994 could be questioned in terms of the causal direction assumed here (i.e., losses do not cause diversification, but rather, diversification causes losses). However my confidence in the direction of causality (i.e., that losses and poor organizational health lead to diversification) is raised by the fact that several managers asserted this line of reasoning—that they decided to diversify into the service sector because they were doing poorly economically. The substitution of losses in 1994 for losses in 1990 does not present a big problem as the two variables are highly correlated ( $r = .820$ ;  $P < .01$ ). In addition, model 2 addresses the causal direction concern by showing the results based upon the 1990 data.

<sup>19</sup> Fig. 2 is based on interviews with 56 industrial managers and directors and 18 governmental officials (see Guthrie [1996] and Walder [1992] for basic discussion of the "nested hierarchy" of industrial structure). In municipal areas, there are two additional levels in the hierarchy of control over factories: street associations, which are a subdivision of the district government, and in the garments sector, some larger factories (usually textile factories) also have control over smaller factories. Most economic organizations under the street associations are in the service sector, so they were not really relevant for the sampling conducted in this study. It should be noted that fig. 2 is an ideal typical representation of the organizational structure of industry in China and that there is considerable variation from bureau to bureau and company to company (e.g., there are 110 factories directly under the jurisdiction of the Electronics Bureau, while there are over 400 factories under the jurisdiction of the Light Industry Bureau). The numbers presented in fig. 2 are ranges derived from the firms included in my sample.

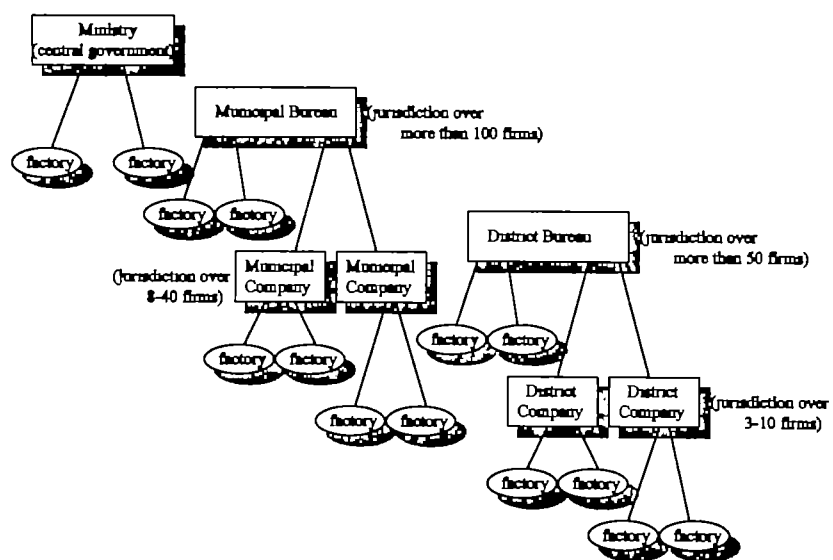


FIG. 2.—Organizational structure of governmental administrative rank in China.

tion that firms at higher levels of this hierarchy will continue to operate under soft budget constraints (i.e., continue to have access to funds from state coffers) also makes governing environment central to organizational practices. If Walder's prediction is correct—that firms at upper levels of the administrative hierarchy are still operating under soft budget constraints and are therefore still very much protected by the state—then we should expect that firms at the upper levels of the hierarchy will be less likely to adopt strategies that indicate uncertainty in the market.

To observe the effects of a firm's governing environment, I include a variable indicating at which level of this administrative hierarchy a firm is located. I do not observe the effect of this variable as an ordinal variable (as Walder [1992a] did), rather, I observe the effect of different administrative levels by including them as dummy variables and comparing them to the other categories that are left out of the equation. The terms are included in the equations as dummy variables for governance by municipal bureau and municipal company; these are compared to district company governance, the omitted category for the final equation.<sup>20</sup>

The sample means, standard deviations, and a brief definition of the variables are presented in table 1. A few of the variables can be compared

<sup>20</sup> These governmental categories are levels 5, 3, and 2 (6 is the highest) in Walder's (1992a) ordinal categorization of governmental administrative levels (he called it "budgetary rank"). Each of these governmental levels is located in the urban sector,

with those of official statistics and other samples. In terms of property rights, the 1994 official statistics for Shanghai report that state-owned enterprises comprise 31.2% of industry, collective enterprises make up 49.4% of industry, and "other kinds" of organizations make up 19.4% of municipal industry (SMSB 1994, p. 133). These numbers diverge from the representations in my sample, which are 58.0%, 33.3%, and 7.4% respectively. This difference is primarily due to the fact that *Statistical Yearbook of Shanghai* municipality data are aggregate data of the 15 districts of Shanghai, which include the six county districts, while my data are only based on the nine city districts. Because the majority of township and village enterprises are located in the counties and the majority of these enterprises are collectively owned, it makes sense that my sample is composed of a smaller proportion of collectives relative to the overall municipality. At 1,581, the mean for data on firm size is higher than the mean for workplace size in a 1986 Tianjin sample where the average workplace size was 1,337 (see Walder et al. 1989, p. 20). This is not surprising, given that my study selected on registered large-scale organizations, which is what constitutes the sector lists of the *Chinese Directory of Organizations and Institutions*. The three smaller organizations (less than 50 employees) that were contacted were rejected on grounds that they might present problems in a comparative sense (see also Edelman 1990, 1992; Sutton et al. 1994), and no small-scale entrepreneurial organizations (*getihu*) or street association factories appeared in my sample. In the Tianjin survey, 6.1% of the individuals' organizations were under municipal bureaus, 44.1% were under municipal companies, and 10.6% were under district companies (see Walder et al. 1989, p. 17) compared to 26.9%, 26.9%, and 32.1% respectively for my sample. The likely reason for this difference is the purposeful inclusion of two heavy industries in my sample (see appendix), which would account for the relatively high representation of organizations under the municipal bureau.<sup>21</sup> One positive aspect of the sample

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so in a sense, the focus of this study is on organizations at the upper levels of China's administrative hierarchy (compared to townships and villages). However, there is also a strict comparison among firms at three different levels of China's administrative hierarchy (see fig. 2 for reference): firms under municipal bureaus that are directly under the city government (30% of the sample), firms under municipal administrative companies that are an administrative level below city bureaus (30% of the sample), and firms under district companies that are an administrative level below district governments (32% of the sample).

<sup>21</sup> It is also likely that this difference in the samples is related to the differences in mean workplace size because, as Walder (1992a, p. 531, n.12) points out, workplace size is largest for organizations under municipal bureaus in the Tianjin survey. Since the Tianjin survey contains a significantly smaller sampling of organizations under municipal bureaus, it is not surprising that the mean workplace size in that survey would be smaller than that in my sample.

TABLE 1  
CHARACTERISTICS OF ORGANIZATIONS FROM FOUR INDUSTRIAL SECTORS IN SHANGHAI, 1995

	Mean	SD	Definition
Diversification ...	395	492	1 = investment in the service sector economy, 0 = no investment
Active employees*	1,580,840	3,724,745	No. of active (not retired) employees, year-end, 1994
Active employees (log)	6,058	1,475	Natural log function of active employees
Employee ratio†	38,346	131,797	No. of active employees per retired employee
Employee ratio (log)	1,691	1,669	Natural log function of employee ratio
Average salary	8,996,914	2,877,991	Average annual salary (including bonuses) in <i>yuan</i> ; <i>yuan</i> -US\$ exchange is 8:1
Organizational health (in millions)‡	183,464	500,550	Gross income in <i>yuan</i> - labor cost - money paid into national pension fund; low values indicate organizations struggling to meet labor costs
Profits (in millions)	20,554	113,078	Profits for year-end, 1994, <i>yuan</i>
Profit margin ....	070	.085	Profits divided by turnover
Losers§	259	441	Organizations that lost money in 1994; dummy, 1 = loser
Loss trend .....	.457	.501	Organizations with declining profits, i.e., [ $P('94 - '90) < 0$ ]; dummy, 1 = loss trend

Turnover (In millions) . . . . .	201.927	542.653	Gross income, year-end, 1994, <i>ymoz</i>
Fixed assets (In millions) . . . . .	147.919	498.331	Fixed assets, estimated at 1994 values
Municipal bureau . . . . .	.296	.459	Municipal bureau governance; dummy, 1 = organizations under municipal bureau jurisdiction, highest governmental level in data set
Municipal company . . . . .	.296	.459	Municipal company governance, dummy, 1 = organization under municipal company jurisdiction
District company . . . . .	.321	.470	District company governance, dummy, 1 = organization under district company jurisdiction, lowest governmental level in data set
Jurisdiction size . . . . .	46.963	72.257	No. of organizations under the <i>i</i> th organization's governing office
Jurisdiction size (log) . . . . .	5.990	2.789	Natural log of jurisdiction size

NOTE.—See text for discussion of the variables and data collection. In terms of the property rights, the sample is comprised of 47 state-owned (*gosyovet*) factories, 27 collectively owned (*jits swoyovet*), six joint venture (*hess qbye*) factories, and one independent factory (*dali gosyovet*). Five of the state-owned factories are public share companies (*refen yovetov gosyovet*).

\* For equations, this variable is logged as I expect that its effect increases at a decreasing rate

† Calculated from the no. of active and retired employees. For firms with no retired employees, a value of "1" was assigned to retired workers to avoid undefined values for the variable. The value in this category is influenced by outliers, elimination of the five factories that had large active employee populations but no retired employees brings the average value down to 11.881 active workers for every retired worker. The term is logged to pull in the outliers.

‡ See n. 15 for discussion of this variable

§ "Lower" is based on 1994 profit figures. Loss trend is based on  $(P_{94} - P_{90}) < 0$ , where  $P$  denotes the profit for that year. The loss trend variable then is more accurately a measure of declining profits than overall losses. Those two loss variables are correlated at .645,  $P < .01$ . The latter term is a lagged variable and incorporates a time trend that reveals declining profits. For the lower ('94) variable, although the coding is based on 1994 figures, it is a more accurate loss measure (as opposed to declining profits), but it is not lagged. However, this problem is attenuated because the majority of organizations in sample (82%, 18/22) that lost money in 1994 also had losing trends over the period 1990–94.

distributions across the governmental categories is that there are relatively even representations of organizations under these three levels of administration; this will allow us to observe most clearly the variations of governance characteristics and their effects on firm practices.

### Findings

Table 2 presents the results of multivariate logistic models predicting the likelihood that the  $i$ th organization will adopt the service sector diversification strategy. In model 1 there are three statistically significant effects. Organizational size has a significant positive association with the decision to diversify into the service sector: larger firms are more likely to adopt the diversification strategy. A firm's overall "organizational health," as it is measured here, has a significantly negative effect on the adoption of this strategy. The more healthy an organization is in terms of its turnover-labor cost relationship, the less likely that organization is to diversify; the more burdened an organization is, the more likely it will be that the firm will adopt the diversification strategy. The loser variable is also significantly associated with the adoption of the diversification strategy. These latter two effects have one important thing in common: they are both indicators of economically weak firms, and both indicators apparently increase the likelihood that weak firms will adopt this short-run investment strategy. Both of these effects lend support to the notion that organizations in tenuous economic situations adopt a strategy that allows them to seek stability in other markets. These effects also support my general interpretation of this short-run strategy as a practice that indicates uncertainty in the economic transition. If diversification was a strategy that was associated with profit maximization, we would expect to see economically stable firms also adopting the practice as part of an overall development strategy. But as the results above show, it is the weakest firms, the ones in the most tenuous positions economically, that are turning to diversification.

Questions may be raised, however, about the direction of causality among these associations given the data employed in this study. Indeed, a central problem with the data is that, as all of the variables included in model 1 are based on 1994 figures, we have no way of discovering the sequencing of events: Did firms adopt the diversification strategy because they were economically weak or did they become economically weak because they diversified? If the latter is true, my assertion that service sector diversification is a strategy adopted by firms experiencing uncertainty in China's markets would appear much more tenuous.

Model 2 addresses these concerns. While time series data were not available in the interviews, I was able to gather data for 1990 for a majority

TABLE 2

LOGISTIC COEFFICIENTS FOR THE ADOPTION OF DIVERSIFICATION STRATEGY BY  
ORGANIZATIONS IN FOUR INDUSTRIAL SECTORS, SHANGHAI, 1995

Independent Variable	Model 1	Model 2	Model 3	Model 4
Structural and economic variable: <sup>a</sup>				
Chemicals . . . . .	.016 (1.180)	.004 (1.142)	.012 (1.210)	.034 (1.184)
Electronics . . . . .	1.150 (.870)	1.142 (1.000)	.372 (1.022)	.200 (1.046)
Foods . . . . .	.614 (.652)	.680 (.876)	.802 (.768)	.404 (.798)
Active employees (log) . . . . .	1.073*** (.365)	1.457*** (.438)	.705* (.394)	.487 (.426)
Employee ratio (log) . . . . .	-1.104 (.819)	-1.278 (.915)	-.867 (.918)	-.766 (.955)
Profits/size . . . . .	.003 (.003)	.001 (.003)	.004 (.003)	.004 (.003)
Organizational health 1994 . . . . .	-.381** (.185)	—	-.440** (.202)	-.419** (.202)
Losses 1994 . . . . .	1.456* (.866)	—	1.574* (.988)	1.592* (1.020)
Loss trend . . . . .	-.300 (.402)	—	-1.048 (.940)	-1.124 (.956)
Organizational health 1990 . . . . .	—	-1.434** (.694)	—	—
Losses 1990 . . . . .	—	.190 (.382)	—	—
Governance variable: <sup>a</sup>				
Municipal bureau . . . . .	—	—	2.632*** (.948)	3.546*** (1.162)
Municipal company . . . . .	—	—	—	1.060 (.774)
Constant . . . . .	-5.513** (2.410)	-7.608*** (2.715)	-2.848 (2.687)	-1.280 (2.931)
$\chi^2$ . . . . .	26.524***	28.669***	35.878***	37.795***
df . . . . .	9	8	10	11
N . . . . .	80	71	80	80

NOTE.—Nos. in parentheses are SEs. Model 2 includes 1990 data for variables of organizational health and losses. As there are six organizations in the sample that are joint ventures, models were also run excluding these six organizations; there were no significant differences in these models.

<sup>a</sup> Garments is the omitted sector category for industrial sector.

<sup>b</sup> Statistically significant at the  $P < .1$  level for one-tailed tests.

<sup>c</sup> District company is the omitted category for governance variables.

\*  $P < .1$  (two-tailed test).

\*\*  $P < .05$  (two-tailed test).

\*\*\*  $P < .01$  (two-tailed test).

of the firms (71 out of 81). The year 1990 was prior to the time that virtually all of the firms had adopted the service sector diversification strategy.<sup>22</sup> If diversification is indeed a strategy that economically weak firms are adopting—as opposed to one that is making firms economically weak—we should see results that are similar to those presented in model 1. As model 2 shows, the results for the 1990 associations are very similar to those of 1994: firms that were in poor economic health in 1990 were more likely to adopt the diversification strategy over the period of the next four years.

Model 3, which introduces the effects of the administrative level of a firm's governing organization, reveals that organizational health still has a significant negative association with the dependent variable, net of all other effects, indicating that poor economic health increases the likelihood that a firm will adopt the diversification strategy. There is also a significantly positive effect of the bureau governance variable: location under a municipal bureau increases an organization's likelihood of adopting the diversification strategy, compared to location under administrative company governance (the omitted category for the equation). While it has already been argued, based on significantly higher gains in output and productivity at lower levels of the administrative hierarchy, that budget constraints are being hardened at lower levels, the findings presented here indicate that budget constraints are *also* being hardened at upper levels of the administrative hierarchy, despite the fact that Kornai's (1990) organizational assumptions hold at those levels. These findings indicate that large-scale organizations under municipal bureaus also are experiencing tightening fiscal constraints and are subsequently seeking profits through outside investments. If the situation were otherwise, if budget constraints were still remaining soft in the upper levels of the administrative hierarchy, what would be the incentive for organizations at these levels to seek profits and spread out risk through diversification? The results of model 4 show that there is not a significant difference between municipal company governance and district company governance for the adoption of the service sector diversification strategy.

In sum, two organizational situations are associated with diversification: poor economic health and location at upper levels of China's administrative hierarchy. These effects are independent of each other, indicating

<sup>22</sup> The question, "When did your organization first invest in the service sector?" also appeared on the questionnaire. While managers were often vague about the exact date that the firm adopted the strategy, virtually all were able to affirm that they had adopted the strategy within the last five years (i.e., since 1990).



that there are two distinct types of organizational situations that give rise to diversification in the transitional Chinese economy.

## DISCUSSION

### Uncertainty in Chinese Markets

The results presented in table 2 indicate that there are two types of uncertainty in the nascent markets of China's economic transition: economic and administrative. Economic uncertainty is fairly straightforward: organizations that are struggling economically, especially those that have weighty labor burdens and are not making enough money to comfortably cover labor costs, are significantly at risk to seek stability by investing assets in the quick return markets of China's rapidly expanding service sector economy. The evidence of economic uncertainty in Chinese markets indicates that significant changes are occurring in the urban industrial sector in the economic transition. The fact that productivity has not risen as rapidly in urban industrial areas as it has in rural areas has been viewed as evidence that reforms are not being implemented in these areas of China; slow gains in productivity have been taken as evidence that firms in China's urban industrial sector are still supported by state funds and soft budget constraints (Walder 1995). Yet weak firms in this sector of the economy are seeking alternative paths to economic stability. If reforms were not being enacted, if firms were still supported by state bailouts, what would be the incentive for an organization to spread risk and invest in quick return markets? A look at the strategies and practices of firms in China's transitional economy indicates that, in fact, market reforms are taking hold in this part of the economy; increasingly, firms are being set adrift, and they can no longer count on the state for bailouts in situations of economic hardship. The implication of this finding is that, although aggregate measures of productivity in the upper levels of China's administrative hierarchy do not reflect the reforms, firms are starting to act under marketlike constraints. Economically weak firms face the most uncertain futures, and they are starting to adopt the survival strategies of spreading out risk and seeking fast returns in alternative markets.

Though related to economic uncertainty, administrative uncertainty is somewhat more complicated. As with economic uncertainty, its driving force is the enactment of reforms. But the reforms have had a different impact on firms at different levels of this administrative hierarchy. An examination of the mechanisms behind administrative uncertainty requires an exploration of the relationships among individual firms, the responsibilities of governing offices, and how these responsibilities and the

constraints that governing offices are subject to are changing in the economic transition.

### Governmental Responsibilities

*Tightening fiscal constraints for administrative offices.*—What are the mechanisms behind the administrative uncertainty experienced by firms under municipal bureaus in China's transitional economy? Location under municipal bureaus significantly increases a firm's likelihood of diversifying, while district and municipal company governance decrease the likelihood of diversification; there are two principle reasons why governance at different administrative levels matters so much for diversification.<sup>23</sup> First, while we know that the number and scale of enterprises increase with higher administrative levels (see Walder 1995), I emphasize the fact that, in the economic transition, economic responsibilities grow with administrative rank precisely because of the larger scale and scope of sectors under higher-level administrative offices. In China today, as the project of market transition continues and accounting, taxation (state revenue extraction), and legal institutional systems become codified and solidified, the revenue bases of governmental organizations are constricted. Under the planned economy, governing organizations simply extracted revenues from the organizations that were under their jurisdictions (Walder 1992a). In the economic transition, as a result of the Profits to Tax Reform (*li gai shui*), firms are required to pay taxes to the government, and they are no longer required to turn profits over to their governing organization. Many governing organizations still collect an administrative fee (*guanli fei*), and some do still collect a portion of firms' profits. However, if the core of the redistributive system lies in the fact that firms turned revenues over to their governing organizations, and this money was "redistributed" to firms in the jurisdiction, the fundamental transformation of that system has been set in motion over the course of the economic reforms.<sup>24</sup>

<sup>23</sup> It is not the case that simple differences in laws or rules define the differences in rates of adopting this strategy for different levels of the hierarchy. If the adoption of this strategy was simply a function of variant legal rules, we would find that no firms under a given administrative level (e.g., district company) were adopting the strategy. This is not the case. While firms under municipal bureaus are the most likely to adopt the strategy, there are firms at each of the lower levels that have adopted the strategy.

<sup>24</sup> Other important policy reforms that have been integral to the changes surrounding government-enterprise relationships are the Self-Responsibility for Gains and Losses Reform (*si fu ying kui*) and the reinvigoration of the independent accounting system (*duli hesuan*). While extensive time and space could be devoted to the particularities and challenges of implementing these policy changes, such a project is beyond the scope of this article. None of the changes are legal in nature (though they have become intertwined with subsequent laws), they are based on administrative policy ideas that have been tested and eventually promulgated on a wide scale over the course of the

The result of these policy changes is that governmental organizations cannot continue to cover the costs for all of the firms under their jurisdictions. With less money coming in from the organizations under their jurisdictions and a finite amount of money coming from city and central governments, governing organizations have a finite amount of fiscal resources to draw from. And the more firms these governing organizations have to watch over—with potentially more firms to bail out—the more constrained fiscal budgets of governing organizations become in the economic transition. This reality increases as the overall responsibilities of governmental administrative organizations increase. For governing organizations that have jurisdiction over many firms (e.g., between 100 and 400 for municipal bureaus; see fig. 2), which in the past, relied on the revenues from many firms for their overall budgets, the change has been extreme. These organizations no longer have access to the revenues of a large group of firms, and thus their access to excess fiscal resources has been reduced significantly. For governing organizations that have jurisdiction over a few firms (e.g., between three and 10 for district companies; see fig. 2), tightening fiscal constraints have much less of an impact. In the planned

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economic transition; many of these types of changes for the urban industrial economy were promoted heavily by Zhao Ziyang in 1984 (see Naughton 1995, chap. 5). But at no point has there been an official (legal) requirement that the organizations adopt these changes. Thus there is considerable variation as to when and to what extent the changes have been adopted by organizations. Naturally, the Self Responsibility policy had virtually no meaning for organizations at the beginning of the reforms. When and how the policy has come to have meaning for firms in practice is an empirical question of the economic transition. Because these policy changes begin as internal communiqués, the actual moments of adoption and change at the organizational level are somewhat difficult to trace. Several of the important early documents that set the stage for the independent budget policy were issued by the 1979 State Council; among these documents were "Some Regulations on Enlarging the Management and Decision-Making Powers of State-Owned Industrial Enterprises," "Regulations on the Retention of a Portion of Profits by State Enterprises," and "Provincial Regulations on Instituting Complete Reliance on Bank Credit for Working Capital in State-Owned Industrial Enterprises" (Field 1984, 1992). One internal (i.e., not public [*neibu*]) document that describes these changes in a broader, detailed fashion for the city of Shanghai is "The Practical Ways to Change the Management Institutions of State-Owned Enterprises for the City of Shanghai" (*Shanghai quanmin suoyoushi gongye qiye zhuanhuan jingying jishi shishi banfa*), which can be found in the internal reference book, *Shanghai Economic Institutional Change Yearbook, 1989–1993* (Shanghai Reform Collections Office 1994). Since 1979, the number of organizations on independent budgets has risen steadily from 5,866 to 10,254 in 1994 in the city of Shanghai (these data include counties; the data on independent budgets in the nine city districts of Shanghai are not available). For medium- and large-scale industrial organizations (mostly concentrated in city districts), the numbers have remained under 1,000 (see SMSB 1990, pp. 127–90; SMSB 1994, pp. 140–68). As quantitative analysis shows, it is not only important to note the growth of this category but also the extent to which it has become meaningful in the economic transition.

economy, these governing organizations only drew their budgets from a small group of firms, and to some extent, their budgets were constrained under the planned economy (they were also turning the majority of their revenues over to the bureaus). In addition, these organizations simply have fewer firms to guide through the economic transition and potentially fewer organizations to bail out in the case of financial difficulties.

The second reason for the adoption of a diversification strategy by upper-level firms is an outgrowth of the first. Whereas bureaus controlled sectors in the planned economy, in the period of economic reform, administrative and economic responsibilities are being pushed down the hierarchy to administrative companies and to firms themselves. The lone factories under bureaus have the most uncertainty in this newly marketized world because they are left alone to succeed in the market or to make their case for funds from state coffers (slack resources) to the bureaus alongside all of the other organizations in the same position. Those under administrative companies, however, are protected from the do-or-die market system, and they are also monitored by a governing organization much more closely than the isolated factories under the bureaus. If it becomes clear that a factory underneath an administrative company is heading for a shortfall, the bureaucrats of the district or municipal company can afford the time and attention to help the organization reorganize and restructure. Further, administrative companies can offer slack resources because, although their overall budgets are substantially smaller than those of bureaus, they also have significantly fewer responsibilities in terms of the number of organizations to watch over and guide through the economic reform. They can offer bailouts, longer run development strategies, and general attention that cannot be offered by the bureaus, which are now very selective about which firms they protect in the economic transition. The overall effect is that budget constraints *are* being hardened in the urban industrial economy, but this is a process that is mediated by the economic responsibilities that governmental offices incur. The larger the economic responsibilities of the governing organization, the greater the extent to which firms are left to fend for themselves in the market economy.

If it is true that the results above can be explained as a function of increasing governmental responsibility, the issue may be more than just a qualitative difference between bureau and administrative companies; as firms compete for resources (administrative and fiscal), the issue may be linked to the number of firms under a governing organization's jurisdiction. We should be able to see similar results to those presented in table 2 as a function of the number of organizations a given factory's governmental organization has jurisdiction over. That is, if governmental responsibility is the real story, then the number of organizations that a

TABLE 3  
LOGISTIC COEFFICIENTS FOR THE ADOPTION OF DIVERSIFICATION  
STRATEGY. ESTIMATING THE EFFECT OF SIZE OF GOVERNING  
JURISDICTION

Independent Variable	b	SE
Structural and economic variable: <sup>a</sup>		
Chemicals .....	.612	1.300
Electronics .....	.614	1.064
Foods .....	1.384	.922
Active employees (log) .....	.346	.424
Employee ratio (log) .....	-.833	.953
Organizational health .....	-.486**	.231
Profits/size .....	.004	.004
Loser '94 .....	1.606	1.124
Loss trend .....	1.186	1.044
Governance variable:		
Jurisdiction size (log) .....	1.455***	.432
Constant .....	-5.375*	2.807
$\chi^2$ .....	46.749***	
df .....	10	
N .....	80	

<sup>a</sup> Garments is the omitted category for industrial sector.

\*  $P < .1$  (two-tailed test).

\*\*  $P < .05$  (two-tailed test).

\*\*\*  $P < .01$  (two-tailed test).

firm's governmental office has jurisdiction over should have a positive influence on whether or not a firm in that organizational field chooses to diversify. Larger jurisdictions mean more firms competing for resources and therefore more uncertainty in that organizational field. To test this issue, data were also gathered on the total number of organizations each firm's administrative office presides over. The net effects of this measurement are presented in table 3.

As with the preceding models, organizational health has a negative effect, indicating that organizations in healthy financial situations in terms of sales-labor cost relationship are less likely to diversify. The significantly positive effect of jurisdiction size (the number of organizations a governing office has to oversee) helps us interpret the other models and lends support to the thesis set forth here that governmental responsibility is the critical factor in the availability of slack resources and the subsequent strategies that organizations adopt. Governmental responsibility is positively associated with diversification, meaning that the more factories a given firm's governmental office has to oversee, the more likely it is that the firm will adopt a diversification strategy. As governmental jurisdiction

increases in size, so does the tendency toward searching for external incomes increase. Bureaus have to oversee many factories—usually somewhere between 100 and 400—so they have great administrative responsibilities, especially in the economic transition. Therefore, as budget constraints become hardened for all organizations (governing and economic), the resource pool that they can offer to organizations beneath them becomes more a function of the number of organizations that are competing for resources than it is a function of the gross value of budgetary resources. Organizations beneath bureaus, then, are forced to seek other paths to stability. This explains the positive effect of bureau governance in the preceding models. The situation for district companies is much different. Although their overall budgets may still be smaller, the number of organizations competing for economic and administrative resources is somewhere between two and 10. There is a greater chance that these offices will have slack resources or redistribute resources among firms under their jurisdictions.

*Monitoring, information, and developmental plans.*—Governmental responsibility is not only about fiscal resources. Issues of monitoring and information are also intimately tied to governmental responsibility and the changing structure of the administrative hierarchy in the economic transition. Accounts of monitoring and information have emphasized the monitoring problems that come with “layers” of bureaucracy (see Walder 1995, pp. 288–89). According to this view, where local governments in townships and villages have direct control over the firms under their jurisdictions, the administrative companies that have control over firms in urban areas simply add another layer of bureaucracy between the firm and the municipal bureau and, in effect, become barriers to the upward flow of information. The result is that, while local governments are better able to monitor the few firms under their jurisdictions (because they have direct contact and control over the firms), higher levels of governments in urban areas not only have more firms to monitor, but there are also more levels of bureaucracy to obfuscate and distort the information required for close monitoring and control. Monitoring then is about both the size of the jurisdiction and the layers of bureaucracy through which information is filtered in the monitoring process.

Under the old system and in the early period of the reform, it is true that companies simply added an additional layer to the bureaucracy. But this view requires modification for the current situation in China’s urban industrial economy. In the past, as the heads of sectors, bureaus exercised significant administrative control over their sectors, and the companies simply acted as the governmental “funnels” (*zhengfu loudou*). In the period of economic reform, administrative and economic responsibilities have been pushed down the administrative hierarchy of the former command

economy. Today, companies perform the same functions that were formerly the responsibilities of bureaus. Rather than acting as an extra layer of bureaucracy, the administrative companies act as the direct representatives of the state in guiding the firms under their jurisdictions through the economic reforms. Bureaus also have direct control over some firms, and they are also basically responsible for guiding these factories through the reform. But the disparity between the number of firms under bureaus and companies is significant (see fig. 2), and this has a direct impact on the amount of administrative attention the respective levels are able to offer the firms under their jurisdictions. Therefore, at this stage in the economic transition, the monitoring capacities in urban areas are less about layers of governmental bureaucracies and more about a simple comparison of the size of a given jurisdiction. District and municipal companies are now much more like the local governments that Walder (1995, p. 266) described as having "a greater ability to monitor firms," while bureaus are much less able to offer administrative guidance and attention in the economic reform. But again the capacity is a function of the size of the jurisdiction—the size of the jurisdiction increases the likelihood that a firm will experience uncertainty and adopt short-run survival strategies.

So how does this account fit with the fact that firms under district companies are less likely to diversify than those under bureaus? The answer again comes back to governmental responsibility, but the resource in question with respect to monitoring and information is administrative rather than fiscal. As administrative responsibilities are pushed down the hierarchy of the former command economy, administrative offices with smaller jurisdictions are able to offer direct attention and long-term development plans to the firms beneath them. The ability of district companies to keep track of and attend to the developmental issues of firms under their jurisdictions is heightened by the fact that they have fewer firms to attend to. In my sample, the majority of factories under district companies reported having significant and frequent contact with their governing organization, while those under municipal bureaus most often asserted that the bureaus had not the time or administrative staff to pay attention to their developmental needs. This is especially true today given the extent to which bureau administrative staffs are pared down as bureaus prepare to make the transition to "State-Owned Asset Management Companies" (*guoyou zichan jingying gongsi*). With no more than 10 factories to watch over, administrators in district companies are often able to guide the firms through the economic reform with long-term development and organizational restructuring plans. In the survey, a factory director in the foods sector attributed the rapid development of his factory over the last few years directly to decisions that the factory's district company had made: "In 1990 things really started to develop quickly for our factory. Our dis-

trict company really changed many things and helped us develop a lot. They made us into a limited public shares factory [*gufen youxian gongchang*], formed a board of directors, and we started thinking about how to develop within this sector aggressively [*jijide fashan*]. Since then we have made many changes within our organization that reflect our market orientation. I think, maybe, you will find that our organization is quite different from many that you will see in China." As this manager indicates, the long-term restructuring programs—a product of close monitoring by the governing organization—come directly from the administrative help and attention of the district company. Bureaus, on the other hand, simply do not have the administrative resources to take an active interest in long-term restructuring programs for firms under their jurisdiction in the economic transition. So while fiscal resources are tightened for firms under both levels of government, firms under bureaus experience a greater sense of being set adrift in the market than do those under municipal and district companies. This sense of administrative uncertainty in the market is the reason that firms under bureaus are the most likely to adopt strategies of spreading out risk through investment in fast return markets.

In short, the larger financial and administrative responsibilities of municipal bureaus (relative to municipal and district companies) have two consequences: first, it makes them unable to guarantee bailouts for shortfalls; second, and more important, it makes them less able to help the organizations underneath them pursue aggressive development programs. As the factory director whose firm had just been converted into a public shares factory indicated above, the type of development strategies that district companies are creating for the organizations below them have more to do with fundamental changes in the organizational structure than they do with quick returns. With only a few organizations under their jurisdictions, these companies have the resources and administrative time to focus attention on long-term development plans for the organizations they control. Thus organizations under district companies have less need to seek stability through diversified investment; their worlds are stabilized by the close attention of their governing organizations. Organizations directly under bureaus, on the other hand, do not derive stability from their governing organizations because bureaus do not have the time or resources to help organizations restructure into more successful organizational forms. With considerably more independence, these organizations do what they can to ensure that budgets will be met and create stable worlds for themselves. They seek market stability through spreading themselves out across different sectors and searching for fast-return capital investments.

One additional explanation—one that is linked directly to issues of monitoring and information—must also be considered: corruption. It may



be that actors within organizations that make decisions about diversification investments stand to gain directly from these investments, whether it is from siphoning funds from the investments or getting direct kickbacks from the individuals who are running the ventures. In governmental jurisdictions where it is more difficult to monitor firms, these opportunities may be greater. In larger jurisdictions, there are more firms for a given governmental office to oversee, as I have noted above. Lower monitoring capacities of governing offices may allow corrupt factory directors to pursue profit and personal gain in any way they see fit. One of these avenues may be investments in the rapidly expanding service sector. As fiscal constraints are tightened for firms in China's marketizing economy and organizations expand their scope of activity to cope with these changing constraints, actors that are directing these investment strategies may stand to gain the most by diversifying investments. The adoption of these corrupt economic practices may also be directly linked to the institutional changes and tightening fiscal constraints I described above. Whereas factory managers of large-scale organizations had access to slack resources under the planned economy, those doors are now closing. As a result, under these tightening fiscal constraints, managers may be more likely to pursue opportunities for profit and gain in external investments. In addition, the accounting practices for diversification investments may be scrutinized less closely than the accounting practices relating to an organization's manufacturing activity in its home industrial sector. Managers may find more opportunity to line their pockets with funds siphoned off of their less scrutinized diversification investments.<sup>25</sup> It is important to note here,

<sup>25</sup> The fact that virtually all managers had a difficult time telling me the turnover and profit figures of their diversification investments (separate from overall turnover of the organizations) indicates (1) that the accounting practices for these investments are not separated from those of the company or factory and/or (2) that the accounting practices for keeping track of these investments may be softer than those of the factory or company as a whole. In either case, corrupt managers would have an opportunity to siphon funds from the diversification investment into their own pockets. In general, the corruption issue is an important argument to consider in explanation of economic strategies in China's transition. However, it is also a difficult one to get a handle on empirically. Over the course of the 155 interviews that were conducted for the completion of this study, it is not surprising that no one mentioned corruption in connection with the economic strategy of diversification (though the subject did come up in a few of the interviews, albeit in other contexts). Governmental officials and factory managers alike are interested in portraying China's reforms in a positive light, particularly in the context of the swirl of media attention surrounding intellectual property rights and the possibilities for a rational legal framework to emerge in China, issues which were omnipresent in all of the interviews I conducted. In the interviews conducted for this research (average interview time was one hour, 45 minutes, though some interviews ran over two hours), it was simply impossible to build the confidence that would allow for open discussions about corrupt economic practices (as I was most often interviewing factory directors and managers, these discussions would have

however, that as an explanation for diversification strategies, managers' corrupt economic practices are not mutually exclusive with the explanations described above about tightening fiscal constraints and the experience of economic and administrative uncertainty at the organizational level. What both of these explanations hold in common is that broad institutional changes and tightening fiscal constraints are impelling economic actors (organizations or the individuals running them) to pursue alternative economic strategies for stability in the marketplace. It is likely that both situations are occurring simultaneously.

## CONCLUSIONS

Where studies of economic transition have most often looked to growth and economic vitality as evidence of reforms, the reality is that there are many economic activities that may be revealing in terms of a changing system but not reflected in rapid economic growth. This view follows a more general economic sociology model of action where economic actors strive to create stable worlds (Fligstein 1996b), and the changes in the former redistributive system must be viewed in this light. Under the redistributive system, the economic worlds of firms were inherently stable because the state took care of budgetary and resource allocation for all firms. Gains and losses at the firm level were not a part of the equation. In the economic transition, gains and losses are increasingly becoming a part of the marketized worlds that firms experience. And the extent to which transition is becoming a reality for these firms must be understood not only as a function of the gains that sectors of the economy accrue but also as a function of actions that are much more about survival and stability than they are about economic gain. One strategy that firms have adopted

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been about their own corrupt practices), as it would be with a more ethnographic method. The general story I tell about economic constraints and administrative and economic uncertainty fits well with the data, and it fits well with what the managers and governmental officials themselves told me about this particular economic strategy and about the general changes occurring in China's industrial hierarchy. Yet this interpretation does not exclude the possibility of corruption playing a role in empirical patterns we are observing; indeed it is likely that both causal scenarios exist and are occurring. But it is important to note that these causal scenarios are *not* mutually exclusive: both explanations operate on the premise that organizations (and the factory directors) had virtually unlimited access to funds from state coffers under the planned economy and in the economic transition they do not. In both explanations, budget constraints are being hardened, and in both cases these changing institutional constraints are driving the actions of economic actors (organizations and the individuals that run them) to pursue alternative routes to economic stability.

in the drive to create a stable world is the practice of investment into the low-risk fast-return market of the service sector economy.

The economic strategy of service sector diversification is, by its very nature, part of China's economic transition from the planned economy to a market system. Before the economic reform began in 1978–79, a competitive service sector was virtually nonexistent. Over the last few years, it has become one of the fastest growing sectors of the Chinese economy. Suddenly, in the Chinese economy, there is a sector that offers fast returns on capital investments. Whether or not organizations take advantage of the growth in this sector is a question that has implications for how organizations are making their way through the economic transition. If organizations are investing in this sector, we might view them as adopting a short-run strategy of seeking stability in markets through diversifying into the service sector economy. Firms that are in poor financial shape experience a type of economic uncertainty, and they are likely to adopt this strategy. Similarly, firms that are located directly under the jurisdiction of municipal bureaus are experiencing a type of administrative uncertainty. Where higher level governing offices used to mean more access to slack resources for the firm (Walder 1992a), increasingly in the period of reform it means that the firm will be forced to handle the majority of administrative and economic responsibilities itself. The difference in the amount of attention a municipal bureau (with several hundred firms under its jurisdiction) and a district company (with jurisdiction over fewer than 10 firms) can pay to organizations under their respective jurisdictions is crucial to a firm's sense of being set adrift in the economic transition. Direct attention to long-term development strategies are offered by companies to the firms underneath them, while bureaus have not the time or administrative resources to deal with such developmental details for all of the firms under their jurisdictions. The result is that firms at the upper echelons of the industrial hierarchy experience an administrative uncertainty, and, much like economically weak firms, they also seek stability through short-run, fast-return investments.

The implications this study of organizational practices in four industrial sectors in Shanghai has for a general understanding of China's transition from a command economy to a more open market system should not be overstated. However, I do offer compelling evidence on the relationships among state structure, economic strategies, and the organizational search for stability in the economic reform; the changes in these relationships are grounded in the realities of the economic transition. In contrast to economic transitions in Eastern Europe and the former Soviet Union, China has been moving in a very gradual fashion (Rawski 1994), and as Rawski (1995) has recently argued, the objectives and direction of this project are unclear, even to the Chinese (see also Walder 1994). But if China is mov-

ing toward a stable system that is truly a "Chinese style socialism" as it has often been called, what would that stable system look like, or what is it beginning to look like now?

First of all, it is a system in which economic responsibilities are being pushed down the skeletal hierarchy of the former command economy. District companies and factories themselves are being given the power and responsibility to make economic and administrative decisions that were beyond their realms under the command economy. Organizations are increasingly responsible for their own losses, a fact that indicates the hardening of budget constraints both nominally and often in practice. It is also a system in which organizations are increasingly sensitive to emerging markets, and they are taking their own initiative in investing in them as Nee's marketization approach implicitly predicts. But this set of transformations should not be mistaken for the disappearance of the hierarchy. While Nee (1989) has argued that the hierarchy of the command economy would be dismantled as market mechanisms are strengthened, I think it is a misreading of the economic reform to assume such a paradigm shift lies in the near future. Even though economic and administrative responsibilities are being pushed down the hierarchy, the hierarchy itself is far from disappearing. First, even in the most marketized sectors, the state maintains a majority stake in most of the organizations. Second, the state still maintains the power of administrative fiat at the level of bureau offices. These particular functions of the state are even true in sectors where the bureaus have been replaced by State-Owned Asset Management Companies: even in these sectors, where we would expect to see the greatest withering of the hierarchy, we still see a state that controls over 80% of the industrial assets (managed by the new asset management companies, the former bureaus), the power to hire and fire managers of large factories, and the administrative fiat to control large investment and development projects.

The emerging system is also a system that is highly contingent on the preexisting institutional structures of the state (Walder 1995; Stark 1992, 1996) and the politics of market construction (Fligstein 1996b). Overall state control still allows for variation on governance patterns and strategies, especially in a nested organizational hierarchy like China's. Large organizations that are under bureaus have to seek other means to create stable economic worlds, a developmental strategy that organizations under district companies are less likely to seek. The question for the future will be, which approach allows for more stability and longer term economic growth? The answer to that question may indicate much about the selection process that will ensue as China's economic reform continues.

A final word about the implications of this study for the hardening of budget constraints: the analysis I present on China's urban industrial

economy fits well with some of the conclusions Walder (1995) presents but, in addition, offers evidence that tests and questions some of the hypotheses presented in that analysis. Walder noted that Kornai's (1990) theory of soft budget constraints required modification for the case of China because, where the organizational assumptions of the theory did not hold, budget constraints have been hardened, with a result of surprisingly successful growth patterns in these sectors (lower levels of the industrial hierarchy). He further hypothesized that Kornai's theory is correct for the upper levels of China's industrial economy, where his organizational assumptions do hold, particularly for those firms under bureaus and in urban areas. The path dependence of my findings fits well with Walder's assertion that different levels of the governmental hierarchy will approach the reforms in different ways. But the data presented here also reveal somewhat of a different picture than Walder predicted for the urban industrial economy. While my data cannot be taken as direct evidence of hardening budget constraints—many managers are quite inhibited when it comes to talking about these issues directly—the findings I present here do reveal actions and institutional changes that belie a system of soft budget constraints for upper-level firms (the question being, What would be the incentive for firms operating under a system of soft budget constraints to adopt stability-seeking strategies?). The perspective I present here argues that budget constraints are being hardened at all levels of China's administrative hierarchy (not just lower levels), but we clearly need different types of data and different types of analyses to illuminate the full picture of what is occurring in China's economic transition. Aggregate data on growth in output and productivity are useful for revealing where hardened budget constraints have led to successful transition patterns, yet they tell us little about where firms are failing or experiencing uncertainty in the market reform. Budget constraints are being hardened through a tightening of state budgets and the introduction of the self-responsibility policies of the market reform, but this is a process that is grounded in the relative responsibilities of governmental offices and the number of firms among whom they have to divide fiscal and administrative resources. Fiscal constraints may be hard at all levels of the administrative hierarchy, but the greater monitoring capacities that lower-level offices have over the relatively few firms under their jurisdictions give the firms at lower levels of the hierarchy an added advantage in terms of administrative attention to long-run development plans. The upper-level firms, which do not benefit from such administrative attention, turn to shorter run strategies. In the case of urban industrial China, we see a market transition in which the state implements reforms in a decentralized and stepwise process. Self-responsibility and independent accounting policies are experimented with on local levels and eventually promulgated on a widespread

basis. Governmental administrative organizations have discretion over the extent of the implementation of these policies, but they too are bounded by the funds (and administrative resources) they have available to them. As the reform proceeds, government organizations can no longer afford to support the firms under their jurisdictions blindly, and the larger these jurisdictions are, the more difficulty the governmental organizations have supporting them financially and the more difficulty they have offering them feasible development options. As the governmental offices—particularly the upper-level offices—become more investment oriented, the selection process of which firms to support and which ones to cut loose works very much like market selection. The favored firms are the ones that are already in the best economic health, and the others, increasingly responsible for their own gains and losses, are left to survive in the emerging markets of the economic reform. The outcome is a transition where hard budget constraints and marketization are realities of the reform, but the conditions of the marketization and the governance of firms in the new markets are, at the same time, contingent on the state institutional structure and a firm's position in the hierarchy of the former command economy.

APPENDIX  
Comparative Sector Data

TABLE A1  
CHARACTERISTICS FOR FOUR INDUSTRIAL SECTORS IN CHINA, 1994

	Electronics	Foods	Chemicals	Garments
Shanghai data: <sup>*</sup>				
Fixed assets .....	49,162,609	13,967,857	694,333,333	6,473,818
Active employees .....	1,634	378	5,094	279
Retired employees .....	723	148	1,159	88
State owned .....	652	.714	867	.182
Collectively owned .....	.348	.143	0	.727
Other owned .....	0	.143	.133	.091
Age in years .....	33	33	39	22
Profit margin .....	.06	.08	.08	.07
Profit/size .....	2,545	5,573	15,252	6,175
Profit/fixed assets .....	.097	.286	.216	.336
1994 turnover .....	92,826,087	45,874,286	845,133,333	26,399,715
1990 turnover .....	66,326,087	27,042,105	394,181,818	14,503,880
1994 turnover/size .....	55,222	95,637	216,839	102,574
Average salary .....	7,346	10,214	10,887	8,286
National data (aggregate industry): <sup>†</sup>				
Capital construction ( <i>Jiben Jianshe</i> ):				
Projects completed .....	90	184	382	40
Projects completed/projects contracted .....	49.8	59.2	45.8	38.8

TABLE A1 (Continued)

	Electronics	Foods	Chemicals	Garments
Technical updates and transformation ( <i>gongzhi gaisao</i> ):				
Projects completed . . . . .	256	670	2,255	129
Projects completed/projects contracted . . . . .	34.6	61.2	57.7	60.1
Increased fixed assets (in billions) . . . . .	3.78	2.73	7.89	.39
Indicators for industrial enterprises with independent accounting systems ( <i>dali kezhuan</i> )				
No. of enterprises . . . . .	6,313	14,074	23,553	17,921
Gross output value (in billions) . . . . .	129.9	117.8	237.7	99.3
Net output value (in billions) . . . . .	69.8	34.1	68.4	33.5
Total capital assets (in billions) . . . . .	14.5	32.7	39.1	52.4
Total fixed assets (in billions) . . . . .	48.8	37.8	151.6	24.1
Total turnover (in billions) . . . . .	134.1	69.1	228.5	81.7
Total profits (in billions) . . . . .	10.3	8.7	19.3	5.4
Profit margin . . . . .	.07	.11	.08	.06

NOTE.—All fiscal figures given in *yuan*.\* Data is based on mean values for sample organizations in Shanghai data set, *N* = 81.

† Data for these industrial categories were gathered from the 1994 *Statistical Yearbook of China*, pp 164–67, 378–81, 396–97, and the 1994 *Statistical Yearbook of Shanghai*, pp 135–68, the data are 1993 year-end data, one year earlier than the data gathered for my study. The sectoral categories reported are based on the same sectoral categories from which the sample for my survey was selected in the *Directory of Chinese Organizations and Institutions*. As my survey combined the sectors of “food manufacturing” (*shiyi shiye*) and “beverage manufacturing” (*yinqiao shiye*) to sample organizations in the foods sector, these categories are averaged for the values under the foods category presented here. The other categories are “electronic and telecommunications” (*dianzi ji tongxin shiye*) for organizations in the electronics sector, “raw chemical materials and chemical products” (*huaxue yanbao ji huai shiye*) to sample organizations in the chemicals sector, and “garments and other fiber products” (*ruanhuang ji qia gong shiye shiye*) to sample organizations in the garments sector.



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# Productive Activities and Support Systems of Single Mothers<sup>1</sup>

Lingxin Hao  
*Johns Hopkins University*

Mary C. Brinton  
*University of Chicago*

Young single mothers' human capital development and labor market participation are important issues of public policy concern in the United States. This article uses a dynamic approach to model the determinants of single mothers' entry into and exit from productive activities. Using 14 waves of data from the National Longitudinal Survey of Youth, the article shows that kin coresidence facilitates young single mothers' entry into productive activities but does not play a significant role in sustaining participation. Women's individual trainability, the local labor market conditions, child support, and some family background factors all play a role. The results also demonstrate the insignificance of race and never-married versus ever-married status.

## INTRODUCTION

Over the past two decades, there has been a divergence in married versus single mothers' labor force participation and school attendance. While employment has steadily increased among married mothers, especially those with young children, employment among single mothers has been stagnant since 1980 (U.S. Bureau of the Census 1995). The divergence is even greater when considering female recipients of Aid to Families with Dependent Children (AFDC), the cash assistance program for poor families prior to the 1996 welfare reform legislation. Female AFDC recipients' employment fell from 16% in 1979 to 5% in 1984 (Moffitt 1992) and remained low (6%) in 1991 even after the reinforcement of the work and training requirement by the 1988 Family Support Act (U.S. House of Rep-

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representatives 1993).<sup>2</sup> At the same time, the gap in educational attainment between women giving birth as teenagers versus as adults has increased (U.S. General Accounting Office 1994). The low employment rate and low educational attainment of single mothers both have a profound impact on their prospects for self-sufficiency and are therefore issues of great scholarly and public policy concern.

The absence of a husband means that a single mother must play the dual role of provider and nurturer, a task that is nearly impossible without some type of external support. Single mothers commonly rely on two types of support systems. One is public assistance, with AFDC as the most important program that was particularly targeted to single mothers and their children. The other support system is parents or kin who provide financial or in-kind assistance and, often, normative supervision. Research on single mothers' labor market participation has primarily focused on the first type of support rather than the second. But the absence of strong empirical evidence concerning the relationship between kin support and single mothers' labor force participation or other productive activities, such as school attendance, is an important lacuna in social science research. Meanwhile, the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 focuses on ways that single mothers can be assisted to reach self-sufficiency.<sup>3</sup> For example, the new welfare legislation requires states to deny AFDC benefits to single mothers under age 18 unless they live with their parents and are enrolled in school or in a training program. It is not known what the effect would be of a policy requiring single mothers to live with their parents or other adult kin. Would single mothers be more likely to enter the labor force or return to school, given the presence of other adults in the household? Would they be more likely to remain in these activities once they have entered them if other adults are present? Social science research to date has provided few guidelines on which to make these crucial assessments.

In this article, we seek to develop a theoretical framework to predict the role of kin support in single mothers' decisions to participate in productive

<sup>2</sup> The 1988 Family Support Act combines an emphasis on moving people into jobs with increased funding for the education and training believed necessary to make this possible. A program called Jobs Opportunity and Basic Skills (JOBS) allows working AFDC recipients to receive AFDC benefits for the first 12 months while working, even though the earnings may exceed the AFDC income eligibility threshold. In addition, JOBS increases child care assistance in order to facilitate AFDC recipients' employment.

<sup>3</sup> These attempts have been driven by the increase in the number of single mothers, rising welfare case loads, increased long-term dependency, and the growing share of single mothers on welfare who have never married (U.S. Bureau of the Census 1991; U.S. General Accounting Office 1994).

activities outside the home. Our approach departs from previous research in four ways. First, we extend the focus of prior research to include school enrollment as well as employment. We do so because both of these activities have profound implications for single mothers' self-sufficiency and current policy concerns focus more on the distinction between productive and nonproductive activities than on the distinction among types of productive activities. Second, most previous studies have examined static and cross-sectional employment rather than the dynamics of labor market entry and exit. This article examines entry into and exit from productive activities as separate processes. This has great importance for the types of policy implications that arise. Do the same factors that promote single mothers' entry into the labor force or education also *sustain* participation, or do other variables assume greater importance in facilitating its continuation? Third, in examining the effects of kin support, we treat it as endogenous rather than as exogenous, in contrast to most previous research. And finally, prior research has generally not examined the reciprocal causal processes between participation in productive activities and kin support. We, however, use a dynamic model that considers the possible interdependence between these two.

The article is organized as follows. The second section briefly reviews the literature on support systems for single mothers and their effect on participating in productive activities. We develop a theoretical framework that integrates elements of microeconomic and sociological theory in order to make predictions about welfare and kin support. We also devote attention to other economic and social factors that may affect single mothers' productive activities, including individual characteristics (such as human capital and family background) and characteristics of the local economic and social environment (including welfare benefit levels, poverty rates, and unemployment rates). The third section describes the data used in the analysis and outlines the methods used to test the hypotheses. In the fourth section, we examine the consistency of the data with the predictions, assessing in particular whether kin support plays an important role in facilitating the entrance of young single mothers into productive activities and helping them to remain engaged. The final section summarizes our findings and outlines policy implications of the analysis.

#### SUPPORT SYSTEMS FOR SINGLE MOTHERS

Social scientists have extensively documented the negative health, social, and economic consequences produced by the dual role dilemma of single mothers. The responsibilities of being both a provider and a nurturer often interrupt a single mother's education, which in turn lowers her worth in both labor and marriage markets and increases her likelihood of being

trapped in poverty and welfare dependence (Duncan, Featherman, and Duncan 1972; Krein and Beller 1986; McLanahan 1985; Hill, Augustyniak, and Ponza 1987; Furstenberg, Brooks-Gunn, and Morgan 1987; Bumpass and McLanahan 1989; Hao 1994, 1996). Recent studies on the economic consequences of out-of-wedlock child bearing have further confirmed that it has adverse economic consequences and that these effects differ substantially by race and across cohorts (Lundberg and Plotnick 1995; Geronimus and Korenman 1992; Bronars and Grogger 1994).

Single mothers often rely on government or kin support. About 35% of young single mothers received AFDC payments during 1985 (Hao 1995), and AFDC support was received by over three-quarters of all poor female-headed families in 1984 (Ellwood 1988). It has been estimated that between 48% and 69% of AFDC participants exit the AFDC program within two years, leaving a substantial minority who remain dependent for a longer period of time (Harris 1993).

Many single mothers receive substantial support from their parents or other kin in the form of coresidence, financial transfers, and child care. Stack (1974), Uehara (1990), and Edin (1991) have shown that poor women on public assistance must typically supplement AFDC with private support.<sup>4</sup> Using a nationally representative survey of youth, Hogan, Hao, and Parish (1990) found that, in the mid-1980s, 56% of black single mothers and 31% of white single mothers resided with adult kin; one-third of white single mothers and one-quarter of black single mothers received financial aid from kin that covered at least half of their living expenses; and two-thirds of single working mothers, black or white, had kin-provided child care assistance. Schoeni (1994) used another national survey to show that, in 1987, 28% of single-mother families received financial transfers from family and friends and 39% received help in household work or child care from relatives and friends.

Welfare and kin support each provide single mothers with external support that can affect their decisions about engaging in productive activities. On balance, more is known about the effects of welfare than the effects of kin support.

### Welfare Support

In microeconomics, welfare benefits and the attached tax rate on earnings are conceptualized as having two effects that reinforce each other on a person's decision to participate in the labor market. First, a higher level

<sup>4</sup> However, the generalizability of these studies is limited by the fact that they are usually based on specific communities and small samples.



of welfare benefits may enable one to consume more without giving up nonmarket time. When the prices of consumer goods and nonmarket time are held constant, a higher benefit level will reduce the likelihood of employment. This is usually called the "income effect" and is familiar from the literature on married women's employment (where husband's higher income typically lowers a woman's probability of labor force participation). A second effect involves a change in the price of nonmarket time (or the opportunity costs of not working). If the price of nonmarket time decreases, a woman is less likely to work. This is usually called the "substitution effect" or "price-of-time effect." In the case of welfare participation, since the tax rate imposed on earnings lowers the price of nonmarket time, recipients have little incentive to work. Because the income effect and the substitution effect operate in the same direction, welfare has an unambiguous negative effect on work (Moffitt 1992).

The work disincentive effects of welfare have been extensively documented. The AFDC program provides cash payments to single mothers with children under age 18 if family income is below the eligibility threshold. An increase in AFDC benefits does not affect families with incomes far above the eligibility threshold because the gain from labor market participation remains much higher than the gain from the program. But increases in AFDC benefits affect families whose income is lower than or narrowly above the threshold. For example, a single mother working in the labor market full-time with a relatively low wage rate is not eligible for AFDC benefits and the accompanying Medicaid and housing subsidies. However, her market earnings may hardly exceed the combined welfare benefits as well as the costs related to work (child care, transportation, clothing, etc.). If a woman instead works part-time while participating in the program, the AFDC benefits are reduced dollar for dollar of the amount of earned income after four months of employment, and other assistance such as Medicaid is reduced or withdrawn. Therefore, for a single mother with a low earning capacity, working may produce less income than participating in the AFDC program. It makes little sense for her to use AFDC payments for child care costs in order to work, since the financial outcome of work will be a reduction in AFDC benefits (Jencks 1992).

Throughout the development of the AFDC program, researchers have conducted numerous evaluations using participants-only data, nationally representative samples, and large-scale social experiments. Many studies have shown that the AFDC program has a moderate negative effect on recipients' labor supply (see Moffitt [1992] for a review). But because school attendance does not disqualify the AFDC recipient and is not imposed with a tax rate, AFDC benefits should not have a price-of-time effect on schooling. Therefore, when we combine employment and school

enrollment, AFDC benefits should still exemplify a case where unearned income produces a negative effect, which should be weaker than for the case of employment alone.

### Kin Support

In microeconomics, financial, material, and in-kind transfers from kin are conceptualized as "unearned income." According to microeconomic theory, unearned income is expected to reduce market time but have no effect on the price of nonmarket time (the opportunity costs of not working). Under this conceptualization, kin support is predicted to have only an income effect that reduces market time.

Social capital theory challenges the microeconomic proposition with its emphasis on the normative expectations and obligations fostered in social relationships (Coleman 1988, 1990). We argue that kin support may change the price of nonmarket time through normative supervision and encouragement provided by kin for the single mother to achieve self-sufficiency and through the normative obligation of the single mother to fulfill these expectations.

Normative supervision and encouragement are hard to pick up in social surveys and are seldom measured. However, the situation where single mothers live in their parents' or other relatives' home may be the typical one where in-kind support and normative supervision and encouragement take place.<sup>5</sup> We suggest that kin coresidence may change the price of a single mother's home time in three ways: (1) normative supervision—coresidence may facilitate more effectively parents' and kin's expectations of the single mother and raise her obligation to fulfill these expectations by being engaged in productive activities; (2) child care assistance may be provided; and (3) exposure to the world of work—coresident kin may provide examples of the experience of a normal work routine and also may be helpful in providing information about jobs and access to social networks of employed friends and other kin.<sup>6</sup> Coresidence could be initi-

<sup>5</sup> The positive effect of kin coresidence for single mothers can be substantiated by many ethnographic studies (Stack 1974; Uehara 1990; Lundgren-Gaveras 1991). However, kin coresidence in some instances may turn out to be a burden for single mothers, i.e., in the following situations: (1) parents/kin are ill behaved or abusive, (2) support needed by kin is greater than what kin can provide to the single mother (Lundgren-Gaveras 1991), and similarly (3) coresidence is for the purpose of pooling resources, with the single mother contributing more than parents and kin. At the same time, noncoresident kin, particularly those living in close proximity, may be able to provide normative supervision and encouragement for single mothers.

<sup>6</sup> We are aware of potential conflict between parents and daughters, particularly when parents initiate assistance and impose supervision, but the prediction remains the same. In addition, while this study assumes that parents conform to social norms, we

ated either by a single mother who wants to be self-supporting or by kin members who desire this outcome. Whoever initiates it, we would predict the outcome to be the same: kin coresidence should produce a dominant price-of-time effect and therefore increase the probability of productive activities in the market or in education.<sup>7</sup>

*Normative supervision.*—We argue that, when adolescent or young adult children have economic or personal difficulties, it is likely that many parents make transfers to their children not only out of a felt obligation to support them but also out of a desire to provide normative supervision. Supervision and encouragement from parents and kin are important in helping adolescents or members of the younger generation follow socially approved paths such as completing high school or higher education and obtaining appropriate employment (Goode 1961; Mueller and Pope 1977). A few sociological studies consider the motivations and outcomes of intergenerational transfers. Applying social exchange theory, Mutran and Reitzes (1984) emphasized the notion of intergenerational roles and the fact that roles and role outcomes are not fixed but are instead somewhat flexible and negotiable; when intergenerational transfers are made, a realignment of the hierarchy of roles often occurs. In the context we are considering, parents who provide room and board and other help may become influential in supervising their daughters' productive activities.

*Provision of child care assistance.*—The presence of an adult other than the husband in the household is positively correlated with women's work (Connelly 1989; Blau and Robins 1989; Parish et al. 1991). Child care by relatives, especially for young children, tends to be highly valued by parents (Mason and Kohlthau 1989). Klerman and Leibowitz (1990) have argued that the presence of family members in the household other than the husband who can substitute for the mother in child care promotes an earlier return to work for women who had worked before giving birth.<sup>8</sup> In the case of single mothers, the provision of in-kind and child care assistance while residing together would presumably free up time for education and employment.

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acknowledge that it is nevertheless important to keep in mind the possibility of negative influence from parents as well.

<sup>7</sup> Few studies in sociology have examined the impact of kin support on single mothers' labor market participation. Parish, Hao, and Hogan's study (1991) reported that kin support in the form of financial transfers, coresidence, and child care provision promotes the labor market participation of married but not single mothers. But because this study used only cross-sectional data, it could not delineate the effect of kin support on the processes of labor market entry and exit.

<sup>8</sup> They found that the presence of a grandmother significantly affected the mother's return to work within the first three months after birth. The importance of relative care, however, was attenuated after the first three months.

*Exposure to work and to social contacts.*—Ethnographic studies have shown that many unemployed single mothers who live alone experience extreme social isolation from the world of paid work (Uehara 1990; Lundgren-Gaveras 1991). Living with parents and kin ends this isolation and may offer everyday exposure to the work routine of employed household members as well as to their contacts in the working world. Sociological and economic studies have demonstrated the importance of informal networks in the job search process (Marsden and Campbell 1990; Wegener 1991; Montgomery 1992, 1994). Kin coresidence may provide labor market contacts not only directly through the employed household members but also indirectly through these members' social ties.

Kin coresidence, however, is not something that is automatically available. Individual actors with different interests create and maintain such a living arrangement. Prior research on the determinants of kin coresidence has focused on whether it represents primarily a response to economic need or whether it tends to be a characteristic of the culture of particular race or ethnic groups. Some scholars report that the available economic resources relative to a family's basic needs explain away racial differences in living arrangements (Morgan 1982), whereas others find that both race/ethnicity and income are important determinants of kin networks (Tienda and Angel 1982). These inconsistencies in empirical research suggest the need for further examination of the determinants of kin coresidence and the need to incorporate kin coresidence as an endogenous variable affecting participation in productive activities.<sup>9</sup>

Empirical work examining the impact of kin support on employment suffers from four methodological problems. First is the possible reverse relationship between kin support and employment—does employment determine kin support (i.e., does a young woman's work or school enrollment lead her to request help from parents and kin?). Previous research has seldom considered the possible interdependence between kin support and participation in productive activities. Second is the issue of the endogeneity of kin support (i.e., kin support may be determined by a set of factors that also determine productive activities, an issue also seldom considered in previous research). Two additional problems are that empirical work has seldom examined the effect of kin support on school enrollment and has largely ignored the dynamic nature of women's productive activities.

Integrating microeconomic theory and social capital theory, we argued that kin support, particularly in the form of kin coresidence, could effec-

<sup>9</sup> Most sociological and ethnographic studies of kin support have focused on its prevalence according to race, ethnicity, socioeconomic status, family structure, and gender rather than on its motivations and effects.

tively change the price of nonmarket time. In other words, because of the high value placed on working due to normative supervision and encouragement, together with the reduced child care costs resulting from kin-provided assistance and reduced costs in job searching due to parents' labor market attachment, the opportunity costs of not working should increase. Under this new conceptualization, kin coresidence is predicted to exert both an income effect and a price-of-time effect. Unlike the case of welfare, where the income and substitution effects operate in the same direction, in the case of kin support, the two effects operate in opposite directions. We suggest that, because of the higher value placed on working and the reduced costs of child care and job searching, it is likely that the price-of-time effect dominates, so that overall, kin support promotes participation in productive activities.

Our test of the effect of kin coresidence is made rigorous by modeling its endogeneity and considering its potential reverse relationship with productive activities. We also test for the role of noncoresident kin by examining whether kin support measured only in terms of financial transfers affects single mothers' entry into or exit from productive activities.

A number of other variables may also affect single mothers' participation in productive activities. Welfare support generally reduces the probability of productive activities (Moffitt 1992). But we expect that this effect may be relatively weak in our model since we include school enrollment, which AFDC does not penalize. Parental education and income are common measures of parental socioeconomic status and social class, which are related to the greater valuing of self-sufficiency for children (Kohn 1969). Thus a middle-class background may reinforce single mothers' intention to participate in productive activities.

Mother's employment when the respondent was a teenager reflects gender role socialization (Stockard and Johnson 1980). For a young mother, one effect of having had a working mother is that she was exposed to a woman who balanced commitments to work and to children. By virtue of this, we predict that this experience increases a single mother's propensity to engage in productive activities outside the home. Single mothers' schooling at first pregnancy and her "trainability" are measures of human capital that we use to predict participation in productive activities (Becker 1975). Marital history and number of children determine the opportunities and constraints of kin support. Never-married mothers have fewer economic resources than their ever-married counterparts and so are more likely to coreside with parents and kin. A large number of children would place a heavy burden on kin and may make kin coresidence less likely. More children also impose greater time constraints on single mothers' participation in productive activities, so we predict a negative effect of num-

ber of children. Child support is a source of unearned income for single mothers and might enable them to purchase child care, thereby producing a dominant price-of-time effect on productive activities.

The local economic context is also important. We predict that higher unemployment rates and poverty rates lower the likelihood of single mothers' participation in productive activities. Unemployment rates reflect local employment opportunities and the probability of mismatches between available jobs and local residents' job qualifications. Poverty rates are a reflection of the degree of community isolation from mainstream social, occupational, and political institutions (Wilson 1987).

We realize that these explanatory variables do not exhaust all of the important factors that may affect productive activities and kin coresidence. For example, unobserved individual background differences and characteristics such as innate ability and personality traits may be associated with a woman's propensity to engage in productive activities. Having caring parents and relatives also increases the likelihood of coresidence. We therefore also include this unmeasured individual-specific heterogeneity in the model.

## DATA AND METHODS

### Data

We draw on 14 waves of the National Longitudinal Survey of Labor Market Experience, Youth Survey (NLSY) 1979–92. The NLSY is appropriate for our goals because it provides complete education and employment histories as well as detailed information on household members over a 14-year period; from this we can determine whether the respondent was living with adult kin. The longitudinal nature of the data also enables dynamic analyses. The NLSY sample of 12,686 young Americans ages 14–21 years old in January 1979 includes a national, probability-stratified random sample of 6,111 youths and a supplemental sample of 5,295 blacks, Hispanics, and economically disadvantaged whites (Center for Human Resource Research 1987*a*, 1987*b*). Thus we have a large sample of young single mothers over a fairly long period, during which the relationship between kin coresidence and productive activities can be rigorously modeled.

We are interested in the population of women who became single mothers either through an out-of-wedlock birth or through a divorce. Because unwed childbearing occurs in the teenage years and beyond and divorce occurs at various ages, we include all NLSY single mothers in our analysis. Both fertility and marital status are required to determine single motherhood status. The timing of a woman's first single motherhood spell is determined by the first unwed birth or the timing of first divorce given

that she has had a marital birth. In our analysis, we consider those women who became first-time single mothers at some point during the 14 years of observation.<sup>10</sup> Second-time single motherhood spells are excluded from the analysis since they represent somewhat different experiences and difficulties from the first spells and only a small proportion of the single mothers in the analysis experienced a second spell during 1979–92. In the longitudinal framework, we include in the sample the entire period from the time a woman became a single mother for the first time until she married or until the end of the observational period. The length of a single motherhood spell therefore ranges from one to 14 years. We include in the analysis only those spells that are at least three years in length, for two reasons:<sup>11</sup> (1) our dynamic model requires two previous states to include state dependence ( $t - 1$ ) and duration effects ( $t - 2$ ); and (2) our dynamic model deals with initial conditions by conditioning on the endogenous variables in the first two years of the processes (described below).

The resulting sample consists of 1,576 single mothers whose first spell of single motherhood was three to 14 years long during 1979–92, yielding 9,225 person years. We define productive activities as being either enrolled in school or being employed for at least 50% of the time between two interviews.<sup>12</sup> We assign the value “1” if an individual spent at least 50%

<sup>10</sup> It is likely that some women had become single mothers before 1979 by a birth out of wedlock or by the disruption of their first marriage. That is, for some single mothers, 1979 may not be the initial year of single motherhood. Also, entrance into the initial state of productive/nonproductive activities and coresidence/noncoresidence for a single mother, while not coterminous with the single motherhood spell, may have occurred before 1979. The potential resulting problems are related to “left censoring” (Tuma and Hannan 1984, Heckman and Singer 1986) and to “spells with unknown origin” (Hamerle 1991). Of the individuals in our sample, 15% were left censored, i.e., their single motherhood started before 1979. The majority of these left-censored individuals were never married and had long spells of single motherhood. While we have retrospective data on marriage, fertility, and employment prior to 1979, we do not have information on one of the dependent variables—kin coresidence—and many exogenous variables prior to 1979, therefore we could not restore the entire spells for these left-censored spells in the analysis.

<sup>11</sup> About 24% of the NLSY single mothers experienced a single motherhood spell of two years or less. Excluding short single motherhood periods may mean that we include single mothers who have a lower probability of marrying, since single motherhood spells ending in marriage are shorter than censored single motherhood. To test for this sensitivity, we estimated a model that included spells of two years or longer (excluding the second-order state dependence). The results (not shown here) are largely the same as the results for single motherhood lasting at least three years. We further tested the sensitivity to eliminating the one-year spells by reducing our dynamic model to a pooled cross-sectional model, here the effect of coresidence was positive but weaker and marginally significant. In addition, we include in our model variables indicating married-out and censored-out as well as single motherhood duration.

<sup>12</sup> Sensitivity tests show that the signs of the estimates remain the same when different cut points are adopted. The positive effect of coresidence on entry into productive

of her time between interviews in school or working and the value "0" otherwise. We choose 50% of the time as a cut point since it indicates a substantial amount of time and implies a serious commitment.

Kin coresidence is defined as living in the home of parents or other adult relatives, as reported at the time of each interview. Cases where parents or other relatives were living in the single mother's home are not considered as coresidence. We restrict the operational definition of kin support in this fashion so as to rule out the possibility that the single mother instead supports her parents or other kin. Additionally, because parents and other relatives headed the household, we can postulate that they were in a more powerful position than otherwise to provide effective normative supervision. Therefore, this measure is appropriate for our theoretical purposes.<sup>13</sup> Although the survey provides a detailed week-by-week employment history and month-by-month school enrollment history, we use discrete-time measures of productive activities and kin coresidence with intervals between interviews, since kin coresidence and many time-varying explanatory variables such as community conditions were measured only at the time of each interview.

### A Model for the Interdependence of Two Endogenous Processes

The analysis is designed to estimate the effects of kin coresidence on productive activities among single mothers in a longitudinal framework, taking into account the potential reverse causal relationship by specifying cross-lagged causal relationships between kin coresidence and productive activities. The longitudinal data on both coresidence and productive activities enable us to sort out the causal order between the two endogenous variables through constructing proper feedback effects for given individuals as they change status with respect to the two endogenous variables (Finkel 1995; Hsiao 1986).

We adopt a model advanced by Yamaguchi (1990) that uses a multinomial logit specification to model the interdependence of two discrete-state endogenous processes. The dependent variable of the multinomial logit model is the cross-classification of the two endogenous variables. This model permits a cross-lagged causal analysis of the two endogenous

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activities increases with higher cut points (from 10% to 90% with an increment of 10% in each test) and the effect is significant at the .05 level in all tests. Women who moved directly from school to work or vice versa with no intermediate nonproductive spell are treated as continuing productive activities.

<sup>13</sup> We tested whether an operational definition that includes cases where parents and kin lived in single mothers' households would make a difference. Results showed that the effect of kin coresidence on entry into productive activities remained positive but was somewhat weaker.



processes and the simultaneous analysis of transitions between the two states of each process.

Using Yamaguchi's model, we developed a method that links multinomial logit parameters to the parameters that fit the two endogenous processes. This method allows us to model the interdependence of two endogenous processes using existing statistical packages. The converted estimates are simple linear combinations of the multinomial logit estimates; standard errors of the converted estimates can also be obtained through linear combinations of elements in the variance-covariance matrix of the multinomial logit estimates (see the appendix for a detailed technical exposition).

The dependent variable of the multinomial logit specification in our analysis is the cross-classification of productive activities and kin coresidence. This yields four outcomes: neither kin coresidence nor productive activities, only productive activities, only kin coresidence, and both. The transitions between the two discrete states are entry into and exit from productive activities and kin coresidence. We test the cross-lagged causal relationships between kin coresidence and productive activities in the past two periods and simultaneously analyze entry and exit, which is efficient since the potential differential effects of a particular covariate can be tested. We deal with initial conditions by conditioning on kin coresidence and productive activities at year 1 and year 2. Thus, our analysis starts from year 3, and our sample includes single motherhood spells that are at least three years long.<sup>14</sup> Within each endogenous process, we consider both the state dependence of the immediate past period (first-order state dependence) and beyond the immediate past period (second-order state dependence). Yamaguchi (1990) shows that the second-order state dependence is equivalent (albeit with different scaling) to the duration effect beyond the immediate past period.<sup>15</sup>

Our discrete-time, discrete-state dynamic model includes individual-specific heterogeneity (Heckman 1981; Chamberlain 1985; Hsiao 1986; Maddala 1987). In addition to the substantive reason why we include individual-specific heterogeneity (as mentioned earlier), controlling for this heterogeneity is crucial to produce consistent estimates. First, because these propensities tend to be persistent over time, they cause the distur-

<sup>14</sup> Chamberlain (1978) suggests dealing with the initial conditions by conditioning on the state at year 1. See Corcoran and Hill (1985) for an empirical example. We extend Chamberlain's idea to condition on the states at year 1 and year 2 since we are also concerned with the duration effect that is captured by the state at  $t - 2$ .

<sup>15</sup> This model is also called a "semi-Markov model," which allows the transition rate to be a function of the duration effect over and beyond the state dependence, captured by the state at  $t - 2$  (Bartholomew 1982; Yamaguchi 1990).

bance terms to be serially correlated. Ignoring these individual-specific effects will lead to a failure to single out the true state dependence. Second, because these propensities are likely to be correlated with the lagged endogenous variables (both productive activities and kin coresidence), failure to control for them will lead to bias in the estimates, including those regarding our major hypothesis about the lagged causal effects of kin coresidence.<sup>16</sup> By decomposing the error term into an individual-specific heterogeneity component and an independently and identically distributed stochastic component in the latent function underlying the  $j$ th state of the multinomial logit model, we use a random-effect dynamic model, as shown below:

$$y_{ijt}^* = \beta_0 + \beta_1 PD_{i,t-1} + \beta_2 KIN_{i,t-1} + \beta_3 PD_{i,t-2} + \beta_4 KIN_{i,t-2} \\ + \beta'_5 x_{i,t-1} + \beta'_6 z_i + \alpha_{ij} + \epsilon_{ijt},$$

where  $y_{ijt}^*$  is the latent variable for individual  $i$  in  $j$ th state at time  $t$ , PD represents productive activities, KIN represents kin coresidence,  $x_{i,t-1}$  is a vector of time-variant exogenous variables,  $z_i$  is a vector of time-invariant exogenous variables,  $\beta$ 's are the corresponding parameters to be estimated,  $\alpha_{ij}$  is the individual-specific heterogeneity in state  $j$  and  $\epsilon_{ijt}$  is the independently, identically distributed disturbance term. This model is estimated using a maximum likelihood estimator.<sup>17</sup>

### Exogenous Variables

We expect that welfare support lowers the probability that single mothers participate in productive activities. *Welfare support* is measured by the monthly state-level benefits from the AFDC program for a family consisting of an adult and a child. We have added to the NLSY data the maximum guarantee level of the AFDC program in the state in which the respondent resided at each wave of the 14-year period. The guarantee levels are thousands of constant 1989 dollars adjusted by the Consumer Price Index (CPI).

We expect that a woman's educational level and trainability, two measures of human capital, affect her schooling and employment. School enrollment is part of one of the endogenous variables, so, rather than using

<sup>16</sup> This is similar to the "omitted variable" or "misspecification" problem. The longitudinal data of NLSY that provide information on the intertemporal dynamics at the individual level can be utilized for a model including unobserved individual heterogeneity (Hsiao 1986).

<sup>17</sup> The program used to estimate this model, SON-of-CTM, was written by James Heckman, Steve Cameron, and George Yates. The authors gratefully acknowledge the permission to use the program.

current schooling as an exogenous variable, we use years of schooling at first pregnancy. *Trainability* is measured by the Armed Forces Qualification Test (AFQT). We choose a subset of tests that are more important for women's than men's jobs such as word knowledge, paragraph comprehension, numerical operations, and coding speed (excluding arithmetic reasoning, auto and shop information, and mechanical comprehension). Since the scores are not normed by educational levels, we construct the percentile ranks of the selected AFQT scores among all female NLSY respondents with the same level of schooling. The percentile ranks should then represent trainability net of education effects.

We use a number of background variables including *parents' education* in years, yearly parental family income, family structure, and mother's employment status when the respondent was 14. Parents' education and family income are common measures of socioeconomic status and social class. Higher education, greater family income, and intact family structure represent the "middle class," which is more likely to adhere to mainstream norms and values; this thereby indirectly measures the degree of normative supervision. Mother's employment when the respondent was 14 is used as an indicator of gender role socialization that emphasizes mothers' participation in the public sphere. Parental socioeconomic status and mother's work status also indicate the degree of labor market attachment that key adult family members have demonstrated to their daughters. In sum, higher parental education, greater parental family income, intact family, and a working mother are predicted to lead to a greater likelihood of single mothers engaging in productive activities.

The model controls for demographic characteristics including race, age, marital history, number of children, and age of youngest child. We contrast whites and nonwhites (including blacks and Hispanics) because ethnographic studies have suggested more closely knit kin networks among minorities. We distinguish single mothers who were never married from those who were once married, hypothesizing that never-married mothers are more likely to live in the home of parents and kin. Women who are older, have fewer children, and have an older youngest child should have a higher probability of participating in productive activities outside the home. We also include the amount of yearly child support (in thousands of dollars) received and whether the single mother was living with a partner. Greater child support might promote productive activities since it enables the single mother to purchase child care and thereby produces a dominant price-of-time effect. Living with a partner reduces the possibility of living with parents or relatives and thereby lessens parents' supervision. Since the *time spent in single motherhood* may indicate the cumulative difficulties in handling the dual role of provider and nurturer, we include the number of years elapsed from the beginning of single motherhood, with

the expectation of a negative impact on participation in productive activities. Also, since a substantial proportion of single motherhood periods are censored (i.e., remaining in single motherhood until the end of the observational period), we distinguish censored single motherhood from single motherhood that ended via marriage. Women who exited single motherhood through marriage were observed with the event occurring while women whose single motherhood is censored have lower probabilities of marriage; this is particularly the case for women with long single motherhood spell duration.

We include several variables that reflect the contextual social and economic situation that is likely to affect single mothers' productive activities: residence in urban areas and in the South, state-level unemployment rates, and county-level poverty rates. Urban areas (including suburban areas and cities of various sizes) and areas with low unemployment and poverty rates represent environments that offer greater opportunities for single mothers to participate in productive activities. We also control for region, given regional differences in the United States in factors such as religiosity and social norms that may reflect social control and ideas about self-sufficiency. Finally, we control for the idiosyncratic period effect of economic cycles and the political and social environment by adding a set of dummies representing the 14 years of observations.

We test for all interactions between each of the exogenous variables and each of the endogenous variables. If the original multinomial logit estimates for these interaction terms are statistically significant at the .10 level or better, we keep them in the equation. If the interaction term is kept for an exogenous variable, its effects are different for entry into and exit from the endogenous variable (resulting from different combinations of the main and interaction effects, see technical appendix). If the interaction term is eliminated for an exogenous variable, its effects on entry into and exit from the endogenous variable are the same in magnitude but opposite in sign.

## RESULTS

Table 1 shows the proportion of the sample with single motherhood spells of three to six years length and seven to 14 years length, and the mean values of the endogenous and exogenous variables at the start of the single motherhood spell. About 44% of women in the sample had a single motherhood spell lasting less than six years, and the remainder had a spell lasting for a longer period. Women with longer spells were less likely to participate in productive activities than women with shorter spells. In contrast, women with longer spells were more likely to coreside with kin than women with shorter spells.

TABLE 1  
DESCRIPTIVE STATISTICS OF VARIABLES AT THE START OF SINGLE MOTHERHOOD, BY  
LENGTH OF SINGLE MOTHERHOOD SPELLS

Variable	3-6 Years	7-14 Years	Total
<b>Endogenous:</b>			
Productive activities .....	.51 (.50)	.25 (.49)	.45 (.50)
Kin coresidence .....	.37 (.48)	.53 (.50)	.46 (.50)
<b>Exogenous</b>			
<b>Individual characteristics:</b>			
White .....	.43 (.50)	.25 (.44)	.33 (.47)
Age (in years) .....	23.2 (4.2)	20.4 (2.6)	21.6 (3.7)
Never married .....	.61 (.49)	.84 (.37)	.74 (.44)
Education at first pregnancy (in years) ....	11.0 (1.6)	10.9 (1.7)	10.9 (1.7)
Trainability (percentile) .....	42.3 (26)	32.6 (25)	36.9 (26)
Censored single motherhood .....	.35 (.48)	.78 (.41)	.59 (.49)
No. of children .....	1.1 (.68)	1.1 (.61)	1.1 (.64)
Age of youngest child (in years) .....	1.5 (2.2)	.73 (1.3)	1.1 (1.8)
Yearly child support (in \$1,000) .....	.32 (.97)	.25 (3.4)	.28 (2.6)
Living with a partner .....	.15 (.36)	.10 (.30)	.12 (.33)
<b>Family background:</b>			
Yearly parental family income (in \$1,000) ....	9.4 (10.2)	7.0 (6.9)	8.0 (8.6)
Missing parental income .....	.20 (.40)	.16 (.37)	.18 (.38)
No. of siblings .....	4.3 (2.7)	4.9 (3.1)	4.6 (3.0)
Parents' education (in years) .....	11.2 (2.8)	10.5 (2.9)	10.8 (2.9)
Intact family .....	.56 (.50)	.46 (.50)	.51 (.50)
Mother working .....	.48 (.50)	.46 (.50)	.47 (.50)

TABLE 1 (*Continued*)

Variable	3-6 Years	7-14 Years	Total
Contextual characteristics:			
County poverty rate .. . . .	.14 (.07)	.16 (.09)	.15 (.08)
State unemployment rate .. . . .	7.5 (3.0)	8.1 (3.3)	7.8 (3.1)
Welfare support (in \$1,000) .. . . .	.32 (.14)	.34 (.16)	.33 (.15)
Urban residence .. . . .	.80 (.40)	.81 (.39)	.81 (.39)
Southern residence .. . . .	.42 (.49)	.41 (.49)	.41 (.49)
<i>N</i> .. . . .	697	879	1,576
% of total <i>N</i> .. . . .	44.3	55.7	100

SOURCE.—NLSY, 1979-92

NOTE.—SDs are in parentheses

One-third of all single mothers were white. The proportion of whites among women who had longer single motherhood spells is much lower than for shorter spells (.25 and .43 respectively). The longer the spell, the lower is the woman's age at the start of it.<sup>18</sup> This suggests that a woman is less likely to leave single motherhood if she enters this state at a younger rather than an older age. Women in longer single motherhood spells have lower trainability scores than women with shorter spells, although both groups are in the lower half of the percentile rankings. Compared to women with shorter single motherhood spells, women in longer single motherhood spells have younger children, less child support, and are less likely to live with a partner. Women experiencing longer single motherhood are also more likely to come from poorer, larger, broken families. In addition, women with longer single motherhood are likely to live in a poor community and in an area with higher unemployment rates and AFDC benefits.

Although the explanatory variables show distributional differences between shorter and longer single motherhood spells, our analysis shows that the responsiveness to the explanatory variables does not differ by

<sup>18</sup> The relatively older ages for both groups are due to the fact that the observation period starts in 1979, truncating the previous years for the 15% of women who had become single mothers prior to that year.

length of spell (we tested for potential differential effects for shorter vs. longer lengths and found that these differences were close to zero and statistically insignificant). We therefore include in the final model only the variable indicating the years elapsed in single motherhood and exclude interaction terms.

Converted coefficients from the multinomial logit model are presented in table 2, which shows the effects on entry into and exit from productive activities, and table 4, which shows the effects on entry into and exit from kin coresidence.<sup>19</sup> The coefficients in table 2 represent the effects on productive activities in two-way transitions, taking into account simultaneously the effects on kin coresidence and the effects on the association between productive activities and coresidence. The coefficients represent the effects of the explanatory variables on the log odds of the transition. To calculate the change in the odds of the transition due to one unit increase in the explanatory variable, we use the formula ( $e^b - 1$ ). For example, consider the effect of kin coresidence on entry into productive activities (.138). This means that single mothers living with kin are 14.8% more likely to transit from nonproductive to productive activities than those who did not live with kin ( $[e^{.138} - 1] = .148$ ).

Examining the effects of endogenous variables in table 2, we find that kin coresidence in the immediate past period increases the probability of entering productive activities (.138 or a 14.8% increase compared to the noncoresidential situation), as predicted. This effect is statistically significant at the .05 level. Using data available in the NLSY, we also tested for the role of noncoresident kin by examining whether kin support measured in terms of financial transfers from these kin affect single mothers' entry into or exit from productive activities. The effect was close to zero and was not statistically significant. This offers additional support for our theoretical reasoning about why resident kin support should be important.

We explored the effect of coresident kin further by looking descriptively at the percentage of single mothers who were in coresidence households where at least one relative was in the labor force and by looking at the job search patterns of coresiding and noncoresiding single mothers. This information is available from the NLSY. Of single mothers coresiding with kin, 71% were in households where at least one kin member was gainfully employed. Furthermore, single mothers who lived with kin in 1981 were considerably more likely to be involved in a job search (34%) than those who did not coreside (21%); the gap is similar in 1982 (47% vs. 37%). More single mothers who lived with kin used friends or kin to

<sup>19</sup> Tables with the original multinomial logit estimates and converted coefficients are available on request from the authors.

TABLE 2

## SINGLE MOTHERS' ENTRY INTO AND EXIT FROM PRODUCTIVE ACTIVITIES

Variable	Entry	Exit
<b>Endogenous:</b>		
Kin coresidence at $t - 1$	.138*	-.111
	(2.16)	(1.81)
Kin coresidence at $t - 2$	.016	-.016
	(.57)	(.57)
<b>Exogenous:</b>		
<b>Individual characteristics:</b>		
White	-.007	.007
	(.67)	(.67)
Never married	-.037	.037
	(.64)	(.64)
Age	.001	-.075**
	(.05)	(4.74)
Education at first pregnancy	.001	-.001
	(.12)	(.12)
Trainability	.009**	-.004**
	(6.41)	(2.92)
No. of children, not coresident at $t - 1$	-.072	.009
	(1.15)	(.14)
No. of children, kin coresident at $t - 1$	-.164**	.101
	(2.69)	(1.30)
Age of youngest child	.002	-.002
	(.15)	(.15)
Yearly child support	.054**	-.054**
	(3.13)	(3.13)
Living with a partner	-.019	.019
	(1.11)	(1.11)
Time spent in single motherhood	-.034*	.034*
	(2.00)	(2.00)
Censored single motherhood	-.001	.001
	(.08)	(.08)
<b>Family background:</b>		
Yearly parental income	.016**	-.016**
	(4.06)	(4.06)
Missing parental income	.074	-.074
	(.99)	(.99)
No. of siblings	-.019*	.019*
	(2.05)	(2.05)
Parents' education	.008	-.008
	(.84)	(.84)
Intact family	.028	-.028
	(.56)	(.56)
Mother working	.112*	-.112*
	(2.38)	(2.38)



TABLE 2 (Continued)

Variable	Entry	Exit
Contextual characteristics:		
County poverty rate $\times 10$ .....	-.141*** (2.78)	.051 (.11)
State unemployment rate $\times 10$ .....	-.091 (1.20)	.030 (.34)
Welfare support .....	-.279 (1.15)	.219 (1.29)
Urban residence .....	.017 (.25)	.015 (.32)
Southern residence .....	.226** (3.23)	-.171** (3.37)
Constant .....	-1.911** (4.49)	1.701** (5.61)
Unmeasured individual heterogeneity* .....	.703*** (8.81)	-.703*** (8.81)

SOURCE —NLSY, 1979–92.

NOTE —Table shows the converted coefficients from the multinomial logit model. The absolute values of *t*-ratios are in parentheses.

\* Estimated using a nonparametric extreme value distribution, where two support points were used and tested as appropriate.

\*  $P < .05$

\*\*  $P < .01$ .

search for work than other single mothers (23% vs. 17% in 1981, with a similar gap in 1982 as well).

While kin coresidence seems to help single mothers start work or return to school, it does not play a significant role in helping them to *remain* productive. The effect of coresidence on exit from productive activities is in the predicted direction ( $-.111$  or a 10.5% decrease) but is not statistically significant.<sup>20</sup> This suggests that once a woman is enrolled in school or employed, it may be the job itself or the demands of the specific work organization or school that will have a more important impact than kin coresidence on whether she continues to be productive. We investigated the jobs of working females in the NLSY and found that, while 23% of women without children and 20% of married mothers have jobs in professional or managerial occupations, this is the case for only 13% of single mothers. In contrast, 21% of working women without children and 22%

<sup>20</sup> The estimate of the effect of kin coresidence on single mothers' exit from productive activities is not very precise (due to a large standard error). This reduces our ability to draw inferences about this effect. Future research should pursue a definitive test of this effect using other data sets. The effect on both entry into and exit from productive activities of kin coresidence beyond the immediate past state is small and insignificant.

of working married mothers are in service occupations, compared to 28% of working single mothers. When we further examined the occupations of women in different industries, we found that single mothers were more likely than other women to be in lower-status, lower-paying jobs within an industry. For example, among women employed in the "professional and related services" industry, 38% and 37% of married mothers and women without children, respectively, were in professional occupations; this contrasts with only 23% for single mothers. These findings suggest that the more unstable, lower-paying jobs that single mothers occupy may contribute to their exit, whether involuntary or voluntary, from productive activities. This is something on which kin support can have little effect.

Examining the effects of exogenous variables, we do not find important effects of race and marital history. Current public policy concerns focus especially on never-married single mothers. The majority of women in our sample never married (74%), and many of the ever-married women were once teenage mothers. But the insignificance of the coefficient for never- versus ever-married mothers indicates that the two groups respond to kin and welfare support in a similar way once other variables are controlled. This calls into question whether single mothers should be selected out by marital history in policy discussions. The insignificance of race also obviously argues for the disutility of emphasizing race—or the oft-accompanying intimations of different behavioral responses to various factors by race—in the policy debate (see also Wilson 1987). Older women are not necessarily more likely to start productive activities, but they are less likely to quit them. Educational level at first pregnancy is not important after taking into account trainability and individual heterogeneity. Trainability does show a significant effect on participation in productive activities, with greater effects on entry (.009 or a 9.4% increase for every 10 percentile points) than on exit (−.004 or a 4% decrease for every 10 percentile points). This suggests again that once enrolled in school or employed, the specific job or school will have a greater impact than human capital on whether women continue to be productive.

For single mothers in our sample, women living in adult kin's home with a larger number of children are less likely to enter productive activities (−.164, or a 15.1% decrease). Parents and kin may simply not have sufficient resources to lower the price of home time among single mothers with many children. The age of the youngest child has little effect on productive activities. This may reflect the fact that a large proportion of the women in our sample have preschool children, and there is, therefore, little variation in this variable. The amount of child support facilitates both entry into and the continuity of productive activities (.054 or a 5.5% increase per \$1,000). We interpret this as indicating a price-of-time effect.

In other words, some child care support should free up enough of women's time to make it more expensive *not* to be gainfully employed. Living with a partner, unlike living with a husband, has little effect on women's participation in productive activities. The longer the time elapsed in single motherhood, the less likely it is that a woman will enter into productive activities ( $-.034$  or 3.3% less likely per year) and the more likely it is that she will exit from them at the same speed. The effect of censored single motherhood differs very little from the effect of single motherhood that ended via marriage. Family background characteristics also influence single mothers' productive activities. In particular, higher parental family income (.016 or a 1.6% increase per \$1,000), fewer siblings ( $-.019$  or a 1.9% decrease per sibling), and mother's labor force participation (.112 or an 11.9% increase) promote both entry into and continuity of productive activities.

A poor economic environment that is socially isolated, as indicated by high county poverty rates, inhibits single mothers' engagement in productive activities ( $-.141$  or a 13.2% decrease per 10 percentage points). However, poverty rates have no significant effect on exit. Unemployment rates also have little effect on both entry and exit. Although previous research has shown that AFDC benefit levels have negative effects on low-income women's employment (e.g., Moffitt 1992), our estimate for the effect on entry is negative (positive on exit) but is not significant. This is consistent with research based on more recent data (Hao and Leibowitz 1994; Hao, Klerman, and Leibowitz 1995) that shows that AFDC benefits do not significantly prevent single mothers with young children from working. Turning to regional differences, single mothers living in the South are more likely than those living in other regions to engage in productive activities (.226 or 25.4% more likely), which is consistent with previous research (Moffitt 1992). Since our model includes the effects of state-level unemployment rates, county-level poverty rates, and urban-rural differences, residence in the South may be proxying stronger social control mechanisms in that region, which lead single mothers to engage in productive activities, although this is only a supposition.

Table 2 also shows that the effect of the unmeasured individual heterogeneity is strong and significant, indicating that unobserved characteristics such as innate ability and personality traits are important in influencing both entry into and exit from productive activities. This is evidence of the importance of controlling for individual heterogeneity. Table 3 further illustrates this point. Here, we compare the results from a model with no control for heterogeneity and a model with such a control (we present only variables whose magnitudes and significance levels have changed). Two types of change can be observed. The first type involves an increase in magnitude and the significance level of the estimates. These include the

TABLE 3

## SIGNIFICANT CHANGES IN EFFECTS ON PRODUCTIVE ACTIVITIES

VARIABLE	WITHOUT HETEROGENEITY		WITH HETEROGENEITY	
	Entry	Exit	Entry	Exit
Time spent in single motherhood .....	-.013	.013	-.034*	.034*
Trainability .....		-.002		-.004**
Education at first pregnancy .....	.094**	-.094**	.001	-.001
Censored single motherhood .....	-.184**	.184**	-.001	.001

SOURCE.—NLSY, 1979-92.

NOTE.—Only variables whose sign and significance level have changed are shown in the table.

\*  $P < .05$ \*\*  $P < .01$ .

effect of years elapsed in single motherhood on entry into productive activities (i.e., the estimate changes from  $-.013$  and statistically insignificant to  $-.034$ , statistically significant at the .05 level) and the effect of trainability on exit (from  $-.002$ , insignificant to  $-.004$ , significant at the .01 level). The second type of change is where a decrease in magnitudes and significance levels occurs. These changes include the effect of education at first pregnancy and the effect of censored single motherhood on entry into productive activities or exit from them. In sum, introducing an individual-specific component into the model does produce a change in the estimates for the effects of certain variables, but it does not change the effect of kin coresidence.

We posited that kin coresidence should facilitate the employment or school enrollment of single mothers. But we acknowledge that kin coresidence may instead be a response to productive activities—a way chosen by single mothers to cope with the time demands of schooling or a job. Our model tests this alternative simultaneously (see table 4). Productive activities either in the immediate prior period or before do not have a significant impact on the transition into or out of a coresidential situation. This supports the position that kin coresidence is not necessarily a response to the needs of single mothers who are already engaged in productive activities. Rather, living with kin, through its normative supervision and encouragement, potential in-kind transfers (room and board, child care assistance, and housework assistance), and possible role modeling and social access effects of labor market attachment appear to promote single mothers' productive activities.

Table 4 shows that kin coresidence is determined by a number of fac-

TABLE 4

## SINGLE MOTHERS' ENTRY INTO AND EXIT FROM KIN CORESIDENCE

Variable	Entry	Exit
Endogenous:		
Productive activities at $t - 1$	.162 (.77)	-.151 (.74)
Productive activities at $t - 2$	-.034 (1.41)	.034 (1.41)
Exogenous:		
Individual characteristics:		
White	.005 (.47)	-.005 (.47)
Never married	.153** (2.67)	-.153** (2.67)
Age:		
Not productive at $t - 1$	.001 (.03)	-.001 (.03)
Productive at $t - 1$	-.043** (2.72)	.043** (2.72)
Education at first pregnancy	.012 (1.90)	-.012 (1.90)
Trainability:		
Not productive at $t - 1$	-.003* (2.07)	.003* (2.07)
Productive at $t - 1$	-.003 (1.94)	.003 (1.94)
No. of children:		
Not productive at $t - 1$	-.258** (4.13)	.156** (2.58)
Productive at $t - 1$	-.070 (1.13)	-.032 (.47)
Age of youngest child	-.002 (.14)	.002 (.14)
Yearly child support	.032 (1.24)	-.032 (1.24)
Living with a partner	-.030 (1.67)	.030 (1.67)
Time spent in single motherhood	-.026 (1.45)	.026 (1.45)
Censored single motherhood	.024 (1.43)	.024 (1.43)
Family background:		
Yearly parental family income	.002 (.64)	-.002 (.64)
Missing parental income	-.164* (2.50)	.164* (2.50)
No. of siblings	-.004 (.47)	.004 (.47)

TABLE 4 (*Continued*)

Variable	Entry	Exit
Parents' education . . . . .	-.003 (.40)	.003 (.40)
Intact family . . . . .	.054 (1.26)	-.054 (1.26)
Mother working . . . . .	.052 (1.24)	-.052 (1.24)
Contextual characteristics:		
County poverty rate $\times 10$		
Not productive at $t - 1$ . . . . .	.100 (.20)	-.100 (.20)
Productive at $t - 1$ . . . . .	.478 (.95)	-.478 (.95)
State unemployment rate $\times 10$ . . . . .	-.019* (2.43)	.019* (2.43)
Welfare support . . . . .	.067 (.30)	-.067 (.30)
Urban residence . . . . .	.062 (.96)	-.062 (.96)
Southern residence . . . . .	.112 (1.64)	-.112 (1.64)
Constant . . . . .	-.119 (.29)	.119 (.29)
Unmeasured individual heterogeneity* . . . . .	.135 (1.37)	-.135 (1.37)

Source: NLSY, 1979-92.

NOTE: Table shows the converted coefficients from the multinomial logit model. The absolute values of  $t$ -ratios are in parentheses.

\* Estimated using a nonparametric extreme value distribution, where two support points were used and tested as appropriate.

\*  $P < .05$ .\*\*  $P < .01$ .

tors. First, never-married women, who have fewer economic resources than their ever-married counterparts, are more likely to coreside with parents and kin (.153 or 16.5% more likely). Second, older women who were engaged in productive activities in the past year are less likely to live with kin (-.043 or 4.2% less likely), indicating that coresidence is an age-sensitive phenomenon, particularly when the woman can support herself. Third, women with higher trainability when nonproductive are less likely to move into their parents' home (-.003 or a 3% decrease per 10 percentile points), perhaps because they anticipate a greater ability to deal with their dual role problems. Fourth, women with more children who are also nonproductive are less likely to coreside with parents (-.258 or 22.7% less

TABLE 5

SIGNIFICANT CHANGES IN EFFECTS ON KIN CORESIDENCE DUE TO INCLUSION OF HETEROGENEITY

VARIABLE	WITHOUT HETEROGENEITY		WITH HETEROGENEITY	
	Entry	Exit	Entry	Exit
Never married . . . . .	.064	-.064	.153**	-.153**
White . . . . .	-.228**	.228**	.005	-.005
Age, nonproductive at $t - 1$ . . . . .	-.026*	.026**	.001	-.001
Censored single motherhood . . . . .	.193**	-.193**	.024	-.024
Southern residence . . . . .	.119*	-.119*	.112	-.112

SOURCE.—NLSY, 1979-92.

NOTE.—Only variables whose sign and significance level have changed are shown in the table.

\*  $P < .05$ \*\*  $P < .01$ 

likely per child). This may be because parents and kin simply do not have unlimited resources to support a large family, particularly when this family has no income. Fifth, missing parental income, which indicates the absence of parents or parents' unwillingness to report income, lowers the likelihood of coresidence ( $-.164$  or a 15.1% decrease). Sixth, higher unemployment rates, depicting a difficult economic environment, inhibit parents and kin from providing coresident help ( $-.019$  or a 1.9% decrease per 10 percentage points). Finally, we note that the amount of child support does not have a statistically significant effect on coresidence. This suggests that coresidence cannot be regarded as a simple substitute for the provision of child care funds.

The message from table 4 is that, when all the exogenous factors are taken into account, demands from single mothers' participation in productive activities do not lead to kin coresidence. Including kin coresidence as an endogenous variable in the model offers a clear test that the possibility of a causal link from productive activities to kin coresidence can be eliminated.

Unmeasured heterogeneity has no significant effect on kin coresidence, but including it in the model does change some of the estimates (see table 5). The never-married effect on entry into kin coresidence increases from .064 (statistically insignificant) to .153 (significant at the .01 level). Conversely, decreases in magnitude and significance levels occur for four variables: race (the effect of being white), age while nonproductive, censored single motherhood, and residence in the South.

## CONCLUSION

Our analysis reveals a number of significant findings regarding the conditions of single motherhood and the implications for single mothers' participation in gainful employment or in schooling. The younger a woman is when she becomes a single mother, the longer she is likely to remain in single motherhood. Women with the longest single motherhood spells are also the most likely to come from large, poor families that were not intact when the woman was a teenager. These results underline the significance of the social and economic context from which a woman enters and remains in single motherhood. Moreover, our causal model reveals that the longer a woman spends in single motherhood, the less likely she is to enter productive activities and the more likely she is to exit from them once she starts. Kin coresidence substantially increases the likelihood that single mothers will move from nonproductive to productive activities. Its effect is comparable to the effect of other important factors. For example, the effect of kin coresidence is equivalent to the effect from a \$2,556 increase in child support per year. Similarly, kin coresidence's effect is equivalent to the effect of an increase of 15.3 percentile points in mothers' individual trainability, or that of an 11.4% decrease in county-level poverty rate.

A number of policy implications stem from the results of our models, and these concern what does and does not exert a significant effect on single mothers' participation in productive activities. Our confidence in these results and the policy implications is strengthened by the ability of our dynamic model to take into account the potential reverse causal effect of kin coresidence and the endogeneity of kin coresidence.

The first policy implication is that, although kin support in the form of coresidence does indeed promote single mothers' *entry* into productive activities, there is no strong evidence from our data supporting the idea that kin coresidence can prevent single mothers from *exiting* such activities.<sup>21</sup> In other words, kin coresidence alone cannot sustain single mothers' productive activities. We interpret the lack of a significant effect on sustained productive activities as a possible reflection of the greater importance of two sets of factors. The first is the demand side of the labor market. The characteristics of the jobs that single mothers are able to find and of the firms where they work most likely have an important effect on how long they can keep their jobs. Single mothers are less likely than either women without children or married mothers to hold jobs in the

<sup>21</sup> This finding echoes a recent study of public support (JOBS) showing that welfare recipients who volunteered for the job training program had only a 50% chance of staying employed through the first year after training (Shogren 1994).



more stable, high-skill sector of the labor market. Second, the responsibilities of single parenthood coupled with the lack of a reliable child care system may prevent single mothers from keeping their jobs. For example, if a child becomes ill while the mother is still in a probationary period on her job, missing work to care for the sick child or to take the child to the doctor may jeopardize her job status. Even for mothers who are beyond the probationary period, intensive care for children may force them to quit their jobs. While coresident kin may provide assistance as well as encouragement for a single mother to enter into productive activities, it is after this occurs that the child care burden falls on coresident kin; they may not be able to provide full-blown, day-to-day assistance, especially during difficult times such as when a child falls ill. Unavailability of child care and its high cost plus the responsibility of bringing up a child as a single parent may be the two most important labor supply-side reasons why single mothers leave the labor force.

Although the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 offers a combination of job training, time limits on welfare, work requirements, child care subsidies, and child support enforcement to single mothers in order to encourage them to be independent, exactly how these measures are to be implemented is not clear. Under the Family Support Act of 1988, child care arrangements for working AFDC recipients have often been inadequate and unstable. Notably, our results show that the amount of child support exerts a significant positive effect on single mothers' probability of entering into and remaining in productive activities. Child support is for the care and well-being of the child. While we did not directly assess the effect of public child care subsidies, our finding about child support not only speaks for the importance of regulating child support but also implies that more adequate and stable public child care assistance can have a significant effect on the probability of drawing single mothers into productive activities and maintaining their presence there.

A second set of findings is in regard to the determinants of kin coresidence itself. Our results show that the probability of coresidence is not affected by the amount of child support. In other words, we find no basis on which to argue that coresidence should be regarded as a natural substitute for monetary child support. Rather, coresidence is a multidimensional situation that may provide single mothers with adult working role models and encouragement to work or resume education. As such, it may be well to explore ways of encouraging coresidence but the conclusion that it should be mandated is not supported by our findings.

Third, the positive effect of trainability on single mothers' entrance into and continuation of productive activities merits attention. Obviously,

greater attention needs to be devoted to what types of programs do and do not enhance individuals' long-term trainability, particularly the ability to learn more advanced skills while on the job.

Fourth, even though the effect of welfare support on single mothers' productive activities is negative, the effect is not statistically significant. This is probably due to the fact that welfare does not penalize schooling, which we included in our analysis as an important productive activity. By using a more complete model that considers both welfare and kin support in a dynamic framework, we generate a more precise estimate for the welfare effect. This is particularly useful if we are concerned with the self-sufficiency and productivity of single mothers. Our findings suggest that less time should be spent on acrimonious debates about the level of welfare payments and more emphasis should be put instead on determining the most useful combinations of assistance, including child care assistance.

Fifth, neither race nor never-married versus ever-married status has a significant effect on either entry into or exit from productive activities in our model. This finding merits attention because policy debates are so often underlaid by assumptions about differential behavioral responses based on race and never-married status.

Finally, our findings point to the importance of environmental factors in promoting or discouraging young single mothers' participation in productive activities outside the home. Living in an area of high poverty has a dampening effect. Wilson (1987) points out that joblessness and social isolation in inner-city areas are linked to long-term welfare dependency and other social problems. Because of their low income, single mothers often live in grim economic and social environments. Our analysis shows that the likelihood of entering productive activities is greater when local economic and social conditions are better. Furthermore, kin coresidence is more likely when unemployment rates are lower. These findings argue for the importance of policies designed to generate greater local employment opportunities that benefit not only single mothers and their children but their communities as well. This is obviously a formidable challenge, but one reinforced by the findings of this article.

## APPENDIX

### Technical Note

The purpose of this appendix is to show the relationships between multinomial logit parameters and parameters that fit the two endogenous variables under the model Yamaguchi (1990) advanced. This method should make it easier to model reciprocal causality using existing statisti-

TABLE A1  
CROSS-CLASSIFICATION OF TWO  
VARIABLES

A	B	
	-1	1
-1 . . . . .	$P_{00}$	$P_{01}$
1 . . . . .	$P_{10}$	$P_{11}$

cal packages. The method converts multinomial logit parameters into parameters of interest and their standard errors.

To begin, suppose we have two dichotomous random variables  $A$  and  $B$ , which take the value of either  $-1$  or  $1$ . Cross-classifying the two variables, we have four categories (see table A1), where  $P_{00} = \text{prob}(A = -1 \text{ and } B = -1)$ ,  $P_{01} = \text{prob}(A = -1 \text{ and } B = 1)$ ,  $P_{10} = \text{prob}(A = 1 \text{ and } B = -1)$ , and  $P_{11} = \text{prob}(A = 1 \text{ and } B = 1)$ .

For simplicity, we have two covariates,  $X_1$  and  $X_2$ , and  $\delta$  stands for their corresponding parameters. A multinomial logit specification is, then,

$$\begin{aligned}\log\left(\frac{P_{01}}{P_{00}}\right) &= \delta_{20} + \delta_{21}X_1 + \delta_{22}X_2, \\ \log\left(\frac{P_{10}}{P_{00}}\right) &= \delta_{30} + \delta_{31}X_1 + \delta_{32}X_2, \\ \log\left(\frac{P_{11}}{P_{00}}\right) &= \delta_{40} + \delta_{41}X_1 + \delta_{42}X_2.\end{aligned}\tag{A1}$$

The outcome ( $A = -1$  and  $B = -1$ ) is the reference category, and its parameters,  $\delta_i$ , are constrained to zero.

Let  $\mu$ ,  $\phi^A$ ,  $\phi^B$ , and  $\phi^{AB}$  represent a set of parameters that is defined as the following:

$$\begin{aligned}\log P_{00} &= \mu - \phi^A - \phi^B + \phi^{AB}, \\ \log P_{01} &= \mu - \phi^A + \phi^B - \phi^{AB}, \\ \log P_{10} &= \mu + \phi^A - \phi^B - \phi^{AB}, \\ \log P_{11} &= \mu + \phi^A + \phi^B + \phi^{AB}.\end{aligned}\tag{A2}$$

Through algebraic manipulation of equations in (A2), we get

$$\begin{aligned} 4\phi^A &= -\log\left(\frac{P_{01}}{P_{\infty}}\right) + \log\left(\frac{P_{10}}{P_{\infty}}\right) + \log\left(\frac{P_{11}}{P_{\infty}}\right), \\ 4\phi^B &= \log\left(\frac{P_{01}}{P_{\infty}}\right) - \log\left(\frac{P_{10}}{P_{\infty}}\right) + \log\left(\frac{P_{11}}{P_{\infty}}\right), \\ 4\phi^{AB} &= -\log\left(\frac{P_{01}}{P_{\infty}}\right) - \log\left(\frac{P_{10}}{P_{\infty}}\right) + \log\left(\frac{P_{11}}{P_{\infty}}\right). \end{aligned} \quad (A3)$$

The set of parameters to explain  $A$ ,  $B$ , and their association,  $AB$ , is a function of the covariates,  $X_1$  and  $X_2$ :

$$\begin{aligned} 4\phi^A &= 4\lambda_0^A + 4\lambda_1^A X_1 + 4\lambda_2^A X_2, \\ 4\phi^B &= 4\lambda_0^B + 4\lambda_1^B X_1 + 4\lambda_2^B X_2, \\ 4\phi^{AB} &= 4\lambda_0^{AB} + 4\lambda_1^{AB} X_1 + 4\lambda_2^{AB} X_2. \end{aligned} \quad (A4)$$

Substituting equations (A1) and (A4) into equation (A3), we can solve for the parameters corresponding to each covariate. For example, for the parameters corresponding to  $X_1$ , we have equation (A5), where the parameters that fit  $A$ ,  $B$ , and their association  $AB$  can be expressed as linear combinations of the multinomial logit parameters:

$$\begin{aligned} 4\lambda_1^A &= -\delta_{21} + \delta_{31} + \delta_{41}, \\ 4\lambda_1^B &= \delta_{21} - \delta_{31} + \delta_{41}, \\ 4\lambda_1^{AB} &= -\delta_{21} - \delta_{31} + \delta_{41}. \end{aligned} \quad (A5)$$

Since the variance of a linear combination of random variables is the sum of the variance of each variable and twice the covariance among them (adjusting for the scaling and the signs), the corresponding variance for a particular parameter can be expressed as linear combinations of elements in the variance-covariance matrix of the multinomial logit estimation. For the variances of parameters corresponding to  $X_1$ , we get

$$\begin{aligned} \text{var}(4\lambda_1^A) &= \text{var}(\delta_{21}) + \text{var}(\delta_{31}) + \text{var}(\delta_{41}) - 2\text{cov}(\delta_{21}, \delta_{31}) \\ &\quad - 2\text{cov}(\delta_{21}, \delta_{41}) + 2\text{cov}(\delta_{31}, \delta_{41}), \\ \text{var}(4\lambda_1^B) &= \text{var}(\delta_{21}) + \text{var}(\delta_{31}) + \text{var}(\delta_{41}) - 2\text{cov}(\delta_{21}, \delta_{31}) \\ &\quad + 2\text{cov}(\delta_{21}, \delta_{41}) - 2\text{cov}(\delta_{31}, \delta_{41}), \\ \text{var}(4\lambda_1^{AB}) &= \text{var}(\delta_{21}) + \text{var}(\delta_{31}) + \text{var}(\delta_{41}) + 2\text{cov}(\delta_{21}, \delta_{31}) \\ &\quad - 2\text{cov}(\delta_{21}, \delta_{41}) - 2\text{cov}(\delta_{31}, \delta_{41}). \end{aligned} \quad (A6)$$

So far, we have established the relationships between the multinomial logit parameters and the parameters that fit the two endogenous variables. This model considers only one point in time. Note that this model cannot handle the reciprocal causal relationship between the two endogenous variables. Below, we introduce the time dimension and specify the model in the longitudinal framework. By including the lagged endogenous variables on the right-hand side of the equation, we will be able to model the reciprocal cross-lagged causation.

Consider a single endogenous variable in a longitudinal framework first. Repeated measures of the endogenous variable  $A$  for each individual over time describe an endogenous process  $A_t$ , which takes a state ( $-1$  or  $1$ ) at time  $t$ , where  $t = 1, \dots, T$ . To deal with the initial condition, Chamberlain (1978) suggests conditioning on the state at  $t = 1$ . Within the process, two-way transitions are possible: from  $A_{t-1} = -1$  to  $A_t = 1$  and from  $A_{t-1} = 1$  to  $A_t = -1$ . To consider state dependence, we specify a first-order state dependence,  $A_{t-1}$ . (We also include the second-order state dependence,  $A_{t-2}$ , which captures the duration effect beyond the immediate state in the article.)

When we consider two endogenous processes simultaneously, we have two endogenous processes,  $A_t$  and  $B_t$ . We add to the model the effects of  $B_{t-1}$  on  $B_t$ . For  $B_t$ , we similarly have two-way transitions: from  $B_{t-1} = -1$  to  $B_t = 1$  and from  $B_{t-1} = 1$  to  $B_t = -1$ . We will first assume that the effects are equal in magnitude (but opposite in sign) for both ways of transitions and then relax this assumption later.

When we specify the dynamic model as a function of the lagged state of each of the two endogenous processes, we at the same time specify the cross-lagged reciprocal causal relationships between  $A_t$  and  $B_t$  (i.e., the model includes the effect of  $B_{t-1}$  on  $A_t$ , and the effect of  $A_{t-1}$  on  $B_t$ ). The model extends equation (A4) to the following:

$$\begin{aligned} 4\phi^{A_t} &= 4\lambda_0^A + 4\lambda_1^A A_{t-1} + 4\lambda_2^A B_{t-1} + 4\lambda_3^A X_1 + 4\lambda_4^A X_2, \\ 4\phi^{B_t} &= 4\lambda_0^B + 4\lambda_1^B A_{t-1} + 4\lambda_2^B B_{t-1} + 4\lambda_3^B X_1 + 4\lambda_4^B X_2, \\ 4\phi^{AB_t} &= 4\lambda_0^{AB} + 4\lambda_1^{AB} A_{t-1} + 4\lambda_2^{AB} B_{t-1} + 4\lambda_3^{AB} X_1 + 4\lambda_4^{AB} X_2, \end{aligned} \quad (A7)$$

where  $\lambda_1^A$  is the first-order state dependence for  $A_t$ ,  $\lambda_2^A$  is the lagged causal effect of  $B_{t-1}$  on  $A_t$ ,  $\lambda_3^A$  and  $\lambda_4^A$  are the parameters related to the two covariates. Similarly,  $\lambda_1^B$  is the lagged causal effect of  $A_{t-1}$  on  $B_t$ ,  $\lambda_2^B$  is the first-order state-dependence for  $B_t$ ,  $\lambda_3^B$  and  $\lambda_4^B$  are the parameters related to the two covariates. The parameters for the association between  $A_t$  and  $B_t$  are expressed by  $\lambda^{AB}$ .

To specify the multinomial logit model for cross-lagged reciprocal causal relationship between two endogenous variables, we cross-classify

$A_t$  and  $B_t$  obtain a four-category dependent variable. The multinomial logit specification for the two interdependent processes is shown below:

$$\begin{aligned}\log\left(\frac{P_{01}}{P_{00}}\right) &= \delta_{20} + \delta_{21}A_{t-1} + \delta_{22}B_{t-1} + \delta_{23}X_1 + \delta_{24}X_2, \\ \log\left(\frac{P_{10}}{P_{00}}\right) &= \delta_{30} + \delta_{31}A_{t-1} + \delta_{32}B_{t-1} + \delta_{33}X_1 + \delta_{34}X_2, \\ \log\left(\frac{P_{11}}{P_{00}}\right) &= \delta_{40} + \delta_{41}A_{t-1} + \delta_{42}B_{t-1} + \delta_{43}X_1 + \delta_{44}X_2.\end{aligned}\quad (\text{A8})$$

According to equation (A5), we can express the parameters in equation (A7) as linear combinations of the multinomial logit parameters in equation (A8). By the same token, the standard errors of the parameters in equation (A7) can be obtained using equation (A6). For example, the lagged causal effect of  $B_{t-1}$  on  $A_t$  is  $\lambda_2^A$ , which can be expressed as:

$$4\lambda_2^A = -\delta_{22} + \delta_{32} + \delta_{42},$$

and its variance is

$$\begin{aligned}\text{var}(4\lambda_2^A) &= \text{var}(\delta_{22}) + \text{var}(\delta_{32}) + \text{var}(\delta_{42}) - 2\text{cov}(\delta_{22}, \delta_{32}) \\ &\quad - 2\text{cov}(\delta_{22}, \delta_{42}) + 2\text{cov}(\delta_{32}, \delta_{42}).\end{aligned}$$

A more rigorous modeling is to relax the assumption that the effects on the two ways of transitions are equal in magnitude. Interactions between the two lagged endogenous variables will capture the differential effects for the two ways of transitions (see Yamaguchi 1990). For simplicity, we suppress the covariates. The reciprocal causal model for  $A_t$  and  $B_t$  is

$$\begin{aligned}\phi^A_t &= \lambda_0^A + \lambda_1^A A_{t-1} + \lambda_2^A B_{t-1} + \lambda_3^A A_{t-1} B_{t-1}, \\ \phi^B_t &= \lambda_0^B + \lambda_1^B A_{t-1} + \lambda_2^B B_{t-1} + \lambda_3^B A_{t-1} B_{t-1}.\end{aligned}\quad (\text{A9})$$

The corresponding multinomial logit specification is

$$\begin{aligned}\log\left(\frac{P_{01}}{P_{00}}\right) &= \delta_{20} + \delta_{21}A_{t-1} + \delta_{22}B_{t-1} + \delta_{23}A_{t-1}B_{t-1}, \\ \log\left(\frac{P_{10}}{P_{00}}\right) &= \delta_{30} + \delta_{31}A_{t-1} + \delta_{32}B_{t-1} + \delta_{33}A_{t-1}B_{t-1}, \\ \log\left(\frac{P_{11}}{P_{00}}\right) &= \delta_{40} + \delta_{41}A_{t-1} + \delta_{42}B_{t-1} + \delta_{43}A_{t-1}B_{t-1}.\end{aligned}\quad (\text{A10})$$

For the transition from  $A_{t-1} = -1$  to  $A_t = 1$ , the lagged effect of  $B_{t-1}$  is  $(\lambda_2^A - \lambda_3^A)$ , for the transition from  $A_{t-1} = 1$  to  $A_t = -1$ , the lagged effect of  $B_{t-1}$  is  $-(\lambda_2^A + \lambda_3^A)$ , and they can be expressed as linear combinations of the multinomial logit parameters. For example,  $(\lambda_2^A - \lambda_3^A)$  can be expressed as

$$\begin{aligned} 4(\lambda_2^A - \lambda_3^A) &= (-\delta_{22} + \delta_{32} + \delta_{42}) - (-\delta_{23} + \delta_{33} + \delta_{43}), \\ &= -\delta_{22} + \delta_{32} + \delta_{42} + \delta_{23} - \delta_{33} - \delta_{43}, \end{aligned} \quad (\text{A11})$$

and the variance can be expressed as

$$\begin{aligned} \text{var}[4(\lambda_2^A - \lambda_3^A)] &= \text{var}(\delta_{22}) + \text{var}(\delta_{32}) + \text{var}(\delta_{42}) + \text{var}(\delta_{23}) \\ &\quad + \text{var}(\delta_{33}) + \text{var}(\delta_{43}) - 2\text{cov}(\delta_{22}, \delta_{32}) \\ &\quad - 2\text{cov}(\delta_{22}, \delta_{42}) - 2\text{cov}(\delta_{22}, \delta_{23}) + 2\text{cov}(\delta_{22}, \delta_{33}) \\ &\quad + 2\text{cov}(\delta_{22}, \delta_{43}) + 2\text{cov}(\delta_{32}, \delta_{42}) + 2\text{cov}(\delta_{32}, \delta_{23}) \quad (\text{A12}) \\ &\quad - 2\text{cov}(\delta_{32}, \delta_{33}) - 2\text{cov}(\delta_{32}, \delta_{43}) + 2\text{cov}(\delta_{42}, \delta_{23}) \\ &\quad - 2\text{cov}(\delta_{42}, \delta_{33}) - 2\text{cov}(\delta_{42}, \delta_{43}) - 2\text{cov}(\delta_{23}, \delta_{33}) \\ &\quad - 2\text{cov}(\delta_{23}, \delta_{43}) + 2\text{cov}(\delta_{33}, \delta_{43}). \end{aligned}$$

In the same manner, one can obtain the effect of  $B_{t-1}$  on the transition from  $A_{t-1} = 1$  to  $A_t = -1$ ,  $-(\lambda_2^A + \lambda_3^A)$ . Similarly, to obtain the differential causal effects of  $A_{t-1}$  on  $B_t$ , we refer to equation (A5) with respect to  $\lambda^B$ , and solve for  $(\lambda_1^B - \lambda_3^B)$  and  $-(\lambda_1^B + \lambda_3^B)$ .

We could also specify the differential effects of a covariate for one endogenous process by introducing the interaction between the covariate and the lagged endogenous variable. Suppose  $X_1$  has differential effects for the endogenous process  $A_t$ . Suppressing the effect of  $B_{t-1}$  and  $X_2$ , the model for  $A_t$  and  $B_t$  is

$$\begin{aligned} \phi^{A_t} &= \lambda_0^A + \lambda_1^A A_{t-1} + \lambda_2^A X_1 + \lambda_3^A A_{t-1} X_1, \\ \phi^B &= \lambda_0^B + \lambda_1^B A_{t-1} + \lambda_2^B X_1 + \lambda_3^B A_{t-1} X_1. \end{aligned} \quad (\text{A13})$$

And the corresponding multinomial logit model is

$$\begin{aligned} \log\left(\frac{P_{01}}{P_{00}}\right) &= \delta_{20} + \delta_{21} A_{t-1} + \delta_{22} X_1 + \delta_{23} A_{t-1} X_1, \\ \log\left(\frac{P_{10}}{P_{00}}\right) &= \delta_{30} + \delta_{31} A_{t-1} + \delta_{32} X_1 + \delta_{33} A_{t-1} X_1, \\ \log\left(\frac{P_{11}}{P_{00}}\right) &= \delta_{40} + \delta_{41} A_{t-1} + \delta_{42} X_1 + \delta_{43} A_{t-1} X_1. \end{aligned} \quad (\text{A14})$$

For the transition from  $A_{t-1} = -1$  to  $A_t = 1$ , the effect of  $X_1$  is  $(\lambda_2^A - \lambda_3^A)$ , for the transition from  $A_{t-1} = 1$  to  $A_t = -1$ , the effect of  $X_1$  is  $-(\lambda_1^A + \lambda_3^A)$ , and they can be expressed as linear combinations of the multinomial logit parameters, exactly the same as for the reciprocal causal effects of  $B_{t-1}$  on  $A_t$ , as we laid out in equations (A11) and (A12).

The effects of  $X_1$  on  $B_t$ , as expressed in the  $\phi^B$  function, however, are NOT the differential effects for the two-way transitions of  $B_t$ , but the differential effect conditional on  $A_{t-1}$ , be it  $-1$  or  $1$ . Conditional on  $A_{t-1} = -1$ , the effect of  $X_1$  on  $B_t$  is  $(\lambda_1^B - \lambda_3^B)$ . Conditional on  $A_{t-1} = 1$ , the effect of  $X_1$  on  $B_t$  is  $(\lambda_2^B + \lambda_3^B)$ . Note that these conditional effects are identical in magnitude (but opposite in sign) for the two ways of transitions of  $B_t$ . Referring to equations (A11) and (A12), readers can easily write out these effects as linear combinations of multinomial logit parameter and the variance.

The last relaxation of the assumption is to allow differential effects of covariates for both endogenous processes. We extend equation (A13) into the following:

$$\begin{aligned}\phi^A &= \lambda_0^A + \lambda_1^A A_{t-1} + \lambda_2^A B_{t-1} + \lambda_3^A X_1 + \lambda_4^A A_{t-1} X_1 + \lambda_5^A B_{t-1} X_1, \\ \phi^B &= \lambda_0^B + \lambda_1^B A_{t-1} + \lambda_2^B B_{t-1} + \lambda_3^B X_1 + \lambda_4^B A_{t-1} X_1 + \lambda_5^B B_{t-1} X_1,\end{aligned}\quad (\text{A15})$$

and the corresponding multinomial logit model becomes

$$\begin{aligned}\log\left(\frac{P_{01}}{P_{\infty}}\right) &= \delta_{00} + \delta_{21} A_{t-1} + \delta_{22} B_{t-1} + \delta_{23} X_1 \\ &\quad + \delta_{24} A_{t-1} X_1 + \delta_{25} B_{t-1} X_1, \\ \log\left(\frac{P_{10}}{P_{\infty}}\right) &= \delta_{10} + \delta_{31} A_{t-1} + \delta_{32} B_{t-1} + \delta_{33} X_1 \\ &\quad + \delta_{34} A_{t-1} X_1 + \delta_{35} B_{t-1} X_1, \\ \log\left(\frac{P_{11}}{P_{\infty}}\right) &= \delta_{40} + \delta_{41} A_{t-1} + \delta_{42} B_{t-1} + \delta_{43} X_1 \\ &\quad + \delta_{44} A_{t-1} X_1 + \delta_{45} B_{t-1} X_1.\end{aligned}\quad (\text{A16})$$

In this setup, the differential effects of a covariate for the two ways of transitions of one endogenous variable are also conditional on the lagged state of the other endogenous variable. Table A2 presents these conditional, differential effects more clearly.

We can express these conditional, differential effects of  $X_1$  as linear combinations of the multinomial logit parameters, and their variances lin-



TABLE A2  
CONDITIONAL DIFFERENTIAL EFFECTS

	-1	1
Effects of $X_{i-1}$ on $A_i$ , conditional on $B_{i-1}$ :		
From $A_{i-1} = -1$ to $A_i = 1$ .....	$\lambda_3^A - \lambda_4^A - \lambda_5^A$	$\lambda_1^A - \lambda_4^A + \lambda_5^A$
From $A_{i-1} = 1$ to $A_i = -1$ .....	$-(\lambda_3^A + \lambda_4^A - \lambda_5^A)$	$-(\lambda_1^A + \lambda_4^A + \lambda_5^A)$
Effects of $X_{i-1}$ on $B_i$ , conditional on $A_{i-1}$ :		
From $B_{i-1} = -1$ to $B_i = 1$ .....	$\lambda_3^B - \lambda_4^B - \lambda_5^B$	$\lambda_1^B + \lambda_4^B - \lambda_5^B$
From $B_{i-1} = 1$ to $B_i = -1$ .....	$-(\lambda_3^B - \lambda_4^B + \lambda_5^B)$	$-(\lambda_1^B + \lambda_4^B + \lambda_5^B)$

ear combinations of the elements of the variance-covariance matrix of the multinomial logit estimation. Although the expressions are long, the logic behind them is simple and the calculations should be straightforward. Take the effect of  $X_1$  on the transition from  $A_{i-1} = -1$  to  $A_i = 1$ , conditional on  $B_{i-1} = -1$ ,  $(\lambda_3^A - \lambda_4^A - \lambda_5^A)$ , as an example:

$$\begin{aligned}
 4(\lambda_3^A - \lambda_4^A - \lambda_5^A) &= (-\delta_{23} + \delta_{33} + \delta_{43}) - (-\delta_{24} + \delta_{34} + \delta_{44}) \\
 &\quad - (-\delta_{25} + \delta_{35} + \delta_{45}), \\
 &= -\delta_{23} + \delta_{33} + \delta_{43} + \delta_{24} - \delta_{34} - \delta_{44} \\
 &\quad + \delta_{25} - \delta_{35} - \delta_{45},
 \end{aligned} \tag{A17}$$

and the variance is

$$\begin{aligned}
 \text{var}[4(\lambda_3^A - \lambda_4^A - \lambda_5^A)] &= \text{var}(\delta_{23}) + \text{var}(\delta_{33}) + \text{var}(\delta_{43}) + \text{var}(\delta_{24}) + \text{var}(\delta_{34}) \\
 &\quad + \text{var}(\delta_{25}) + \text{var}(\delta_{35}) + \text{var}(\delta_{45}) - 2\text{cov}(\delta_{23}, \delta_{33}) \\
 &\quad - 2\text{cov}(\delta_{23}, \delta_{43}) - 2\text{cov}(\delta_{23}, \delta_{24}) + 2\text{cov}(\delta_{23}, \delta_{34}) + 2\text{cov}(\delta_{23}, \delta_{44}) \\
 &\quad - 2\text{cov}(\delta_{23}, \delta_{25}) + 2\text{cov}(\delta_{23}, \delta_{35}) + 2\text{cov}(\delta_{23}, \delta_{45}) + 2\text{cov}(\delta_{33}, \delta_{43}) \\
 &\quad + 2\text{cov}(\delta_{33}, \delta_{24}) - 2\text{cov}(\delta_{33}, \delta_{34}) - 2\text{cov}(\delta_{33}, \delta_{44}) + 2\text{cov}(\delta_{33}, \delta_{25}) \\
 &\quad - 2\text{cov}(\delta_{33}, \delta_{35}) - 2\text{cov}(\delta_{33}, \delta_{45}) + 2\text{cov}(\delta_{43}, \delta_{24}) - 2\text{cov}(\delta_{43}, \delta_{34}) \\
 &\quad - 2\text{cov}(\delta_{43}, \delta_{44}) + 2\text{cov}(\delta_{43}, \delta_{25}) - 2\text{cov}(\delta_{43}, \delta_{35}) - 2\text{cov}(\delta_{43}, \delta_{45}) \\
 &\quad - 2\text{cov}(\delta_{24}, \delta_{34}) - 2\text{cov}(\delta_{24}, \delta_{44}) + 2\text{cov}(\delta_{24}, \delta_{25}) - 2\text{cov}(\delta_{24}, \delta_{35}) \\
 &\quad - 2\text{cov}(\delta_{24}, \delta_{45}) + 2\text{cov}(\delta_{34}, \delta_{44}) - 2\text{cov}(\delta_{34}, \delta_{25}) + 2\text{cov}(\delta_{34}, \delta_{35}) \\
 &\quad + 2\text{cov}(\delta_{34}, \delta_{45}) - 2\text{cov}(\delta_{44}, \delta_{25}) + 2\text{cov}(\delta_{44}, \delta_{35}) + 2\text{cov}(\delta_{44}, \delta_{45}) \\
 &\quad - 2\text{cov}(\delta_{25}, \delta_{35}) - 2\text{cov}(\delta_{25}, \delta_{45}) + 2\text{cov}(\delta_{35}, \delta_{45}).
 \end{aligned} \tag{A18}$$

Computation of these converted parameters and their variances involves only linear operations and thus can be easily obtained using a spreadsheet such as EXCEL or by a simple program in FORTRAN, C, or other languages.

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# Networks of Contention: Villages and Regional Structure in the Seventeenth-Century Ottoman Empire<sup>1</sup>

Karen Barkey  
*Columbia University*

Ronan Van Rossem  
*University of Delaware*

This article analyzes peasant contention in 17th-century Ottoman villages. The authors argue that peasant contention results from the position of the village in the regional structure, with village-level organization providing the means for contention. The article uses court records to reconstruct the formal and informal networks within and across villages in western Anatolia. Under conditions of state and market expansion, those villages in intermediate positions in the regional structure tend to experience the vagaries of these changes more than central or isolated villages. Those intermediate villages are also most prone to contention. Cooperative village organization is also found to promote contention.

## INTRODUCTION

While 16th- and 17th-century western Europe was rocked by mass protest, the Ottoman Empire stood out for its dearth of such rebellion. But despite the lack of rebellions and uprisings, ample contention did take place in the Ottoman Empire, as it, like its western European contemporaries, went through a transformation caused by market and state expansion. The repertoire of contention, however, was drastically different. It did not involve class alliances nor did it ever fully bloom into peasant rebellion. Contention remained local, diffuse, and quickly channeled into state institutions.

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The absence of collective contention, combined with the unique characteristics of the Ottoman state, provides an ideal setting for the study of individual and small group forms of contention. The autonomy of the courts meant that much of the contention was channeled through them. Although the social movements literature focuses mainly on the more spectacular and more glamorous forms of collective action, such as revolutions, rebellions, and riots, the local and small acts that are the focus of this article are the more basic and common forms of contention, closely linked to discontent, often preceding and underlying collective forms of contentious action.

The argument to be developed in this article is that villages not only provide the organizational means for contention but also provide reasons for discontent and contentious action. Class solidarity and class interests are at best of secondary importance. Intravillage solidarity provides the social support, the means for contentious action. The motives for contention, however, can also be found within the village and within its integration in the regional system. A village's position within that system is indicative of its exposure to transformatory forces, which cause tensions and discontent. The village's position within a regional structure is essential to generate reasons for contentious action among peasants, while the actual occurrence of contention remains dependent upon the village's internal organization. Contention is then most likely in villages that have strong internal solidarity and take an intermediate position within a regional system, neither fully integrated nor completely isolated.

This article uses the case of an Ottoman province in the early 17th century to examine the embeddedness of contentious action in inter- and intravillage structures. Court records are used to reconstruct the level of village integration, the structure of the intervillage network, and the occurrence of contentious action. A structural model is tested in which intravillage networks of solidarity and conflict, the ties of villages to other villages, and their position within the regional system predict the incidence of contention in a village.

### Peasant Contention and Village Structures

The literature on contention focuses mainly on large-scale collective action, such as revolutions, rebellions, and riots, largely ignoring smaller and unspectacular forms of contention. Nevertheless, the latter types of contention are substantially more common than the former and may be better indicators of widespread discontent in the population. The applicability of sociological theories of collective action to this type of contention is limited.

The nature of contention in the Ottoman 17th century and the nature

of the evidence we have used require a clarification of the term *contention*. There is no doubt that the political, social, economic, and cultural characteristics of a society affect its repertoires of contention. Yet, the social sciences thus far have focused almost exclusively on forms of *collective* contention (Wolf 1969; Paige 1975; Migdal 1974; Chirot and Ragin 1975), Scott (1985) being a rare exception. Tilly's (1986) definition of contention excludes both individual contentious action and routine contentious action. Individual or small group contention, such as tax evasion, sabotage, draft dodging, civil disobedience, banditry, and disengagement are routinely ignored. These "weapons of the weak" (Scott 1985) require much less organization and fewer resources. And because they are less visible and do not directly challenge the political power structure, they are also less risky. The types of contention that form the object of this article fall in between Tilly's contention and Scott's weapons of the weak. Like the latter, they are individual or small group challenges, and like the former, they openly challenge state policy and authority.

This article focuses on the village in two ways. First, we use village networks to test hypotheses about local solidarity and community organization. And second, we use the position of the village in the regional structure as a determinant of contention.

The literature on peasant movements largely ignores the potential for community-based organization, preferring to emphasize class-based action. This emphasis on class is part of the Marxist heritage. For Marx, and most social scientists since, class was the main basis of organization for collective action (Stinchcombe 1961; Paige 1975, 1983; Migdal 1974; Skocpol 1979; Goldstone 1991). Marx dismissed the role of the peasantry relative to the roles of the bourgeoisie and the proletariat. Marx (1963) compared the French peasantry to potatoes in a sack, bunched together but lacking interconnection. Without a basis for class solidarity and consciousness, they lacked the potential for class-based contention. He based this argument on the compliance of the French peasantry, the lack of coordinating structures in rural France, and the rampant individualism among the petty-proprietor peasantry. The Chinese revolution and subsequent peasant uprisings in Asia and Latin America drew attention back to peasants as a revolutionary class. In this context, peasant movements were assessed as part of Third World development and dependency (Migdal 1974; Paige 1975; Scott 1976; Sharpe 1977; Popkin 1979; Perry 1980; Boswell and Dixon 1990). As the focus on class remained intact, most theorists continued to ignore everyday interactions and relations within and between villages, which form alternative bases of solidarity, consciousness, and organization.

Class and community are competing, sometimes reinforcing, sometimes contradicting, organizational bases of contention. Several different social

science traditions have paid attention to local communities as bases for contentious action. First, historical studies of village-based life have described how local issues lead to discontent and how informal networks of relations organize and mobilize villagers for contention (Blum 1971a, 1971b; Babeau 1879; La Mire 1948; Goubert 1986). Second, the political process model (Tilly 1978; McAdam 1982; Tarrow 1994; Kriesi 1995) focused on the formal and informal organizations that make mobilization for contention possible. Underlying this organization are the networks of informal and everyday interactions among individuals: kinship, friendship, work-based, and so forth. Most recently, smaller units, such as local communities and small associations, have been studied. For instance, Gould's studies of the Paris Commune of 1871 (Gould 1991, 1993, 1995) show that neighborhood relations form a more important base of organization than trade organizations. Likewise, Bearman's (1991) analysis of desertions of Confederate soldiers points to the importance of locality-based solidarity in promoting desertion, another form of resistance to central state authority.

Overall, both types of studies, focusing on class or community, rely on the concept of organization. Formal organization of varying scale enhances the ability of a group to mobilize for collective action (Tilly 1978; Oberschall 1973). It enables members of a group to reach each other and to coordinate resources and enables organizers to reach potential activists. Organization can also refer to informal social organization, where individuals associate outside formal structures. Informal social bonds that result from daily interaction, such as kinship, friendship, and neighborhood familiarity, become the conduit through which action is coordinated. Kinship and friendship ties, amply present in villages, might provide substantial solidarity and cooperation (Goiten 1971; Boissevain and Mitchell 1973; Gellner and Waterbury 1977; Eisenstadt and Roniger 1980). Studies have tested the explanatory power of such informal bases of association in the rural context. To the extent that villagers engaged in relations outside their kin group, to the degree that they relied on friends, neighbors, and other villagers in their daily lives, they wove networks that could have resulted in collective organization (Pimsler 1977).

Recently, informal social organization has been given priority over class as a mechanism for shoring up solidarity. Both Calhoun (1982) and Gould (1995) find that informal social organization—activated by kinship and friendship (in the former) and neighborhoods (in the latter)—represented means for collective participation. For the French rural setting, both Margadant (1979) and Agulhon (1982) have demonstrated social integration and sociability to provide for effective mobilization. More broadly, we can argue that social networks play a decisive role in movement participation (Tilly 1978; McAdam 1986, 1988; Tarrow 1994; Gould 1995). White's



(1965) notion of a *cainet* describes the most favorable conditions for mobilization: groups with common interests (categories) coincide with groups with strong interactions among its members (networks). Such networks between individuals and groups with common interests are ideal for the diffusion of discontent and the mobilization for contentious action. *Cainets* are more likely to occur within local communities rather than within disparate class organizations.

Finally, the importance of social networks for collective contention is well established, but they are also important for individual or small group contention. Even this type of contention is embedded in social support networks of friends, family, and so forth, who support the claims of the challenger. Individual contention, as described here, does not occur in a vacuum but is also embedded in the village community.

Regional systems may affect contention in two ways. First, the networks of interactions among localities may be an instrument for the diffusion of discontent, organization, and contention. Second, the position of the locality in the regional system may create a common interest among its members. There has been relatively little theorizing about the level of intervillage interaction and the position of a village in the larger (regional, national, or international) structure. In the field of Chinese studies, both Skinner (1964) and Kuhn (1980) have been most explicit about intervillage linkages and how they act to promote collective action. Kuhn described how the Chinese rural landscape, from the 11th century on, was dotted by multivillage leagues to protect villagers from bandits. Skinner (1964, pp. 38–39) argued that the “dense network of supravillage ties” created by market demands fashioned a “social grid for peasant movements.” In both these works, it was intervillage linkages that provided for the social contact and solidarity necessary for peasant movements. In Europe, Lefebvre (1989), in his classic work, described the spread of a wave of rural panic in 1789 through weak ties (travelers, messengers, family, friends) between villages. Margadant (1979) demonstrated that village integration in the larger regional system and rural-urban linkages were consequential to the 1851 uprising in rural France. Likewise, Gould (1991, 1993, 1995) described how weak ties among Parisian neighborhoods helped in the mobilization for the Paris Commune of 1871. Brustein (1985, 1986; Brustein and Levi 1987), studying regional rebellions in early modern France, showed that economic interdependence and exchange remained the key to intervillage communication and potential solidarity.

These studies all focus mainly on the diffusion of discontent through intervillage relations. In the case studied here, the absence of large-scale collective action renders the diffusion model inadequate. Regional structure, however, can accommodate for rural movements in a different way. Both Wolf (1969) and Migdal (1974) are cognizant of regional dynamics,

yet they do not speculate on the impact of the particular position of a village in a larger system. The model proposed here is inspired by recent developments in the theories of corporate action. Traditionally, coordinated action was considered the result of interlocking directorates or other ties among firms (Mintz and Schwartz 1985; Useem 1984). Recent studies of corporate political action (Mizruchi 1992) and market behavior (Burt 1990) have provided an alternative explanation for seemingly "collective" behavior. Firms were found to behave similarly, not because they were linked, but because they were in a similar position within the larger structure. Positions reflected both interest and environment. For this reason, we argue that actors that are in similar positions, facing similar environments, will display similar behavior.

Villages that are positioned similarly in the overall intervillage network should show similar patterns of behavior. The structure of the regional system offers the opportunity to underscore this argument. We argue that different positions in the regional structure lead to different exposure to the forces of the national market and the central state and, therefore, promote different forms of behavior. Historically, regions have been organized around a core town or village that was both the economic and political center of the region. This central town functioned as the central marketplace of the region as well as the seat of central government. In premodern or early modern societies, the interactions between the region and the outside world were largely mediated through the center. Village integration varied widely, as it was mostly related to the extent of interaction between the central village/town and other villages. In each regional system, while some villages were strongly integrated, fully participating in exchanges with the central town and beyond, other villages remained virtually isolated, only sporadically engaging others, while still other villages took an intermediate position. Small isolated villages were linked to the national center only through a multilink chain of larger and larger regional systems in which the respective regional centers fulfilled the roles of "bridge" and "broker."

The degree of integration in the regional system reflects the extent of market and state penetration. Transformations like the expansion of the market and the state have been the subject of intense study in the context of the village. The traditional sociocultural and economic context in which peasants make their daily decisions is a local one, village based. As the central state or the national and international market intrude, the village is exposed to increased taxation and fluctuations in the market, which puts additional strains on the daily survival of the peasants. The development of capitalism brings about a breakdown in the traditional balance of rights and obligations, of costs and benefits in the village, exposing the peasants to dislocation and uprooting (Deutsch 1961; Hobsbawm 1959;

Scott 1976; Migdal 1974; Moore 1966; Wolf 1969; Popkin 1979; Chirot and Ragin 1975; Jenkins 1982). This argument has also been made more generally by modernization theorists who have described the distressing effects of macroscale transformations on traditional populations (Deutsch 1961; Lerner 1967; Tilly 1986; Jenkins 1982; Moore 1966; Migdal 1974; Chirot and Ragin 1975; Scott 1976).

It is within this violation of the moral economy of the village, of the traditional obligations and rights of the villagers, that the motives for contention lie, especially when these violations have direct negative consequences for the villagers (Scott 1976). Thompson (1971) made the same argument for the poor in the cities. Custom and fairness frame contentious action.

The forces of state and market, however, do not penetrate evenly the entire region. The center town and the villages that are strongly integrated will first and most strongly be penetrated by market and state expansion, while the isolated villages for a long time will remain largely untouched by them. Villages intermediately integrated in the regional system will tend to experience a slower and more uneven expansion of market and central state institutions. It is not the most or the least integrated villages that are at the highest risk for contention but rather the villages in an intermediate position in the regional system that are only partially integrated in the modern power circuits. While the center town and highly integrated villages are the first to reap the benefits as well as the costs of modernization with a positive net balance, the intermediate villages tend to experience mainly the costs associated with market and state expansion while benefits are slower to accrue. Such negative net balance provides ample reason for contention. An analogous argument has been made in class terms demonstrating the middle peasantry to be the most revolutionary segment of the peasant class (Wolf 1969; Moore 1966; Alavi 1973). This is extended here as to argue that the villages in an intermediate position experienced the increased demands of state and market penetration, yet were unable to fully take advantage of these processes. Therefore, they were most likely to articulate their grievances. In such instances of market and state intervention, such penetration bestowed few benefits but significantly disrupted the traditional modes of action.

Several hypotheses can be derived from the combination of organization and regional structure. First, in villages with low community organization, contentious action is less likely to arise than in villages with strong community organization. Second, in organized but fragmented villages (i.e., villages with internal cleavages based on religion, occupation, class, kinship, etc.), conflict is more likely to remain inner oriented, rather than directed toward the state (i.e., less likely to turn in to contention). Third, villages in intermediate positions in the regional systems are more likely

to be the locus of contention than the villages with strong ties to the center town or isolated villages.

### Case

Rebellions were not part of the repertoire of contentious action in the Ottoman Empire, unlike most of Europe during the 16th and 17th centuries. Yet, from all accounts, the socioeconomic disruption so apparent in western Europe during the 17th-century crisis was also evident in the Ottoman Empire. The last decades of the 16th century had brought financial, demographic, and economic difficulties to the Ottoman state and society (Kafadar 1991; Gerber 1982; Pamuk 1993). The empire was quite uneven in its economic, political, and demographic development. Certain regions developed to respond to internal imperial needs while others responded to the world market. Still others remained totally out of the loop, backward and unaware of either the center or the international trading network (Goffman 1990; Faroqhi 1994). Therefore, in many aspects, the Ottoman Empire was similar to parts of Europe, experiencing the crises of the 17th century and an uneven development. The response to these crises was, however, different.

The political opportunity structure of the Ottoman Empire was unfavorable to collective contentious action (Tarrow 1994; Kriesi 1995). As Barkey (1991a, 1991b, 1994) pointed out, unlike France, the Ottoman Empire did not experience rural rebellions in reaction to the state's actions of centralization and consolidation in the 17th century. The Ottoman landholders and peasants were unable to ally in rebellion due to the lack of organizational and institutional solidarity of the villages in the Ottoman Empire. Unlike France, where the nobility and the peasantry defined their interests as related, the Ottoman actors did not. This was due to the care with which the state maintained divisions between patrons and clients through the system of rotation and how state action styled each class to perceive their interests in relation to the state rather than as in opposition to the state. Furthermore, Ottoman peasants, unlike their French counterparts, were not organized at the village level. Their institutions produced no united community but instead encouraged communal segmentation and permanent rural migration that impeded collective action (Barkey 1991a, 1991b, 1994).

As a result, the Ottoman countryside displayed contention patterns quite different from its European counterparts. First, Ottoman peasants resorted to individual forms of contentious action. And second, a special role was reserved for the courts, which became an arena of contention. The use of state institutions as the repository of contention is in many ways counterintuitive. The independent judiciary of the Ottoman Empire

allowed both for complaints against state officials to be brought to court and for the solution of grievances before they took a rebellious communal form. Moreover, that the court did act in favor of peasants in many cases attests to its autonomous capacity as a conduit for contention.

To understand the particular position of the Ottoman court, its nature and independence, one should contrast it with the role of the court in most western European countries. While, in the Ottoman Empire, the court was an institution independent of landed interests, this was not the case in Europe. As Bloch (1966, pp. 133–34) shows, “in both Germany and England the main support for the seigneurial regime was entrenched in elective institutions, the junker-dominated German Estates and the English Houses of Parliament, which were composed largely of the gentry who as Justices of the Peace were also responsible for law and order in the countryside. In France the inferior tribunals of the bailiwick and senechal’s courts, the presidial courts and above all the Parlements were all entrenched supporters of landed property.” Furthermore, as individual peasants within the seigneur’s demesne, European peasants were confronted by seigneurial appointees or by judges identifying with landholder interests (Mousnier 1980), while Ottoman peasants appealed to a judge who was an appointee of the central state administration. The Ottoman system provided peasants with an alternative route of action and complaint unconstrained by landholders, as a sort of safety valve. Accordingly, we see numerous cases of peasant protest against landholders, taxes, state policies, and other matters being resolved at the court level, suppressing further disturbance or collective action. In the Ottoman context then, the court system, as positioned in the structure of rural society, acted as a significant deterrent to collective action, dealing with contentious issues on an individual and institutional level.

What kind of complaints did Ottoman peasants bring to court? Much of the time, peasants came to court for official business, to register a land transaction, a sale, a marriage. Peasants also came to court to contest and dispute the “injustice” they suffered, separate from the routine and necessary activity. Perceived injustice, the key to contention, was shared within the village. The court recorded peasant objections concerning injustice done by officials, fellow peasants, merchants, traders, and bandits. Not surprisingly, the main sultanic orders regarding peace and security in the provinces were called *Adaletname* meaning “justice rescripts.” Especially in the 16th and 17th centuries, sultans often distributed these rescripts throughout the provinces. Their mandate was to maintain justice and order, and they knew well that the center depended on the peasantry for replenishing treasuries and granaries.

That peasants were fully cognizant of the role of the court as upholding justice in the name of the sultan is demonstrated by the fact that they

often traveled long distances to inquire on the nature or the excess of taxes. They frequently objected to the increase in taxation, especially if they understood it as a violation of the regional tax code. During this period of economic and commercial transformation, abuse took the form of numerous intermediaries between peasant and state or peasant and landholder. Each intermediary tried to skim off some part of the peasant's produce, selling it in a wider market. In a typical case, a few salt-carrier nomads came to court to complain that the superintendent, Hizir Aga, was imposing extra salt duties on them. For every camel they brought in, the superintendent charged them one count for himself and one count extra as a recording fee. They asked the court to stop this unfair levy on salt. The court investigated the story and summoned experts to testify about past practice. When it was ascertained that such extra fees were never collected before, Hizir Aga was banned from such activity. The nomads returned to their settlement content that tradition was restored (*Kadi Sicilleri*, register 102). Such complaints, either by a single individual or a couple of peasants, were frequent. By the frequent application of such justice, the court arrested the further progression of protest, defusing it before it matured into a major collective movement (Barkey 1991a).

It was not only the state, the market, and their respective agents who directly threatened the peasant's well-being but other villagers as well. In ways reminiscent of the kulaks, when some villagers adapted to the penetration of state and market forces, they found themselves pitted against other villagers in intense struggles of tax collection and exemption. Or, if peasants discovered free riders in the community, they quickly took them to court, demanding that the judge resolve the problem by some equitable sharing of the tax burden. Most often, they questioned the exempt status of certain villagers, maintaining that such exemption shifted an undue burden on the shoulders of some and not all. Peasants overwhelmingly came to court to have justice prevail. They brought up issues of fairness, of just and unjust taxation, or of contested land. Perceiving the actions of state officials as well as fellow villagers as infringements of their typical tradition and way of life, they fought to restore the old order.

While peasants brought complaints against the state officials to the court, officials also directed their accusations and protests against peasants to the court. Often, officials reacted to peasant contention by bringing it to court; initiative, therefore, came from both sides, and the court was used by both sides as the most reliable mechanism for conflict resolution.

Peasants tended to espouse "public transcripts" that evidently imitated state language. Given the rhetoric of the justice rescripts, it is not difficult to understand why the peasantry chose to adopt the same terminology when addressing the court or other state officials. They benefited from framing their issues as concerns the state shared; they sought to present

issues in the idiom provided by the state in the hope that they would earn state support. Justice was at the center of the state discourse, and as such, it was embraced by the populace in the language of petitions and court statements. State orders to local officials read, "do not do injustice to anyone" or "do not do anything contrary to the law."

The peasants used the same language in their petitions, evoking the law, past practice, and the sultan's justice. The framing of issues in terms of justice directly connected with the local system of values and norms. Injustice frames were created to gain solidarity with fellow villages and to justify acts of contention (Gamson 1995). Moreover, petitions to the court used references to the local, old, and accepted system of values and norms as a way to substantiate claims. As such, peasants nearly always argued that "in the past, taxes were collected differently" or "in the past, officials presented adequate documentation for extra charges." The phrase *kadimden*, which referred to "old practice," cropped up in every contentious complaint the peasantry directed to the court.

As an institutionalized channel for grievances, the courts made it easier for individuals to readily bring in their complaints as individuals rather than wait for communal circumstances to arise (Barkey 1994). In the process, the courts provided a record of disputes, the patterns of which can be assessed in light of village networks and levels of integration. Courts not only individualized contention but also defused situations that in the long run might have led to collective upheaval.

Manisa, the central town of the province of Saruhan was located in the Gediz river valley and quite close to Izmir, an Aegean port of some importance. In 1531, its population (town and surrounding villages) was estimated at 30,177 and, by 1575, had grown to 41,995. In 1575, 77.5% of the population were peasants or nomads and 18.1% townsmen with the remainder landholders and officials (Emecen 1989).<sup>1</sup> This region was selected for our study because it was part of a core province of the Ottoman Empire where the traditional system of agriculture and land tenure was firmly implanted. Furthermore, its ideal location—close to the seat of power of the central state but not close enough to have lost all organizational autonomy—ensured that state intervention and market development altered the conditions of the villages in this region, albeit to different extents. As regards the dependent variable, contentious action, the region was similar to the other Ottoman provinces where peasant movements were quite exceptional. The greater Manisa region was both integrated

<sup>1</sup> As a comparison, the various estimates of Istanbul (excluding the surrounding areas) place its population as somewhere between 400,000 and 500,000 around 1550. For the 17th century, estimates of Istanbul vary from 700,000 to 800,000 (Mantran 1962, pp. 45–46).

into the larger Ottoman system of taxation, provisioning, and trade and experienced the transformation of the early 17th century. Therefore, we can safely argue that the region is representative of the typical Ottoman core province.

Already in the 14th and 15th centuries, the Ottoman state had furthered the development of a coherent trading network between Istanbul and Manisa. A productive region, bounteous in agricultural products such as fruits, nuts, rice, and leathers, it participated in the provisioning network from the hinterland to the capital. As such, it was typical of the Ottoman provinces where the movement of foodstuffs to the center was strongly encouraged and controlled, while intraregional trade and transport was strongly discouraged. Yet this network was not to remain untouched by external events. It was affected both by developments in the world economy and the increasing need for cash on the part of the Ottoman state.

In the early 17th century, the Austrian War (1593–1606) and the war with the Safavids (1603–18) put more strains on the treasury. The state was strapped for cash; it was also strapped for men to fight and food to feed the soldiers. In the period from 1592 to 1646, the *cisye* (tax on non-Muslims) doubled, going from 100 *akçes* to 290–325 *akçes*.<sup>3</sup> The *avaris* (an occasional tax levied during wartime) was transformed into an annual tax, its nominal rate doubled, and the population from which it was assessed broadened considerably (Inalcik 1980, pp. 312–13; Darling 1997). In this period, the *avaris* went from 160 to anywhere between 125–500 *akçes*. By the 1640s, the *cisye* was stabilized at about 250–300 *akçes* and the *avaris* at 325 *akçes*. Where the *cisye* kept pace with inflation, the total amount from the *avaris* increased in real terms 236% (Darling 1996, pp. 117–18, 239–40). Much of that was because more people were subject to this tax. The *avaris* became, in the 17th century, the “largest single item of government income” (Darling 1997). The early 17th century, then, presents itself as a period of state financial readjustment, when state bureaucrats increased and reformulated the taxes and strove to pervade more societal interactions.

In the 17th century, while state needs increased, developments in the world economy provided an alternate route for productive classes. Especially in western Anatolia, the region under analysis, many centers of production and centers of transportation developed in response to European demands for new products. Already in the late 16th century, English traders participated in Mediterranean trade to import woolen cloth. Also while the trade in spices decreased, that in coffee increased and raw silk was sold in Izmir, all these changes affecting the western coast of Anatolia

<sup>3</sup> *Akçes* are Ottoman silver coins. For an in-depth view on the Ottoman monetary system, see Pamuk (1994).



(Farooqi 1994). Goffman details, for example, how Izmir went from being a backwater in the 16th century to an important trading port in the 17th century. Where many regions had been satisfied with their relationship to the capital, they now perceived opportunities in new production and trade. As Europe made forays into the western Anatolian coast for grains, Istanbul increasingly struggled to control its own delivery routes (Goffman 1990, p. 38).

How did these changes in state and market interaction affect the western Anatolian peasant? In the late 16th century, the supply lines between hinterland and capital were disrupted when the producers and manufacturers of the region sold their goods to European companies. This change mostly affected Istanbul, forcing the state to struggle for control of trade routes. In the 17th century, however, traders and middlemen entered the scene, breaking the supply lines between peasants, nomads, town markets, and merchants. The middlemen went directly into the hinterland, buying from the local producers (Goffman 1990, pp. 70–71). This not only affected Istanbul and the local traders and manufacturers but also directly exposed the peasantry to the vagaries of the market. Removing traditional intermediaries stripped villages from their protective structures. Under such conditions, not all villages coped with these changes equally well. Some well-integrated villages continued to thrive while others suffered from exposure to the market and state forces. Small-scale struggles and contentious actions followed from the uprooting and malintegration (Deutsch 1961).

## DATA AND METHODS

Systematic data about social relations in historical societies is scarce. In particular, little or no systematic records exist that allow us to reconstruct the intravillage as well as the intervillage social structures. Birth and marriage records, even when available, only shed light on one small aspect of the social structure and the networks within and between villages. Court records, on the other hand, can provide information on a broader range of activities and relations, conflictual as well as cooperative ones (Barkey 1991b).

In this article, court records were used as the source for information both on the relations within and among villages and on the occurrence of contention. Court records are not only rich with conventional data such as wealth, inequality, and patron-client arrangements that organize the social structure of the region but also, and even more significant, provide relational data. Since a case not only includes the individuals in conflict but involves also many others who participate in the case as witnesses, friends, legal representatives, guarantors, and so forth, it can provide a

snapshot of the social structure of a particular village and region. The range of cases reassures the researcher that an adequate sense of the community under investigation will be provided. Smith (1979), who carried out a similar analysis for an English rural setting, makes the point succinctly: "The range of activities is so large that it is hard to conceive how a villager could have avoided appearing before the court at one point in time; either as defendant or plaintiff, acting as a pledge, performing an official duty, entering into land, registering a land deal."

The data on internal and external village network structure and contentious activity used in the analysis are taken from the court records found in the regional archives of Manisa in western Anatolia. A period of four years, 1650–54, and three court registers were sampled (*Kadi Sicilleri* 1650–54).<sup>4</sup> In each register, only cases pertaining to villages (within themselves, in interaction with other villages, or in interaction with the central town, Manisa) were recorded. Those cases that were unreadable because of paper damage or a particularly difficult script or that involved only Manisa residents were discarded.<sup>5</sup> This left us with 190 cases.

The court cases took place against a complex background of economic difficulties and political and economic expansion that undermined the livelihood of the peasant population. Peasants also struggled with more taxes since increased warfare required an expanded state resource base. A quick look at the data confirms parts of our suspicion regarding diffuse, generalized social conflict: the overwhelming number of cases in this period were civil cases that emerged out of the haggling between parties over minimal or dwindling benefits. For example, negotiations between patron and client concerning the control of a few yards of land display a dramatic sense of insecurity. To a peasant, the loss of a trivial scrap of land could spell disaster.

In the provinces of the Ottoman Empire, the administration of law according to Islamic law, the *Ser'ia*, was the function of the *kadi*, the judge,

<sup>4</sup> The years 1650–54 were chosen since a major reorganization of land administration occurred just before improving both registration and record keeping. Furthermore, another sample was drawn for the period before the reorganization (with a comparison of the two periods projected). Yet, this earlier period did not yield comparable or usable data. A total of six registers were listed in the catalogs for the years 1650–54. Of these, one was missing and another dealt mainly with Manisa cases only. Because of limited access to the archives, only three of the remaining volumes were pulled. These volumes were chosen to maximize readability, the number of cases involving a variety of villages, as well as the number of judges. Given the regularity of record keeping, no systematic bias would be expected from selecting cases from only a limited number of volumes.

<sup>5</sup> The decision to discard cases based on Manisa residents only was made since the research was focused primarily on peasant contention. Cases resulting from the interaction between peasants and Manisa dwellers were retained.

a learned official of the Ottoman administrative system. During his time in office, the judge officiated at the central court, listening to cases, appointing agents to investigate matters, and finally providing a ruling that was signed by a group of witnesses, usually elders, present at court. The judge also designated deputies whom he sent to faraway villages to administer law and justice (Inalcik 1988). The office of the judge was organized according to regional jurisdictions of somewhere between 100 and 150 villages. These villages would be further subdivided into 10 smaller jurisdictions to which the judge sent deputies to handle routine matters. The judge held his court in the central town, with villagers coming to him from everywhere under his jurisdiction. Manisa was quite a large administrative town with an official count of 150 villages in 1530 and 180 villages in 1575 (Emecen 1989). By the 17th century, the number of villages had declined, and in our study, 114 of these villages appeared in the court records.

The court records (*kadi sicils*) used for this study were handwritten and locally kept. Courts met every day, usually attending to one or two cases a day, rarely more. This was because the judge had many other administrative duties in the province (Jennings 1972). It was the responsibility of the scribe to summarize the court proceedings in day-by-day entries in the records. It is not known whether the scribe took notes during the proceedings and later on entered them in the records or summarized the events directly into the records. We assume that the records were prepared after the court proceedings. This brings up issues of the scribe's bias or questions of recall that we were not able to control. However, the detail recorded in the records backs up their reliability.

The Ottoman judicial system was not without its biases. On the one hand, since judges collected a nominal court fee, we have to assume that very poor peasants were underrepresented in the court records. On the other hand, neither the records nor the evidence from local histories hints at any major prejudice on the part of the judges in favor of local officials, notables, or other members of the elite. On the contrary, peasants often assumed that the judge was partial to them and went to court to find an ally in patron-client disputes. This, we think, was also not totally accurate, and the impression we obtained was that judges, overall, tended to present impartial rulings.

The manner in which the records were generated and collected, however, puts certain restrictions on the data and the conclusions that can be drawn from them. The data, as collected, do not contain any information about conflicts in which all parties were from Manisa, the central town. The role played by the courts for conflict resolution in Manisa is somewhat different from its role for the rest of the region. While the court got involved in most of the legal conflicts in Manisa, only the more important

ones from the outlying villages were heard. At some level, traditional forms of conflict resolution must have operated within villages, with the elders diffusing intravillage struggles while deputy judges attended to some of the conflict in the outlying regions.<sup>6</sup>

The regional structure derived from the court records has Manisa at its center. Villages that also participated in other regional networks will tend to take a marginal position in the Manisa-centered regional system.<sup>7</sup> The selectiveness of the court cases meant that only a small number of relations was available to reconstruct intra- and intervillage structures. The 190 cases analyzed gave 1,121 dyads with which to reconstruct both inter- and intravillage networks.

### Contentious Action

In this study, we define contentious action as all events involving peasant complaints on issues of state and market intervention, village land structure, and taxation. Court records ranged from family to land, to commerce, to tax collection, to personal, and to criminal issues. We coded as contentious action all cases involving individual or groups of peasants going to court to complain against other peasants, officials, and power holders and where issues of taxation, land, and village rights were in dispute. Contentious court cases all had to do with either taxation or land disputes. In the Ottoman Empire, land disputes were inherently political in nature. Not only was the control over land at the heart of the socioeconomic and political structure of early modern societies, but in this case, the state actually owned the land, with officials administering the land on behalf of the state. Land disputes and taxation issues, therefore, continually challenged state authority.

Of the 190 cases registered in the court documents, 38 (or 20%) qualified as cases of contention, involving 34 (or 30%) of the 114 villages included in the analysis. The remaining cases included 37 (20%) criminal cases, 26 (14%) cases involving family matters (marriage, death, divorce, etc.), 38 (20%) cases involving the registration of or disputes about property, 32

<sup>6</sup> On the one hand, that we did not have access to the reports of such deputy judges probably restricts the data to the closer, better traveled and better networked villages. On the other hand, the fact that we do have some data about the more distant and isolated villages indicates that the court took a central place in the sociocultural life of the peasantry.

<sup>7</sup> The effect of village membership in overlapping regional networks on contention remains unclear. Based on the moral economy argument and on network studies of "bridges" and "brokers" (Burt 1992), one would expect low contention rates in these villages as they are ideally located to benefit from macrostructural changes

(17%) cases about inheritance, 6 (3%) accident cases, 12 (6%) cases involving official registration, and 1 case that could not be classified.

### Content and Identity Networks

The court records are rich with relational information. The individuals who participate in a case have network ties among each other, as well as with their opponents. We use the ties between people to reconstruct the social structure of the region. Ties are indicative of the intravillage and intervillage networks. The patterns of relationships that emerge will tell us how densely or how sparsely the villages were related to one another and also the extent to which these relationships formed lateral or vertical patterns of interaction. The results should help provide a basis to discuss the likelihood of collective action.

We constructed a data set in which the unit is a tie between a pair of people in the same case. First, each tie is described by its content. Three types of relations were prevalent in these cases: cooperative, conflicting, and neutral. Conflict relations were those relations of personal and communal struggle between individuals who were in adversary positions. Mostly they appeared between plaintiff and defendant but not only so. Other individuals could be drawn into the cases who exhibited conflictive relations with the plaintiff or the defendant. Cooperative relations were coded when a pair was in a supporting role; essentially agreements between parties to act for a common goal, to complain together against a third party, to ask for the lowering of particular taxes in accord with a few members of the community. Pledging and legal representation were both also instances of cooperation where one villager came to the assistance of another. Relations were coded as cooperative when both parties in a dyad were at the same side in a court case and as conflictive when they were at opposite sides. Neutral relations involved neither conflict nor cooperation of people who were in stable relations or people drawn into a case because they happened to observe the events leading up to the case. In this analysis, only the conflictive and cooperative ties were used. The neutral relations were excluded as they were not necessarily indicative of the existence of a tie between the parties involved.

Second, the ties were also described along dimensions of identity. These ties ranged from kinship to economic and the coding allowed for multiplicity of ties. Among these the most important were kinship, friendship, acquaintanceship, trade, and patron-client ties. The friendship and acquaintanceship ties are important to discuss since they involved careful analysis and coding of the description of the relations in the court case. At no time was there a qualification of a relationship by the court. The evidence of friendship versus acquaintanceship was evaluated from the stories in the

court records. Friendship is seen as voluntary and is seen as the product of close ties, implying that the friends saw each other often, and, in such a nonindustrialized context, could be the source of social and economic benefits. For purpose of the analysis, friendship and kinship ties were combined into strong social ties.

Acquaintances were defined as the weak social ties between individuals. Granovetter (1973) pointed to the importance of weak ties that provide bridges between communities. Acquaintances, however, were not only potential allies but, often, opposed each other in conflict situations. Since interactions in early modern society were still overwhelmingly based on face-to-face contact, one was likely to develop personal ties to both allies and enemies. Another set of weak ties were represented by trade. This is because in the absence of a market economy, it was more likely that peasants engaged in spot exchanges where they briefly did business, exchanged commodities, and went along their way. With the spread of commercialization, trade networks span wider and weak ties increased. Patron-client ties included slave-master relations and different landholder-peasant relations, which, though intense, were likely to be conflictive. Both trade and patron-client ties are examples of ties that are mainly instrumental in nature.

The observed dyads between individuals involved in a case are aggregated into relations between villages. The final strength between two villages is the count of the number of relations between individuals of these villages.

### Village Characteristics

In many ways, despite vast structural differences, Ottoman villages shared similar demographic and agricultural patterns with their European counterparts. As in Europe at this time, not all villages were part of a growing market economy; some remained completely isolated while others gingerly moved toward a mixed economy. The network data will be augmented by descriptive information on the villages obtained from several sources (Emecen 1989; *Tapu Tahrir* 1572, 1582, 1655). While most of this data can be gleaned from taxation registers, many 17th-century registers are missing. We have, therefore, used available registers together with secondary material and late-16th-century material to assess and to extrapolate. This information unfortunately is of too varying a quality and is too spotty to include in the statistical analysis.

### Analysis

For the purpose of this article, blockmodel techniques are more appropriate than the more common network autocorrelation models of collec-

tive action (Gould 1995; Marwell 1993). The purpose of the network autocorrelation techniques is to model the diffusion of contention through a system, and, therefore, they are more appropriate to explain the occurrence of large-scale collective contention or of micromobilization in a local community. Large-scale contention, however, was entirely absent in this region during the period studied. Rather, contention tended to take on an individual or small group form. Micromobilization processes did probably take place within individual villages, but there is no evidence that diffusion of contention occurred across villages. Rather, the model we propose here sees contention inspired by the tensions that were created by forces of state and market expansion and that are dependent on a village's position in the regional structure. Structural equivalence-based blockmodeling is well suited to such problems, as blockmodels group together nodes with similar relations to the other nodes in the network (i.e., nodes with similar positions).

A pseudo-CONCOR blockmodel, using the Structure 4.2 program (Burt 1991), captures the regional structure embedded in the two types of networks. A multiplex analysis was performed, including both content and identity networks. A village's position in the regional system was determined by its participation in each of the content and identity networks. Of the content networks, only the conflictive and cooperative relations were included in the analysis. Only those villages that participated in at least a single court case during the period studied were included in the network analysis, resulting in a final sample of 114 villages. The final blocks of structurally equivalent villages were arrived at using a Ward cluster analysis.<sup>8</sup>

Other summary measures of a village's position in the regional structure are choice status (i.e., the proportion of other villages that have a tie to a given village); and the eigenvalue prominence, which captures the visibility and importance of a village in the regional structure (Knoke and Burt 1983).

Further to be included in the analysis are the number of relations a village has with other villages in each of the subnetworks (two content networks and three identity networks). This reveals in what way a village is connected to other villages. The proportion of intravillage relations is a measure not only of village organization but also of the basis of this organization. A high proportion of cooperative intravillage relations is

<sup>8</sup> The Ward cluster method is an incremental sum of squares method, in which the distance to a new cluster formed by combining two earlier clusters is expressed as a weighted sum of the distances to the earlier clusters (Jobson 1992, p. 514). This method has the advantage of tending to provide more clearly delineated clusters than the more commonly used linkage methods.

indicative of a strong community organization, while a high proportion of conflictive intravillage relations suggests little or no community organization and high conflict. High proportions of both cooperative and conflictive intravillage relations, on the other hand, suggests a fragmented village in which several factions feud among each other. However, a high proportion of intravillage relations may also point to the isolation of the village from other villages. The number of outdegrees controls for the latter.

To control for any possible contamination by the sampling frame, the number of cases a village occurs in is added to the analysis as a control variable. One can expect that villages that are involved in more cases have also more extensive relations and a higher probability of engaging in contention.

Because the various network measures are moderately to strongly correlated, discriminant analysis was used to analyze the relation between the structural variables and contention. The discriminant function maximizes the distance between the contentious villages and the noncontentious villages. The structure matrix (i.e., the vector of correlations of the observed variables with the discriminant function) provides information about the relative importance of each of the structural variables in distinguishing contentious villages from noncontentious ones. The advantage of discriminant analysis is that it allows for the description of the dimension separating the two groups without having the variables compete with each other. The strength of the discriminant function will be evaluated by its canonical correlation coefficient and the improvement in predicting group membership.

## RESULTS

The blockmodel analysis distinguished four blocks or positions of structurally equivalent villages. Figure 1 shows a schematic representation of the regional structure including the relationships among villages in a position and between villages in different positions. The thickness of the lines in figure 1 is proportional to the density of ties between or within blocks. The minimum density represented in the figure is 0.005.

According to the results of the blockmodel analysis, the villages in block 2 occupy a central position in the regional structure, having the most extensive relationships not only with villages in other blocks but also among themselves. Other blocks all have relations with block 2 but remain isolated from each other. The central position of the block 2 villages is also confirmed by the structural characteristics that are compared in table 1. The villages in block 2, for instance, not only on the average have significant (at  $P < 0.01$ ) higher choice status (i.e., the proportion of villages in



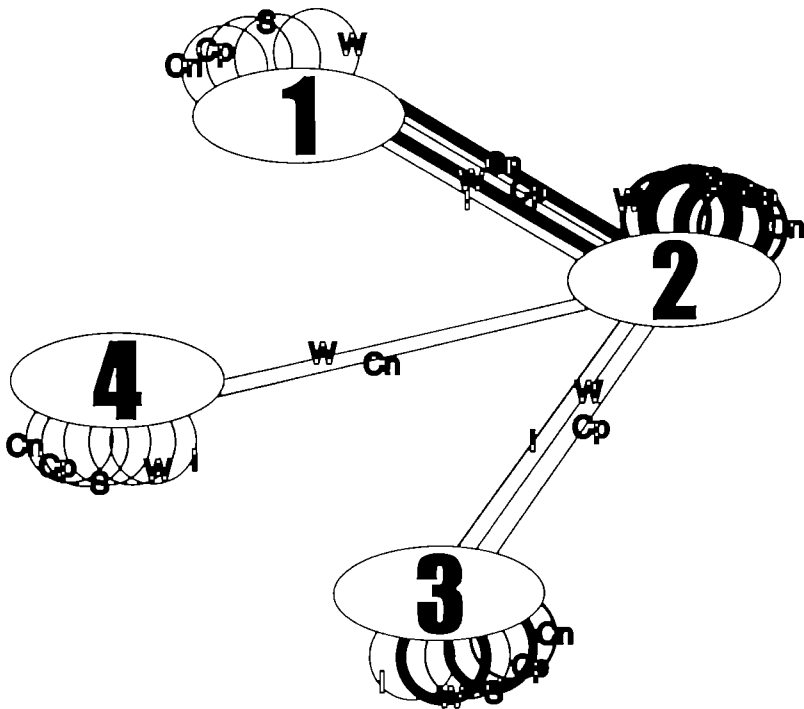


FIG. 1.—Regional structure (conflictive = Cn; cooperative = Cp; strong social = S; weak social = W; instrumental = I).

the system they have ties with) than the villages in the other blocks but on the average also have significantly higher prominence scores than the villages in block 3 (at  $P < 0.05$ ) and block 1 (at  $P < 0.01$ ). The centrality of block 2 is also expressed by a greater number of outdegrees for all subnetworks except for cooperative ties compared to blocks 1 and 4 (all significant at  $P < 0.05$ ) and, in the conflictive, strong social and weak social ties also greater than block 3 ( $P < 0.05$ ).

Block 2 is composed of the central town, Manisa, and five villages. Peasants walked and rode their animals to the town of Manisa for several reasons; they sometimes needed the services of the judge, but more often, they were required to deliver part of their produce to feed Manisa as well as more distant cities such as Istanbul. The laws required peasants to bring their produce to markets no further than one day's trip (Barkan 1943). While most villages surrounding Manisa used it as their central market, the importance of Manisa was much greater for the villages in its immediate surroundings. Three of the villages in this block belonged

TABLE 1

## STRUCTURAL CHARACTERISTICS OF DIFFERENT POSITIONS IN REGIONAL STRUCTURE

	Block 1	Block 2	Block 3	Block 4	Total	Explained Variance (%)
Sampling frame .....	1.563	9.000	2 231	1 798	2 193	11.4**
Choice status .....	.009	.053	.012	.004	.008	18.0
Prominence .....	.043	.181	.069	.092	.088	6.5
Intravillage relationships:						
Conflictive .....	.035	.004	.328	.547	.421	19.4***
Cooperative .....	.125	.172	.420	.624	.507	15.3***
Instrumental .....	.000	.000	.058	.160	.118	4.3
Strong social .....	.188	.161	.308	.560	.458	10.3**
Weak social .....	.027	.019	.373	.484	.383	15.2***
Outdegrees:						
Conflictive .....	4.063	14.167	.692	1.101	2.158	14.1***
Cooperative .....	.313	2.333	3.462	.228	.719	16.0***
Instrumental .....	.188	1.333	.462	.114	.228	8.9*
Strong social .....	.000	3.333	.000	.127	.263	42.9***
Weak social .....	4.125	12.833	4.000	1.101	2.474	12.1**
* .....	16	6	13	79	114	

\*  $P < .05$ \*\*  $P < .01$ .\*\*\*  $P < .001$ 

to Manisa's immediate outlying district. In fact, these villages were mostly abandoned since they were cultivated by urban dwellers in search of supplementary income. Rather small, these villages were characterized by mixed agricultural forms: both subsistence and cash crops. While Manisa and these three villages seem to form a coherent unit, we do not have enough information to categorize the other two. One of them is fairly close to Manisa, while the other is quite distant,<sup>9</sup> but no agricultural production, taxation, or market activity is anywhere provided to see if they fit any pattern.

The level of village organization in block 2 varies widely but remains overall low (see table 1). This could be the result of different factors. Often gardens and vineyards around the town limits were owned by townsmen as freehold property (Faroghi 1994, p. 451). In such cases, if Manisa residents cultivated these villages, they certainly did not contribute to internal

<sup>9</sup> See map in fig. 2. Exact geographical location is available only for a small number of villages.

village organization since they returned home to Manisa every evening. Also, our study excluded those cases involving Manisa residents only, therefore keeping out the cases involving the hub of activity.

Block 1 consists of intermediate villages. These have extensive relationships with the center block 2. Many of these relationships to the center block are conflictive in nature (density = .66) and are made up of weak social ties (density = .67). The villages in this block, on the average, have the lowest prominence of any block. The analysis of the descriptive and historical material confirms this larger structural position and role.

Most of the 16 villages in block 1, which formed a medium distance ring around the central town of Manisa, were usually small, sparsely settled, and largely cultivated by outsiders. Furthermore, the typical villages in this block (such as Adilobasi, Yengi-Pazari, Kadi, or Ormenos) were either market villages or were located on major passageways or near bridges, suggesting a high flow of traffic through these villages (Emecen 1989, p. 244). Even though such traffic might have been highly regulated by local state agents and representatives eager to ensure the proper distribution of goods, it certainly increased the exposure of such villages to external pressures such as taxation and commercialization. Moreover, since peasants paid their dues in cash, they were obligated to attend markets and sell part of their produce.

Mostly characterized by a mixed mode of agriculture, block 1 villages represented a transitional type of settlement with both subsistence and cash crops, mainly cotton, but also other crops such as sesame and chestnuts. They responded to population growth by increased wheat production and to market demand by increased cotton production. Given the expansion of demand for cotton, the villagers in this region finally settled (especially in the 17th century) with cotton as their major cash crop (Emecen 1989, p. 250). Cotton also implied weaving and small-scale yarn manufacturing. Taxes in the block were relatively high (especially compared to blocks 2 and 4), ranging from 6,000 *akçes* to 26,000 *akçes* per village. Ormenos, a village in this region, was taxed 2,400 *akçes* just for its chestnut trees. Wheat was the other main crop, and taxes based on wheat almost doubled between 1531 and 1575. All four of the main villages in this block (Adilobasi, Yengi-Pazari, Ormenos, and Kadi) cultivated cotton, with two (Yengi-Pazari and Ormenos) also specializing in sesame.

The villages in block 3, although on the whole similar to those of block 1, were especially distinguishable by their weaker ties to the center. Overall, block 3 also appears to be intermediate. However, its intermediate position is derived from different factors. Rather than extending ties beyond their boundaries, villages in this block tended to frequently focus on other similar villages. Based on weak social ties, the relations between villages were more cooperative than conflictive.

Typically, they appeared in court cases as part of village conglomerates of three to five villages added together to form a collective tax unit. In typical fashion then, Erdelli appeared with Sindelli and Ballica, while Kafirboz, a primarily non-Muslim village, appeared together with other non-Muslim villages. Like the villages of block 1, the villages of this block also engaged in mixed agriculture, in subsistence agriculture for their daily needs and their regular taxes and in cash crops in response to state and market needs. The non-Muslim villages not only produced cotton and sesame, but also especially rice, while the Christian villages specialized in wine production. Since they were relatively rich and relatively far from Manisa, stricter official control was implemented to ensure taxation and administration. It was the urgency to administer such distant and relatively wealthy regions that ushered in the practice of village conglomerates that were assessed and taxed collectively.

The villages in block 3 have higher than average intravillage organization, this in contrast to block 1 villages that were largely unorganized. A corollary of this higher level of organization was that the cultivators in this block were never outsiders; they were settled agriculturists engaged in subsistence and increasingly in cash crop production. In the late 16th century, their tax payments ranged from 6,500 *akçes* to 43,000 *akçes*. The highest tax figure was from a conglomerate of five villages where both cotton and rice were cultivated.<sup>10</sup>

Block 4 consists of a wide variety of villages with the distinguishing feature of marginal integration into the system dominated by Manisa. Many were small villages that were relatively isolated. Yet, interestingly enough, the block also includes a few major villages (such as Osmancalu and Palamut) that were at the center of their own regional system. Far enough from Manisa yet important with respect to trade and administrative practice, these villages had gained some degree of autonomy. They were part of larger village conglomerates but with a central role within their respective groups. Having invested heavily in cotton and rice production, they also had an interest in developing their own trading zones. For example, Emecen (1989, pp. 270–71) estimates that, as market towns, Osmancalu commanded the trade of an area of about 1,000 people while Palamut commanded about 4,000 people. Both these towns were also known as social and cultural centers since they had both mosques and dervish convents. They therefore attracted peasants from lesser villages who came to pray, slaughter sheep as sacrifice, and perform rain prayers (Emecen 1989, p. 219). Moreover, we assume that, as distant centers, they

<sup>10</sup> To put these sums in perspective, in 1575, 1 *kile* (1 Manisa *kile* ≈ 75kg) cost about 24 *akçes* (Emecen 1989, p. 242; Mantran 1962, p. 273).

were regularly visited by deputy judges (*naib*) whose case reports did not appear in the Manisa registers.

In terms of the larger regional structure, the villages in block 4 retained only weak ties to the center and held few ties to other villages. As far as the Manisa-dominated regional system is concerned, they remained isolated. They did however, demonstrate high levels of intravillage interaction, which on further analysis shows fragmentation rather than cohesion since both high proportions of cooperative and conflictive ties were present in the same village.

Contention is most frequent in the intermediate blocks 1 and 3 and lowest among the isolated villages (block 4). Half (8/16) of the villages in block 1 and 46% (6/13) of the villages in block 3 experienced contention during the period studied, compared to one third (2/6) of the center villages of block 2 and only 23% (18/79) of those in block 4. Although the overall differences are not significant ( $\chi^2_{(3)} = 6.7$ ), the difference between blocks 1 and 4 is significant ( $P < 0.05$ ).

Contentious court cases on the average involved more persons than noncontentious cases, namely 6.1 as compared to 4.7. This difference is significant ( $P < 0.05$ ). Cases about collective contentious action on the average have significantly ( $P < 0.01$ ) more persons involved than either individual contentious or noncontentious cases (namely 8.0 persons per collective contentious case compared to 3.6 persons per individual contentious and 4.7 persons per noncontentious case). Peasants are overrepresented as participants in contentious cases. Of the 182 participants in contentious cases, 121 or 67% were peasants, compared to only 34% ( $n = 236$ ) of the participants in the noncontentious cases ( $\chi^2_{(1)} = 42.6$ ;  $P < 0.001$ ). Participants in contentious cases are also more likely to be Muslims, rather than Christians or Jews, than participants in noncontentious cases (98% [ $n = 218$ ] vs. 88% [ $n = 679$ ];  $\chi^2_{(1)} = 19.1$ ;  $P < 0.001$ ) and also more male (96% [ $n = 218$ ] vs. 85% [ $n = 679$ ];  $\chi^2_{(1)} = 20.5$ ;  $P < 0.001$ ).

A strong relation exists between the content and identity of ties (Cramer's  $V = 0.43$ ;  $P < 0.001$ ). Weak social ties tend to be predominantly conflictive (61%,  $n = 465$ ), and this against only 21% ( $n = 257$ ) of the strong social dyads and 16% ( $n = 86$ ) of the instrumental dyads. Strong social ties tend to be cooperative (42%,  $n = 257$ ) or neutral (38%,  $n = 257$ ), while instrumental ties are predominantly neutral (69%,  $n = 86$ ). Contentious court cases are less likely than noncontentious cases to have instrumental dyads (7% [ $n = 375$ ] vs. 21% [ $n = 674$ ]; Cramer's  $V = 0.18$ ;  $P < 0.001$ ). The distribution of the remaining dyads among conflictive and cooperative dyads does not differ significantly between contentious and noncontentious cases. There is also a significant difference in the identity of dyads between contentious and noncontentious cases (Cramer's  $V = 0.33$ ;  $P < 0.001$ ). Weak social dyads are also more common among

TABLE 2

## STRUCTURE MATRIX FOR DISCRIMINANT ANALYSIS OF CONTENTION

Variable	r
Regional position 4	-.39
Sampling frame	.30
Regional position 1	.30
Cooperative intravillage relationships	.29
Conflictive outdegrees	.27
Choice status	.23
Regional position 3	.21
Weak social outdegrees	.20
Strong social intravillage relationships	-.19
Cooperative outdegrees	-.16
Instrumental outdegrees	.14
Prominence	.13
Weak social intravillage relationships	.12
Strong social outdegrees	.06
Conflictive intravillage relationships	.04
Regional position 2	.03
Instrumental intravillage relationships	-.02
Group centroids:	
Noncontentious villages	-.40
Contentious villages	.93

NOTE.—The variables in this table are ordered by the size of their correlation with the discriminant function. Correlation coefficients with an absolute value of .19 or greater are significant at  $\alpha = .05$ .

contentious cases than among noncontentious ones (77% [ $n = 192$ ] vs. 52% [ $n = 616$ ]), while strong social dyads are much rarer (5% [ $n = 192$ ] vs. 40% [ $n = 616$ ], respectively). There is no difference, however, between contentious and noncontentious court cases as to the likelihood of intravillage or intervillage relations.

Contentious action therefore was mainly initiated by peasants who were hardest hit by changes in the moral economy of their villages and played itself out largely within networks of mainly weak social ties. Both opponents and supporters in a contentious case were more likely to be drawn from among one's circle of acquaintances (i.e., people with whom one interacted regularly but was not close to). This contrasts with the noncontentious cases in which relatively fewer peasants were involved and where kinship and friendship relations were prominent.

The results in table 2 further confirm the above interpretation. The structure matrix contains the correlation coefficients of the different variables with the discriminant function that distinguishes between contentious and noncontentious villages. The canonical correlation associated

with the single discriminant function is 0.52, and the function classifies 50% of the contentious cases ( $n = 34$ ) correctly while leading to only 13% false positives ( $n = 80$ ) among the noncontentious villages. This compares favorably with what could be expected under conditions of randomness, namely 10.1 correctly classified contentious villages and 23.9 misclassified noncontentious villages. The group mean score on the discriminant function for the noncontentious villages is  $-0.40$  compared to  $0.93$  for the contentious villages. All correlation coefficients with a size of  $0.19$  or larger are significant ( $P < 0.050$ ). A positive correlation between a variable and the discriminant function indicates that a higher score on the variable will increase the risk for contention.

The discriminant analysis indicates that strong social intravillage relations are significantly and negatively correlated with the discriminant function and thus tend to prohibit contentious action. On the other hand, conflictive relations with other villages increased the likelihood for contention. No significant correlation was recovered for strong social outdegrees. Both regional positions 1 and 3 increase the likelihood of contention, while position 4 tends to decrease it. As expected, the sampling frame (i.e., the number of court cases a village is involved in) is also positively associated with the likelihood of contentious action, as is the proportion of intravillage cooperative relation and the number of weak social ties to other villages.

Different villages follow different routes to contention. For instance, contention in blocks 1 and 2 seems mainly fueled by conflicts with other villages. The villages in block 2 on the average have more than 14 conflictive ties with other villages, those in block 1 have an average of four such relations, compared to only about one for blocks 3 and 4. On the contrary, the large number of strong social ties to other villages in block 2 reduces the chance of contention. The low reliance on other villages for support and status in the villages of block 1, as expressed in low prominence, further enhances their contentious capacities.

The lack of strong social ties within the villages of block 1 also contributes to their contentious potential. Contention in block 3 on the other hand seems largely due to the presence of strong cooperative networks within the villages and is somewhat tempered by the strong social ties in the villages and the lack of conflictive ties to other villages. (See fig. 2 for map of the region.)

## DISCUSSION

The regional system reconstructed on the basis of the court records is consistent with the image of gradual market and state expansion, moving out from regional centers to more isolated villages in the region. During

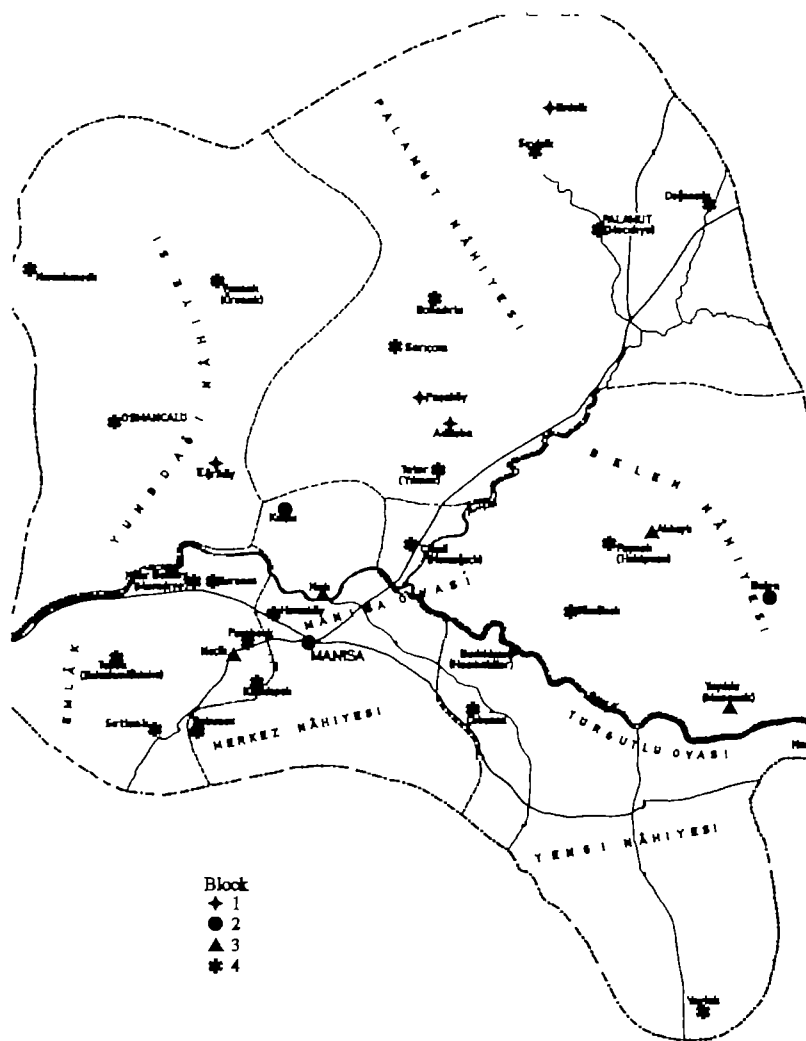


FIG. 2.—Map of the Manisa region (adapted from Emecen [1989])

the early stages of such expansion, integration of villages in the emerging national structures will vary widely, reflecting the degree of integration in the regional system.

This interpretation is further supported by the economic, social, and geographical characteristics of the villages. The expansion of the market economy and the central state in the Manisa region followed the preex-



isting ties among the villages. The penetration of these processes affected the traditional moral economy of the villages in question, threatening the status quo. Some peasants saw their livelihood threatened by the economic changes as they became exposed to national and international competition, were forced to switch to the production of cash crops, and felt the burden of increased taxation. Others may see the existing order as an obstacle to making use of the opportunities offered by integration in the national economy.

For instance, many of the intermediate villages responded to economic and political pressures by increasing the production of cash crops, like cotton or rice, for the national and international markets. These changes in economic production were accompanied by increased state control over production and distribution as well as increased taxation (Goffman 1990, p. 38). Such processes occurred in both intermediate blocks, blocks 1 and 3, especially with cotton, sesame, and rice cultivation. While these cash crops put strains on the existing village economy, their impact on the peasantry was quite different: cotton and sesame production promoted entrepreneurship among the peasants while rice production further strengthened the servility of the peasantry.

Both cotton production and cotton weaving in block 1 were subject to tight state control up to the end of the 16th century. During the 16th century, state documents testify to the necessity for cotton in order to make cotton cloth, shirts for slaves, and cotton sails for the imperial navy (Emecen 1989, p. 253). Cotton, therefore, was sold to the state below market prices. Even in the first quarter of the 17th century, the Aydin-Manisa-Bergama area regularly provided the state with 44,000 pieces of cotton cloth a year to make the undergarments of the Janissaries (Faroghi 1994, p. 459).

State control and below-market prices raised the locals' incentive to respond to a more lucrative international market in the form of contraband cotton yarn smuggled out of the empire from several western Anatolian ports. By 1623, the Ottoman policy of regulation of cotton export was reversed and Venetian merchants started buying increasing amounts of cotton for lower prices (Faroghi 1994, p. 458). As a result, peasants increased their cotton production. As the Manisa region was a center in cotton production, the late 16th and early 17th centuries constituted a transitional phase for villages active in cotton production.

Increased exposure to market forces brought additional uncertainty for peasants and petty craftsmen. The peddlers, merchants, and middlemen bypassed traditional institutions in marketing the local products, leading to numerous complaints on the part of weavers and other craftsmen suddenly confronted with shortages of raw material and unemployment. For example, the weavers of Manisa complained in 1607, while, two years

later, cobblers complained, and, in 1612, leather manufacturers joined the protest to the courts about the lack of raw materials (Goffman 1990, p. 71). The government response to the local *kadi* underscored that weavers should have the first pick and purchase the amount of cotton thread they required before merchants and middlemen were allowed close to the goods (Uluçay 1942, pp. 138–39).

Peasants were confronted with many problems. Peasants were now exposed to fluctuating market prices. At the same time, the overall taxes collected from the peasantry increased. As the demand for cotton increased, disputes over taxation and land became more prevalent. Peasants also confronted new conditions when rapacious entrepreneurs swindled them. Sesame, the second major cash crop of this region, was converted into oil. Past practice ensured that sesame seeds would be distributed to the peasants by the region's superintendent; in return, the peasant was obliged to give half his produce to the superintendent and sell the other half to the superintendent of the oil mill for a fixed price. But early in the 17th century, interest in sesame oil from the West disturbed this previous arrangement. Several intermediaries mushroomed: some local notables, others resident members of the military, all interested in new opportunities for profit. They opened their own oil mills, took the produce away from the peasants, and sold the oil to foreign merchants (Uluçay 1942, p. 42). Court cases attest to the discontent of the peasantry confronted by this transformation from a protected order to one subject to individual and market whims.

Contention was the result of both the larger commercial transition that occurred and the type of cultivator affected by this transition. Much of the contention in block 1 was related to the presence of outside cultivators (*hariç reaya*). Court cases as well as tax records describe the outside cultivators as most likely "single men worthy of land," poor peasants with little land, and nomads moving along the bounds of villages (Inalcik 1959; Emecen 1989). Despite their merit as cultivators in relatively deserted villages, these landless, single, and vagrant youth were also the cause of much instability. They moved around, and state officials followed them, sometimes registering them in multiple localities. Most cases reflected the struggle between these itinerant peasants and state officials regarding tax payments.

A contentious case, for example, was brought by a tax collector against peasants and nomads for the failure to pay their taxes. The taxes in question indicate the status of peasants and nomads: the *resm-i bennak*, the tax for those who have very little or no land (therefore were not settled agriculturists), or *resm-i mücerred*, the tax for single, unmarried men (again, mostly unsettled agriculturists), and *aded-i agnam* taxes, for sheep. Peasants and nomads contested these taxes on the grounds that they really

belonged to another part of the province, paid taxes there, and should not be liable for taxes in Manisa. They finally brought a title-deed to this effect and won the case. The contention as well as the confusion regarding their dues arose precisely from the fact that some of these single and land-poor men were moonlighting in several villages and were moving together in groups with nomads from different tribes. They therefore appeared on record as both members of several villages and nomadic tribes and were subject to double taxation.

The expanded market for cotton, combined with the availability of unsettled land, provided opportunities for young men who wanted to get ahead. That these villages were either market villages or villages located on rivers, roads, and waterways facilitated movement and the search for more cultivable land. As these young men cultivated in various villages, the tax officials had a hard time finding them. Their unsettled patterns and their limited tax liability also increased conflict with other peasants about shared tax burdens. Also important is that, in settlements where the main agriculturists were vagrants and unstable men, one cannot expect high levels of intravillage organization. This is confirmed by the low average scores on the intravillage relation indicators for block 1 (see table 1).

Like cotton and sesame, rice cultivation led to increased state control and market exposure. But the effects on the peasantry were different. Like cotton production, rice cultivation was potentially lucrative, and the Ottoman state both encouraged its cultivation and attempted to control its producers. But very often, rice patties (*çeltük*) were owned (as freehold property) and directed by grandees, rich officials with enough cash to invest into the initial development of rice cultivation. Once they developed such agricultural units, the state helped them keep the peasants in their unit under semibondage. The rice patties resembled plantation agriculture where a servile peasantry was under strict control (Inalcik 1982). As the grandees responded to the increased pressures of the market, peasants found themselves squeezed for more production.

Rice cultivation required different organizational arrangements than other cash crops. It was more labor-intensive, more onerous, and the tendency for peasants to want to flee prompted stronger organization and supervision (Inalcik 1982). Often rice production was carried out as part of conglomerates, with a few villages and *mesra'as* (periodic settlements) assessed together for tax purposes. In block 3, rice cultivation, collective taxation, and pious endowments ensured strong intra- and intervillage organization. Generally, when states engage in the collective taxation of villages, they generate intervillage organization as a by-product. Villages designated as income for a pious endowment were similarly forced into a different social order whereby peasants organized more intensely inside

the village. This higher level of organization is confirmed by the contentious cases in the court records. All of the cases involving block 3—where agricultural and administrative practices enhanced collective organization—dealt with the *avariz* tax whose burden was shared collectively. All these cases involved free riders in the villages, brought to justice by their neighbors and covillagers. This intermediate region, then, was more organized on account of its production and taxation structure. And, given better organized networks, peasants were clearly better able to spot free riders.

Successful development led to further efforts for more growth. As they expanded their cash crop potential, many villages extended their boundaries to incorporate unsettled lands in their vicinity; Ali Beyli, from block 3, was just such a village that incorporated land, showing an immediate tax increase of 500 *akçes* in the records. Confident that they had become significant, the villagers of Ali Beyli promptly appealed to the central administration requesting that their village become a designated market town. Such requests attest to the fact that many villages in this intermediate regional position developed and took advantage of the market opportunities they perceived.

These peasants, some who tried to contain the market, others who tried to conquer the market, were made of the same cloth Eric Wolf describes when he refers to the middle peasantry (Wolf 1969). Wolf stressed how this intermediate class of producers with enough resources to have been exposed to the vagaries and the benefits of the expanding world-market became the most vulnerable and the most rebellious section of the agricultural community. Whether involved in cotton or sesame or other cash crop production, peasants in western Anatolia were both pushed and pulled by market forces: pushed because they perceived benefits for more production, hampered because they had to fight multiple other claimants to these new benefits. It is the beginnings of this struggle that the court records so dramatically portray. These struggles are most pronounced in the intermediate villages and less so in the central and isolated villages. Because of their position in the regional system, intermediate villages were more vulnerable to disturbances by market and state penetration.

## CONCLUSION

In conclusion, recent scholarship on peasant collective action offered much evidence of the effect of market and state expansion on peasant discontent. They have offered much evidence on the organizational structures that best promote or harm collective action. They have also speculated on the class background of the contentious and rebellious troublemakers.

In this article, we have shown that regional structure, the position of villages in the larger regional agrarian system, was an important factor in explaining contention among villagers, as it provided reasons for contention. The processes of state and market expansion altered the regional structure, ensuring that some villages, closely tied to the center became quickly integrated, while other isolated villages remained largely outside of these processes. This focus on processes of state and market expansion is not new. This article is novel in that it links regional structure and processes of market and state development to contention. The position of villages in the regional system shaped how much or how little peasants were affected by change. Villages in the intermediate positions were most affected by the 17th-century developments.

The western Anatolian coast of the Ottoman Empire, and the empire more generally, began responding to economic developments in the West in the late 16th and early 17th centuries. The western Anatolian coast developed seaports from which goods were sent off to Europe. Inland centers of production, whether agricultural producers of raw materials or artisanal producers of finished goods, responded to the demand from beyond. They juggled continued state demands for cheap goods and novel international demands. In the process, the old patrimonial institutions and the layers of protective social fabric were ripped apart, leaving artisan and producer vulnerable. This is when the appeals to the local judge and their appeals to the central state intensified to control the profiteers, the merchants, and the middle men. Also, court cases increasingly demonstrated the willingness of peasants to challenge state decisions regarding taxation. Peasants walked long distances to make sure that the increased amounts were really needed by the sultan.

Not all segments of the rural population pursued this appeal; the outcry and agitation was most prevalent in those villages most affected by these changes. Intermediate in their regional structural position, they encountered some benefits of the changes but also met with increased exploitation and control. Cultivators in these intermediate villages increased production; sometimes it was taken away by various new claimants and at other times, they increased production under duress. It was, then, those villages that both encountered the benefits and the harmful effects of the market and the state simultaneously that had reason to display contention.

A village's regional position is a good proxy for its exposure to processes like state expansion and commercialization. Isolated villages escape these pressures while central villages adapt much more rapidly and also tend to be the first to benefit from commercialization and state expansion. Intermediate villages get caught in the middle; exposure to these processes changes their internal structure while as a whole they do not immediately benefit from increased integration. This in no way means that contention

is a villagewide activity. Even during the most massive uprisings elsewhere, the majority of the village population tends to remain on the sidelines. Only under extraordinary circumstances will contentious action mobilize an entire village. Much more frequent were the individual and small group forms of contentious action. The Ottoman system provided an outlet through the court, preventing large-scale contention.

We have also shown that village organization matters in contentious action. Everyday interaction, rather than class solidarity or formal organizations, form the basis for organization. Ottoman villages in the 17th century lacked institutions that brought residents together; many of those institutions that were part of the provinces divided rather than united (Barkey 1994). Everyday interaction, however, breeds both conflict and cooperation, and both shape organization within the village. However, fragmented villages tend to be less conducive to contention as they pit the different factions in the village against each other. Cohesive villages are more likely to engage in contention. Again, village organization is not a new variable to explore for contention. Rather, the fact that we studied village organization and village conflict together differentiates our analysis from others. Organization in this context was solely based on everyday interaction in the village context, making this variable much less formal and much more the product of networks of association, friendship, and gossip in everyday interaction.

Position in the regional structure and village organization both emphasized the village as the unit of analysis, drawing us away from class and other bases of organization. A village has to be considered within its larger context. What happens within a village is to a large extent contingent on its environment. Environmental changes gradually penetrate the village, creating new tensions and opportunities. The regional position of a village reflects the configuration of its environment and its exposure to macrosocial processes.

The nature of the data shaped the analysis used in this article. In this instance, court records proved to be a good source of systematic data about the relationships within and between villages. The network data obtained from these court records allowed us to reconstruct the networks of everyday interaction to test for structural hypotheses on the occurrence of contention. Court records also provided us with the data on our contentious cases. We learned both who talked to whom and who came to court to challenge the landholders and the state. Court records, therefore, served as a unique source of data on crucial variables. This was due to the paucity of data regarding this period. But also, we saw this as an invaluable opportunity to actually match people, events, and complaints for the context of a preindustrial, preliterate society. Court records provided us with a

superb venue to carry out historical research within a network framework.

Finally, this article contributes further to understanding the history of contention in the Ottoman empire; we offer an alternative perspective to rebellion. Here, we acknowledge both that Ottoman peasants confronted serious socioeconomic problems in the 17th century and that they did not rebel. But we offer a portrait of these villagers as not complacent and subservient. They were not passive; they actively challenged the state and its officials when their interests were threatened. However, the forms of contention were drastically different from those in the West. For peasants in the Ottoman Empire, the courts played an important role in channeling contention through its institutionalized forms of conflict resolution. This was possible mainly because the courts were independent from the local landholding hierarchy and because the peasantry was keenly aware of the autonomy of the courts.

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# Generalized Exchange<sup>1</sup>

Peter Bearman

*University of North Carolina at Chapel Hill*

Generalized exchange, in which sections of a tribe exchange women in a cycle and thus guarantee social solidarity, was induced from models of the norms governing classificatory kinship systems. A blockmodel analysis of one aboriginal tribe yields sections that serve as marriage classes in a generalized exchange system, though the norms that govern kinship would fail to manifest, if followed, a cycle for exchange. Generalized exchange systems emerge from inequalities exogenous to the kinship system, specifically gerontocracy. Models of norms are weak predictors of actual exchange structures. Models of relations yield insight into the etiology of systems that build social solidarity from social exchange.

When I was in my mother's womb, social structure seemed a simple thing. (Gang of Four)

## INTRODUCTION

This article focuses on identifying the conditions under which social solidarity emerges from exchange relations. The empirical focus is on the observed behaviors of persons whose exchanges induce a stable social order. The setting (Groote Eylandt, an island off Australia), the persons (Aborigines), the language in which exchange is conducted (kinship), the relevant theoretical literature (classificatory kinship theory), and the values exchanged (women) are exotic for most sociologists. But many of the issues involved—the relationship between normative orders governing action and actual behaviors, the identification of micromechanisms that yield stable emergent structures, and the relationship between solidarity, exchange, and inequality—are central to problems we encounter in more familiar settings.

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### Models of Models and Models of Empirical Relations

Some deep theories about the nature of human society and the basis for human order have been advanced with classificatory kinship systems as referent (Homans 1962; Lévi-Strauss 1969). These theories are mainly derived from models of norms rather than models of actual social relations. The payoff has been the development of elegant models of exchange structures, which Lévi-Strauss and others argue yield social solidarity (Lévi-Strauss 1969; Ekeh 1974; Korn 1973; Uehara 1990; Yamagishi and Cook 1993). Yet the patterns of exchange studied and the solidarity outcomes theorized as their products tend to be *ideals*, not empirical realities. Because this article works with *tangible relational data*, it turns this tradition on its head and consequently yields some new insights.

Traditionally, kinship theory is derived from models of the normative rules governing marriage and descent in a society. In his classic statement, Lévi-Strauss (1969) argues that native norms provide a model of social structure. For Lévi-Strauss, the model is a grammar. Just as linguistic grammar is the structure of language that organizes the placement of types of words in sentences, the structure of kinship is the grammar that organizes the placement of people into social positions and the placement of sections (subsets of the population) in systems of exchange. Here Lévi-Strauss's argument is not subtle. It rests on the claim that the norms and rules articulated by natives, the grammar of assignments to social positions, are the social structure.<sup>1</sup> Thus it does not mean anything substantively if people fail to behave in the prescribed ways, that is, to act "ungrammatically." Just as many people speak improperly without the grammar of language being called into question, so it follows for Lévi-Strauss that people may act "irregularly" without the structure of kinship rules being called into question. To understand how exchange yields solidarity, structural anthropologists like Lévi-Strauss (1969) treat the grammar of kinship as the relevant social structure to be modeled.

The strategy outlined by Lévi-Strauss is powerful, but many observers have a hard time cutting through noise in the field to identify clear norms. For example, Worsley (1968, p. xxiv) notes, "I studied an Australian Aboriginal tribe [Groote Eylandt] where the knowledge of beliefs and rituals of 'their' culture on the part of most members was abysmal: as elementary and confused as that of the average Church of England congregation member." This sentiment is echoed in other studies of classificatory kinship systems, where a "correct" understanding of norms, prescriptions, culture, and ritual is seemingly held by only one or two informants. Such

<sup>1</sup> Lévi-Strauss (1963, p. 279), e.g., argues that "social structure has nothing to do with empirical reality, but with models which are built after it."

normative disorder might well be expected. Demographic constraints and contact with other societies with very different marriage systems combine to force natives to piece together scraps of various rhetorical fabrics in their search for an ideal normative structure coherent enough to regulate social relations and ensure social solidarity. Further, local solutions to classificatory "errors" are common, and these solutions are often at odds with the formal normative models developed to account for these systems (White 1963a; Rose 1960; Meggitt 1962). In real time (as opposed to genealogical time), classificatory kinship systems are very fluid. Perhaps quilt patterns can provide an analogy: although the formal "normative" pattern suggests a straightforward "nine patch," the local reality seems much more likely to yield a "drunkard's path," which, while nominally disordered, is in fact highly ordered.

In this article, I use a nonnormative strategy for identifying the structure of social exchange among a population of Aborigines living on Groote Eylandt in the early 1940s. That is, my model is not based on stated cultural norms regarding the exchange of women.<sup>3</sup> Instead, I model tangible, named kinship relations among residents of Groote Eylandt. The exciting finding is that, rather than disorder, I show that subsections of the tribe—induced on the basis of structural equivalence—appear as marriage classes that exchange women in a nearly perfect cycle.

Ethnographers living on Groote Eylandt found no clear *positive* prescriptions guiding marriage choice and, in fact, claimed that the native norms governing spouse choice on Groote Eylandt were so muddled that, if they were followed, the kin system that these Aborigines did recognize would be anarchic.<sup>4</sup> Despite this, on Groote Eylandt, the apparently free expression of marriage choices yields, in practice, increased constraint. Because of this, natives are bound by a supple form of social solidarity—chain generalized exchange.<sup>5</sup>

<sup>3</sup> By anthropological convention, I refer to marriages as the exchange of women. One could think about wife exchange as the exchange of brothers-in-law, but that is cumbersome. A problem is that wife exchange is not the same as the exchange of husbands, since men commonly have more than one wife. The reality of Groote Eylandt society is that girls (nine years old or so) were stolen, kidnapped, exchanged, or given away by older men (25–40 years old), so in this case the convention carries substantive meaning.

<sup>4</sup> Rose (1960) argues that the normative system as natives articulate it is largely chaotic and inconsistent. The normative demand of a classificatory kinship system is that the cumulation of roles be patterned across individuals. For the relation "male marries," there is no discernible pattern that governs kin classification beyond the exogamy restriction. This absence of pattern suggests anarchy.

<sup>5</sup> Other nearly perfect or perfect exchange cycles have been observed for exchanges of more traditional economic goods, e.g., the Kula ring system (Ziegler 1990; see Yamagishi and Cook [1993] for a recent review).

Modeling the actual structure of kinship relations reveals order in the pattern of marriages on Groote Eylandt, even though the natives do not recognize that women are exchanged in a cycle and even though the cycle that emerges bears no relationship to the cultural rhetoric claimed to govern their marriage choices. Furthermore, the "sections" that organize the exchange of women are not recognized culturally; they are nominally invisible to natives. Despite this, however, natives penalize those who violate this exchange structure, even though the various marriage choice norms they do recognize (and describe to ethnographers) have *not* been violated. These findings are unusual. Explaining how a system found predominantly in books appears in real life—where it is not supposed to—is the task of this article. In the next section, I explore the relevant theoretical issues, which set the stage for the empirical analyses that follow.

## THEORETICAL ISSUES

### Classificatory Kinship Systems and Structural Equivalence

Kinship is a social relationship. How others are understood as related to us depends, not on blood, but on the grammar of assignment.<sup>6</sup> This basic principle is evident in our own kinship system, where many individuals with quite different blood relations to us may all be classified as "uncle," and even more clearly in Australian "section systems," where individuals with identical blood relations to a focal actor are often classified in different ways (Lévi-Strauss 1969).

Classificatory kinship systems, which appear simple, are very complex. They rest on an enormous expansion of the kinship grammar, well beyond the more familiar Western systems (White 1963*b*). The key to this expansion is structural equivalence. Actors fully interchangeable, in terms of public relations with all others, are structurally equivalent. Structurally equivalent actors share the same social position. As a result, they occupy the same roles and hold similar kinds of obligations and debts to similar others. In modern settings, pure equivalence is rare and must be socially engineered: the army private and the bishop in the Catholic Church are two obvious examples. In elementary (classificatory) kinship systems, brothers, like privates in the army, are theoretically interchangeable, that is, they ought to occupy the same relationship to all others.

Thus under the logic of classificatory kinship, an individual's father's

<sup>6</sup>A kinship grammar is a set of rules about the organization of kin terms. For an analogy, consider the grammar of friendship in high schools, which asserts that the friend (*F*) of a friend (*F*) is a friend (*F*), so  $FF = F$ . If we find for some students that friends of friends are not friends but enemies (*E*), so that  $FF = E$ , then the normative grammar still exists but is not observed empirically.

brothers (uncles) would be classificatory fathers to him or her. It follows that mother's sisters (aunts) are classed as mother—they are classificatory mothers—and thus their daughters (cousins) must be classificatory sisters. A fundamental fact of classificatory kinship systems is that members of a tribal section are structural equivalents. As White (1963b, pp. 81–82) notes: "[Every] person in one [section] can agree on the relations of every pair included. Of course I can always agree on how two people are related to each other by putting myself in one of their places as ego, but it is only in a classificatory system that I as ego can group others in exactly the same clusters of equivalence as they do."

For our purposes, a central feature of ideal-typical classificatory kinship systems is that, as a result of the classificatory logic, *the only women that a man may marry are members of the section that provides women to his section*. Showing this requires a complicated algorithm (White 1963a; Boyd 1991), but the logic of classificatory kinship makes life simpler. The fundamental problem is knowing who one is to marry. People need some way to figure out where their spouse is coming from. But individuals do not need a mathematical model to do the right thing. They do not even need to understand the global logic. Instead, they only need to follow a normative rule that prescribes the class of spouses they may marry.

In classificatory kinship systems, all men should marry women in the same kinship relation to them; for example, their father's sister's daughter (FZD) and/or their mother's brother's daughter (MBD). There are many different possible systems, and each system defines a different class of eligible spouses. Whatever relations are normatively endorsed, men (or women) can know they are marrying a person in the right relation to them if they are in the right section and are not otherwise prohibited (White 1963a). Because the logic of these kinship systems is so consistent, members of a section will be structurally equivalent. Therefore potential wives (from a male's perspective) are (in theory) all members of a unique section. It follows that, if we are to identify the sections in aboriginal societies that exchange women, the core element must be that individuals in each section are, as reported in the kin system, structurally equivalent. For one tribe, I induce such sections and show that cyclic generalized exchange occurs even in the absence of clear normative rules prescribing it.

### Social Scarcity and Social Exchange

Why exchange in the first place? The answer emerges by focusing on what is uniquely social about social exchange. I start with Adam Smith. Smith ([1776] 1965, p. 15) argues that men distinguish themselves from other animals through exchange, noting that "it [exchange] is common to all men, and to be found in no other race of animals" and further that "nobody

ever saw a dog make a fair and deliberate exchange of one bone for another with another dog." Critics of Smith have already noted that it is reasonable to wonder how dogs would benefit from an exchange of bones, since a fair exchange presumes that the values given and received (in this instance, bones) are the same. Bones are bones. While this appears to be a reasonable criticism, it is naive and misses what is essential to social exchange—that it yields social solidarity. People exchange equivalent values. Dogs do not. Dogs just do not know how to benefit from the exchange of equivalent values. They cannot turn the exchange relation into a role structure. People can, and do.

If dogs could define *their* bones as "bones they cannot chew," an exchange of otherwise equivalent bones would make more sense. A negative proscription, "you may chew others' bones but not yours," induces a positive incentive to exchange one bone for another. Bones that are prevalent locally would become prohibited and therefore scarce. Prohibited (and scarce) values are useful for exchange, especially if all the parties need them. Of course, this is the case with the incest taboo that prohibits "marriage" with some kin relations. The incest taboo requires an exchange, the "gift" of a prohibited relation to another person. Because the values that are exchanged (sisters, brothers-in-law) are not objectively scarce in nature, and therefore need not be exchanged with others in the absence of a prohibition, the incest taboo may be seen as the first truly social or cultural act (Lévi-Strauss 1969). Smith was right, if for the wrong reason. Animals do not exchange values that are equivalent because they cannot prohibit themselves from using the values they have. That takes *thought*.

Men may give classificatory sisters who are prohibited, and therefore of no value to them as marriage partners, up to other men. The gift begs others to exchange their classificatory sisters in return. Yet the incest taboo only insists that men not marry their own "sister"; it does not require the "release" of sisters to other men. If sisters are "released," it is without guidance with respect to which men they should go. If men want a wife, the negative rule (you may not marry people in some kinship relation) insists on exchange but does not positively prescribe a structure through which exchange is to occur. Without a positive prescription, or a norm of reciprocity, givers of sisters have no guarantee that they will become takers of wives. A univocal gift is risky; it is an invitation to exploitation.

A negative norm (the taboo against incest) creates an incentive to exchange but fails to provide individuals or groups with a structure through which exchange can occur. For a structure for exchange to emerge, it is assumed that a positive norm—a norm of reciprocity—or a positive prescription (you may only exchange with persons B) is required. Exchange theorists (Lévi-Strauss 1969; Ekeh 1974; Yamagishi and Cook 1993) dis-



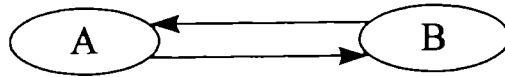


FIG. 1.—Direct (restricted) social exchange

tinguish two forms of exchange: direct (restricted) exchange and indirect (generalized) exchange. Direct, or restricted, exchange is the dyadic exchange of values, a gift given induces a direct reciprocation: a bone for a bone or, perhaps, a spouse for a bone. The key is that dyads exchange values back and forth between themselves; they form an exchange pair. Figure 1 represents this kind of exchange structure.

In indirect, or generalized, exchange, values move in one direction across a network of at least three actors. There are two main forms of generalized exchange. In this article, I focus on one type, "chain" generalized exchange, as distinct from "net" generalized exchange (Ekeh 1974; Sahlins 1965; Uehara 1990; Yamagishi and Cook 1993). "Net" generalized exchange, which involves exchanges between an individual (A) and a group (BCDE), is relatively common and is represented in figure 2. Play groups, carpools, revolving credit associations, and duplicate bridge games in which the players cycle through hosts are all examples. Reviewers of journal articles participate in net generalized exchange systems as well when they submit their own article for review. Chain generalized exchange is qualitatively different, as shown in figure 3. Whereas net generalized exchange can be reduced to direct "person-to-group" dyadic exchanges as in Blau (1964), chain generalized exchange cannot. The key in chain generalized exchange is that values have to flow through all parties in a cycle before a giver can become a taker, that is, receive a gift in return.

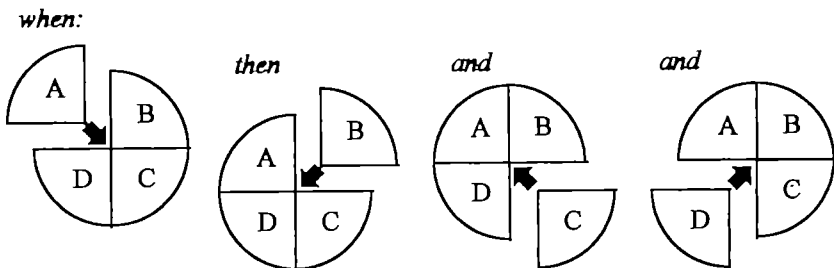


FIG. 2.—Indirect (generalized) social exchange: net form

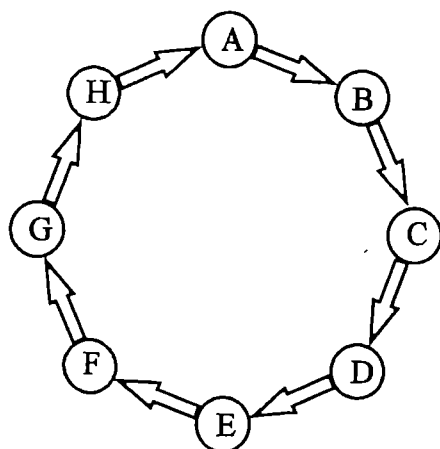


FIG. 3.—Indirect (generalized) social exchange: chain form

### The Norm of Reciprocity

Both generalized exchange and restricted exchange rest on a norm of reciprocity: takers are obliged to be givers. In direct dyadic exchange, the norm of reciprocity insists that takers give gifts to those who gave to them. The help I get with an article is a gift I return by an acknowledgment. I give the gift of attendance at a dinner party in exchange for the food the host provides. The norm of reciprocity provides guidance to both parties. Hosts do not provide food for guests who forget to come, and dinner parties would be few if hosts did not exchange food for the social efforts guests expend when they do arrive. Ambiguity over valuation of gifts received and taken may lead to subsequent streams of exchange (Leifer 1988). Dyadic exchanges governed by a norm of reciprocity lock actors into "endless" exchanges, as each alternates occupying giver and taker roles. These roles, and the exchanges within pairs, provide the basis for a weak form of social solidarity.<sup>7</sup>

In chain generalized exchange, the norm of reciprocity insists, as well,

<sup>7</sup> Within dyadic exchange, there will always be some transgressions of the reciprocity norm, but dissonance over the valuation of gifts given and taken rather than true violations of the norm is the central mechanism of exploitation. Others may be cads now and then, but even those who at first glance seem to build careers by transgressing reciprocity norms—grifters and confidence men—rely on them. "Gifts" from confidence men only work when the taken mistakenly think themselves to be takers, creating the irony that exploitation is easier if the norm of reciprocity is adhered to (Leifer 1988, Waller [1937] 1970).

that takers give. But they give to someone else. In systems of chain generalized exchange, many of the exploitation possibilities inherent in direct exchange are elided. This is no world for a confidence man. If he succeeds in eliciting a gift (in exchange), it goes elsewhere. Under generalized exchange, where gifts are univocal and givers are always givers, exploitation can take place only if actors explicitly reject the guiding norm of reciprocity. In this case, individuals or subgroups contemplating the exploitation of givers find little room for subtle action and thus face sanction from the entire group whose debt they have failed to repay.

As with restricted exchange, in chain generalized exchange, the norm of reciprocity guides actors to return gifts. But to whom should they give? When exchange involves sisters for wives, the incest taboo alone is no help. It does not positively tell actors who should be the recipient of the returned gift; it only excludes potential receivers. A cyclic system that fixes, for each dyad, giver and taker roles in perpetuity has certain advantages over restricted exchange systems. The role structure induced by cyclic exchange allows for the exchange of equivalent values without ambiguity. Giving is secured by the fact that subtle exploitation across dyadic exchange pairs is impossible. In contrast, restricted exchange provides opportunities for exploitation within pairs of exchange partners because actors on the outside may be largely indifferent to the exchange outcomes they observe. Because, under restricted exchange, self-interest is largely independent of other pairs' exchanges, actors are under no obligation to enforce collective norms. Two things result from this indifference. The first is that exploitation may characterize dyadic exchanges, thereby weakening one party or group by allowing inequalities to emerge between pairs. The second is more troublesome for group solidarity; interacting pairs may cleave away from the whole group, thereby creating a new, and self-sufficient, exchange system.<sup>8</sup>

Generalized exchange systems block these inherent tendencies toward subgroup cleavage. First, each section in the system cares a lot about every exchange. They are all invested in preventing dyadic exploitation. Secondly, in a cycle, there is no endogenous or structural basis for subgroup cleavage. Failing to adhere to the norm of reciprocity drains everyone of a scarce resource and, as a result, "allows the realization of more supple and effective solidarity" (Lévi-Strauss 1969). As a result, generalized exchange systems, once established, should be remarkably robust, for the only threats to their stability are exogenous in nature. Such systems are

<sup>8</sup> Note that exploitation is not a necessary outcome of restricted exchange. Here I simply argue that under certain circumstances (e.g., when group sanctioning mechanisms are weak), restricted exchange may provide a framework from which exploitation occurs.

presented as ideals, as logical outcomes of normative structures. Nobody really thinks they are real, but if they are, what accounts for them?

## CONTEXT, DATA, AND METHODS

### Groote Eylandt

In the 1940s, approximately 350 Aborigines lived on Groote Eylandt and neighboring Woodah and Bickerton Islands. Population density was one Aborigine per three square miles, three times the average for mainland aboriginal tribes (Rose 1960). By aboriginal standards, food and water were plentiful. In common with mainland aboriginal tribes, the sexual division of labor was rigid. Prestige and status were based on age and gender. Gerontocracy was pronounced, with older men controlling access to women, ritual knowledge, and material resources. Natives lived in kin-based cooperating groups composed of two or three adult brothers and their dependent wives and children. In two exceptional cases, single-family units lived in isolation from all other groups.

Residence was patrilocal; descent was patrilineal. Groote Eylandt Aborigines recognized 11 sections, loosely associated with locality. Sections were assigned to one of two moieties. Many natives could not correctly identify the section they belonged to (Rose 1960). In addition to section, natives belonged to totemic groups called the "dreamtime." The dreamtime connects individuals together on a different basis than "straight" kinship. The dreamtime is a ritual world; if kinship is economics, dreaming is comparative literature. Assignment of Aborigines to these totemic groups was determined by the mother's geographical location on the island when she publicly acknowledged conception. Since residence was patrilocal, the totem of a child was generally associated with his/her father. Gender, age, section, and totemic group are the only forms of social differentiation—other than kinship—discussed by ethnographers as recognized on Groote Eylandt.

### Norms Governing Exchange on Groote Eylandt

The rules governing spouse choice on Groote Eylandt are pretty vague. There are many of them, and they are contradictory. Because of this, different observers have argued that the Groote Eylandt system is similar to many different classificatory kinship systems found in Australia, from the Kariera to the Aranda. The problem is that a single society cannot coherently follow two or more contradictory normative systems at the same time.

For example, there is a norm on Groote Eylandt that men should marry

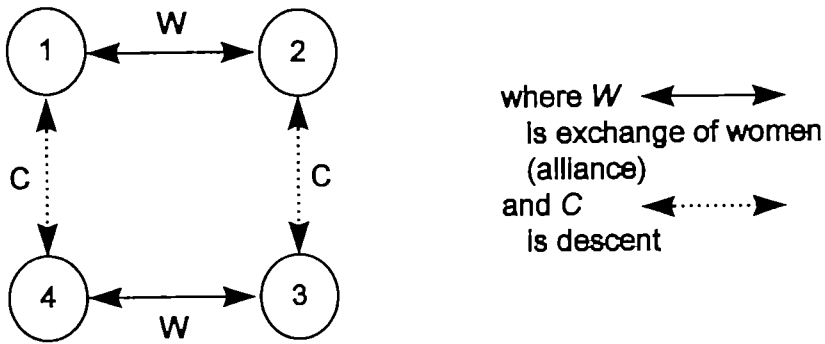


FIG. 4.—Kariera classificatory kinship system

their classificatory cross-cousins, their FZDs (who are also their MBDs). This norm suggests a Kariera system, in which classificatory cousins are exchanged dyadically across pairs of sections. Such a system is represented in figure 4. In the Kariera system, a male in section 1 marries a female in section 2. Their children are assigned to section 4 but reside with their father in section 1. Turning the pair around, a male from section 2 will marry a female from section 1; their children will be assigned to section 3 and will reside in section 2. A Kariera system rests on bilateral cross-cousin marriage with exchange of sisters. There is a lot of ethnographic evidence that the ideal marriage, given Groote Eylandt kinship terminology, would fit a Kariera model. Specifically, Groote Eylandt Aborigines not only do not prohibit FZD marriages, their stated norms endorse them. Furthermore, Worsley (cited in Rose 1960) argues that the "ideal pattern of kinship terminology implies an exchange of sisters." As I show later, while endorsed, such marriages are impossible in practice.

Leaving aside for the moment the norm recommending FZD marriage, Rose (1960), Worsley (as cited in Rose 1960), and Warner (1958) all suggest that other aspects of Groote Eylandt kinship terminology suggest the possibility of an Aranda-like kinship system. Specifically, there is another norm on Groote Eylandt that prescribes distant mother's brother's daughter (MBD) and mother's mother's brother's daughter's daughter (MMBDD) marriages. Such marriages are the basis for an Aranda system. Aranda kinship and descent systems have four lineages (two in each moiety), each divided into alternating-generation subsections that exchange women dyadically. Figure 5 describes the flow of wives across numbered sections ( $W$ ) and the assignment of children to sections ( $C$ ) in an Aranda system. For example, men in section 3 marry women in section 1 and women in section 3 marry men in section 1. Looking at the assignment

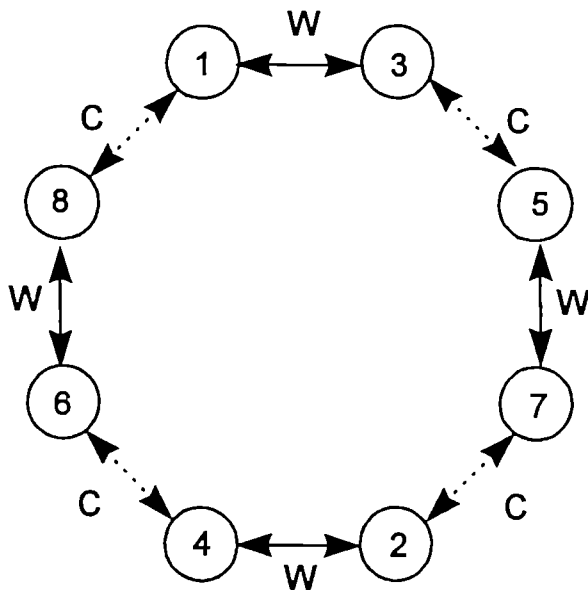


FIG. 5.—Aranda classificatory kinship system

of children (C), the children of a 1F → 3M marriage are assigned to section 5, while the children of a 3F → 1M marriage are assigned to section 8.

In an Aranda system, marriage with one's MMBDD (from a male view-point) ensures that the person that an individual is supposed to marry is in the right section. Note that under this system, marriage with one's FZD is prohibited. Therefore, the Aranda norm contradicts the Kariera norm. Both systems cannot work at the same time. Referring to figure 5, one can see that marriages with a FZD would be incorrect, whereas marriages with a MMBDD make the system work.<sup>9</sup>

Because on Groote Eylandt marriage with a male's MMBDD (who is also a distant MBD) is also prescribed, an Aranda system, like a Kariera,

<sup>9</sup> Consider, e.g., a male in section 3 looking for a wife. The normative rules tell him to marry his MMBDD. She is in section 1 because

his M is in section 7,  
his MM is in section 4;  
his MMD is in section 4;  
his MMBD is in section 6; and  
his MMBDD is in section 1.

Thus men in section 3 get wives from section 1, in exchange for sisters they gave to section 1 earlier.

is potentially supported by local norms. But again, *one society cannot be both Kariëra and Aranda*. And it gets worse. Other elements of the ethnographically revealed normative structure suggest similarities with other systems, the description of which is beyond the scope of this article. Rose (1960, p. 153) concludes that "the Groote Eylandt kinship system could very easily be described as a Kariëra, Aranda, Karadjeri, or possibly even an Ungarinyin type."

Two things are clear from this review of possible systems and their fit with Groote Eylandt kinship norms. The first is that the normative structure governing marriage choices on Groote Eylandt is too vague and contradictory to yield a *single consistent* exchange system. Incompatible systems are simultaneously endorsed, and the result is a hodgepodge.<sup>10</sup> The second is that there is no normative evidence for a cycle of exchange across named sections. Certainly it is hard to see how a system with 11 recognized sections could fit one of these ideal types. The only thing everyone can agree on is that men are prohibited from marrying women in their moiety.

#### Rose's Ethnographic Data

Kinship data, gathered *prior to detribalization*, is provided in Rose (1960) for 221 of the 350 natives living on Groote Eylandt, a 1,000 square mile island located in the Gulf of Carpentaria, off Northern Arnhem Land, Australia, in the early 1940s.<sup>11</sup> Thirty-two kinship relations—mother, mother's brother, older brother, mother's brother's daughter, sister, and so forth—are uniquely identified. As each relation has a role complement, mother/daughter, older brother/younger brother, accuracy in identifications can be estimated by consistency across pairs. Of all the identifications that were reciprocated, 98% were correct. Over 24,000 relationships (51% of the 48,620 possible) between the 221 persons surveyed were collected. Adults were able to identify and report their relationship to 77% of the Aborigines in the sample.

Rose's methodology was simple and innovative. He took photographs of Aborigines and asked others what they called them, for example, what kinship relation they were in. He collected data only on living Aborigines.

<sup>10</sup> Because groups cannot theoretically follow more than one kinship system at the same time without yielding disorder, it does not really help to argue that order comes from the fact that norms governing marriage choice may operate in some contexts but not others. Of interest are the mechanisms that effectively translate such micro-level "incoherence" into a coherent macrostructure.

<sup>11</sup> Rose fails to discuss selectivity associated with his sample. Missing are young men (10–13 years old), presumably not married, who spent their time in the bush and therefore could not be reached.

As a result, these data are different than those gathered by more traditional ethnographic methods, which rely on gathering normative information from a few informants. More traditional methods allow natives to articulate ideal images of the kinship system (who one should marry, ignoring actual behavior); Rose's data make possible tests of the fit between the ideal (normative) models of a kinship system and the practical reality of kinship relations as experienced by natives. Rose's kinship data are especially valuable since current research on aboriginal social structure is now complicated by detribalization. Life on Groote Eylandt at the time of Rose's fieldwork was probably very much the same as it was prior to Western contact.

### Relations and Aborigines Sampled

The population of interest is the 151 Aborigines on Groote Eylandt who were married or of marriageable age in 1941. For girls, this meant anyone over one year of age. Males typically first married girls aged 1-9 after they were 25 years old. The kin relations of interest for marriage exchange are cross-moiety relations. These are defined by Rose (1960, p. 60) as the "key relationships in the Groote Eylandt system."<sup>12</sup>

Data reporting each aggregate relation were transformed into five 151 × 151 matrices. These kinship matrices report whether or not an Aborigine is in a specific aggregate relation to another Aborigine. Thus, if an Aborigine in row *i* calls an Aborigine in column *j* "mother's brother's daughter" (MBD), a "1" indexes that *j* is MBD to *i*. Thus in cell *ij*, a "1" is reported. These five matrices served as the raw data for subsequent analyses.

<sup>12</sup> The relations selected comprise 20 unique English equivalents, identified below. Each is associated with a unique aboriginal name, e.g., MBD is named *denda* and FZD is named *dabura*, whereas in English we would say "cousin" even though we can distinguish them as FZD and MBD. Likewise, *denda* is also the term for "mother" and *dabura* is the term for "sister's sister." Rose collapses these detailed kin relations into aggregate categories and presents data on the categories of equivalent kinship relations. My models take these kinship data as their central input. These relations are defined by Rose (1960, p. 18) as

1. mother's brother (MB), mother (M), mother's brother's son (MBS), mother's brother's daughter (MBD)
2. mother's mother's brother's daughter's son (MMBDS), mother's mother's brother's daughter's daughter (MMBDD), wife (W), husband's sister (HZ), wife's brother (WB), husband (H)
3. sister's son (ZS), sister's daughter (ZD), father's sister's daughter (FZD), father's sister's son (FZS)
4. father's mother's brother (FMB), father's mother (FM), daughter's daughter (DD), daughter's son (DS)
5. mother's father (MF), mother's father's sister (MFZ)



TABLE 1  
GROOTE EYLANDT ABORIGINES BY MOIETY: POPULATION AND SAMPLE

SEX	AGE COHORTS (IN YEARS)					Total
	0-9	10-19	20-29	30-39	40+	
Moiety 1:						
Males.						
Population .....	19	12	10	11	12	64
Sample .....	0	3	3	7	10	23
Females.						
Population .....	12	14	7	4	9	46
Sample .....	11	14	7	4	7	43
Moiety 2:						
Males:						
Population .....	17	11	10	14	9	61
Sample .....	0	2	6	13	8	29
Females:						
Population .....	10	12	9	4	11	46
Sample .....	7	12	9	4	8	40

### Sample Characteristics

The sample of all married Aborigines differs with respect to age and gender composition from the population as a whole, as should be expected. These distributions are reported as frequencies, by moiety, in table 1.<sup>12</sup>

The large number of infant females and the small number of young men in the sample reflect the fact that men marry women who are on average more than 20 years younger than themselves. There are more women than men because of extensive polygamy. As men age, they acquire wives. The average number of wives (per married man) increases from 1.3 for men aged 21-30, to 1.8 for men aged 31-40, and to 2.9 for men aged 41-50 (Rose 1960, p. 69). Since men obtain more wives as they age, it follows logically that women are married polygamously as well. For women in their peak reproductive years (aged 15-28), 83% of their husbands have at least a second wife (Rose 1960, pp. 87, 247-465).

## RESULTS

### Modeling Groote Eylandt Kinship with Named Sections

In this section, I focus on modeling the pattern of wife exchange across the 11 named marriage sections Rose identifies as present on Groote Ey-

<sup>12</sup> Results reported in the body of the article are restricted to the 135 Aborigines currently married or promised as spouses in 1941.

landt. The goal is to identify a parsimonious model that captures the observed pattern of exchanges. Tests of different kinds of exchange systems are possible using log-linear models. The relevant modeling context is derived from work on social mobility (Bearman and Deane 1993; Sobel, Hout, and Duncan 1985); blocks (sections of structurally equivalent actors) are treated as classes in a marriage system, and the structure of the flow of women across these classes is the key focus.

I begin with the incest taboo, which requires men to marry women from the opposite moiety. Table 2 reports the mobility of women from origin (rows) to destination (columns) by culturally recognized, or named, section. Consider this mobility as the "gift" of a sister. An inspection of table 2 clearly reveals the presence of an exogamy rule that insists that marriage be with a spouse from the opposite moiety. The consequence of this normative rule is zeros in all within-moiety marriage cells. Since I am searching for structure in exchange beyond a negative prohibition, I treat these zeros as structural rather than sampling zeros in all of the models that follow.<sup>14</sup> Model fits are estimated using GLIM (Baker and Nelder 1978). Given exogamy across moiety, I test whether section of origin carries implications for section of destination. An independence model tests whether the association between origin (as sisters) and destination (as wives) is determined solely by the cross-moiety marginals—in this case, the proportion of persons assigned to one of the 11 sections that natives recognize. Call this a moiety model. A test of the moiety model, which fits a grand mean and the main effect of origin and destination,<sup>15</sup> fits the observed exchanges, yielding a log-likelihood ratio ( $G^2$ ) of 45.18 with  $df = 32$ ; and  $P = .061$ . This tells us that there is no deep structure to the pattern of exchange beyond moiety, at least using culturally recognized sections as the exchange units. Ethnographers were right to observe a hodgepodge, for a simple model of exogamous exchanges across moieties provides a satisfactory image of Groote Eylandt social structure, *using their own categories* as exchange sections.<sup>16</sup>

<sup>14</sup> It is possible to fit determined models with structural zeros easily (Lindsey 1989). Note that the degrees of freedom reported for all models are greatly reduced due to the presence of structural zeros.

<sup>15</sup> The equation for the moiety (Independence) model (Agresti 1990, eq. [5.2], p. 131) is

$$\log \pi_{ij} = \mu + \lambda_i + \lambda_j.$$

Since section 8 sent no women, I combined sections 8 and 9 to yield a 10-section system.

<sup>16</sup> Although there is no formal reason to expect that a structure for exchange exists across totemic groups, I tested the same independence model using totems as categories. The model fits exceptionally well ( $G^2 = 27.44$ ,  $df = 30$ ;  $P = .6003$ ). This means

Recall that aspects of the Groote Eylandt kinship terminology imply bilateral cross-cousin marriage with exchange of sisters, as does the absence of a prohibition against father's sister's daughter (FZD) marriage. These norms are consistent with a *Kariera* system, yielding restricted rather than generalized exchange. While there is normative support for sister-exchange marriages, due to demographic constraints, the chance of their occurrence is extremely low.<sup>17</sup> Nor is there any evidence in table 2 that the actual exchanges observed across sections yield an *Aranda* system, which assumes paired sections. Thus, although both ideal-typical models are suggested by the normative rules, the most we can uncover using culturally recognized sections is a moiety model.

This finding, that the observed structure of exchange, beyond strict adherence to an *exogamy* rule, does not fit the outcomes of formally prescribed norms, is commonplace. Norms are a model, an ideal. We should not necessarily expect them to be realized in practice. But something beyond *exogamy* (sidedness) does structure the exchange of women (White and Jorian 1997). In the next section, I go beyond identifying "sidedness" to show that the deep structure of marriage exchange on Groote Eylandt is cyclical across blocks of structurally equivalent actors.

### Operationalizing Structural Equivalence

Since it follows that, in classificatory kinship systems, sections that play a role in the social exchange of women are defined by structural equivalence, it should be possible to induce these sections (marriage classes) from a model that partitions the population into blocks of structurally equivalent actors. Blockmodeling (White, Boorman, and Brieger 1976; Boorman and White 1976; Burt 1980) is one method for generating structural equivalence in a social network. The basic strategy in blockmodeling is to induce a partition of a social network, where nodes are aggregated into blocks on the basis of similar patterns of relations. This aggregation of

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that there is no structure to the exchange of women across totems beyond *exogamy* by moiety.

<sup>17</sup> As Rose notes (1960, p. 177), "If exchange marriage were possible then in the ideal case we would expect that a girl of about nine years is married to a man about 34 and that girl of nine years has a full brother aged 34 years who receives as wife the first man's full sister also aged 9 years." Homans's (1962) argument that matrilineal cross-cousin marriage occurs more frequently than patrilineal cross-cousin marriage in patrilineal systems because of the positive sentimental ties a male has to his mother's brother cannot be right. The problems are demographic. First, under a gerontocratic social system, the male who is to marry his father's sister's daughter (FZD) when she reaches puberty has probably not yet been conceived. Second, even if he were born, his FZD is likely to be his classificatory mother (Rose 1960, pp. 120-23). On this basis we can reject a *Kariera* model.

TABLE 2  
THE EXCHANGE OF WOMEN ACROSS NAMED SECTIONS

ORIGIN	DESTINATION									
	Mosdy 1					Mosdy 2				
	1	2	3	4	5	6	7	8	9	10 11
1 Djaragba .. . . . . .	-0	-0	-0	-0	-0	3	2	0	0	0
2. Anwurugrigba . . . . .	-0	-0	-0	-0	-0	8	1	0	2	0
3. Jadigba . . . . .	-0	-0	-0	-0	-0	5	0	0	2	0
4 Badalumba . . . . .	-0	-0	-0	-0	-0	4	0	2	2	1
5 Bickerton Island 1 . . . . .	-0	-0	-0	-0	-0	1	0	0	4	2
6. Dalimbo . . . . .	2	3	1	5	8	-0	-0	-0	-0	-0
7 Amagula (Woodiah Island) . . . . .	0	0	0	1	3	-0	-0	-0	-0	-0
8 Umbakumba* . . . . .	0	0	0	0	0	-0	-0	-0	-0	-0
9. Bickerton Island 2 . . . . .	1	0	0	0	3	-0	-0	-0	-0	-0
10. Amulanwa . . . . .	0	0	1	1	0	-0	-0	-0	-0	-0
11. Bickerton Island 3 . . . . .	0	2	0	3	3	-0	-0	-0	-0	-0

NOTE.—Structural zeros are reported as “-0.” Three residents of Rose River are married to residents of Groote Eylandt. They are not part of Groote Eylandt’s section system and have been omitted from this table.  
\* Section 8 has no married women members

structurally equivalent actors into blocks means that actors have homogeneous internal relations and homogeneous external relations with others, for all relations considered simultaneously (Wasserman and Faust 1995).

The blockmodel output is an homomorphic reduction of the raw network data, referred to as an image matrix. A blockmodel shows how patterns of social relations in a population bind nodes into structurally equivalent bundles. Consider blocks as positions and an image matrix a model of a network of positions. This model is a model of social structure—but it is empirically grounded. In theory, the blockmodel is a pure distillation of the more complex social structure it represents. Since our social identities are shaped by the positions we occupy, a blockmodel is interpreted as an identity array generated from patterns of social ties linking persons in a social network.

The five aggregated kinship matrices were stacked on top of one another, to form a  $755 \times 151$  matrix. CONCOR, a commonly used block-modeling algorithm, was used to partition the stacked matrix (White et al. 1976). The first partition of the matrix yielded moiety, as expected. Three further partitions for each moiety yielded eight blocks, four in each moiety.<sup>18</sup>

After running the CONCOR algorithm, all Aborigines were assigned to one of eight blocks. All block members share moiety but are otherwise heterogeneous with respect to demographic characteristics and culturally recognized categories. Other than moiety, the cultural categories that are recognized by natives (section, age, gender, totem) are neither statistically nor substantively associated with block membership. Equivalence is thus not derived from shared categorical attributes. Below I focus on the structure of exchange (mobility) of women across blocks.

### The Structure of Exchange across Blocks

In theory, blocks of structural equivalents could function as sections in an exchange system, even if—as is the case here—these sections are culturally unnamed. On Groote Eylandt, there are no words that describe the induced blocks in the marriage exchange system, and therefore these blocks, while culturally constructed, cannot be culturally recognized. They neither fit the named sections nor the totems of Groote society. Earlier, I showed that native prescriptive norms governing marriage choice could never sustain a generalized exchange system. Yet such a system

<sup>18</sup> The cutoff used was relatively strict (.95). Correlations were computed on columns only, since 98% of all relations were symmetric. In other models, I attempted to yield the 11 section system that was recognized by the Aborigines on Groote Eylandt, but no partition reproduced their categories.

TABLE 3  
EXCHANGE OF WOMEN ACROSS BLOCKS

ORIGIN	DESTINATION							
	Moiety 1				Moiety 2			
	1	5	4	8	7	2	6	3
1	-0	-0	-0	-0	7	3	0	0
5	-0	-0	-0	-0	0	9	0	0
4	-0	-0	-0	-0	0	1	9	0
8	-0	-0	-0	-0	1	0	2	11
7	1	5	0	0	-0	-0	-0	-0
2	4	0	11	2	-0	-0	-0	-0
6	0	0	0	7	-0	-0	-0	-0
3	9	0	0	1	-0	-0	-0	-0

NOTE.—Structural zeros are reported as "-0."

emerges from practical behavior. Below, I show that natives manage kinship relations under the watchful eyes of a constraint imposed by the operation of generalized exchange, even though their tribal norms do not demand such a constraint.

Table 3 reports the mobility of women from origins (as sisters) to destinations (as wives) across the induced blocks, or marriage classes. Recall the unusual tabular structure from table 2, which placed structural zeros in the cells corresponding to within-moiety marriages. This tabular structure is replicated here.

Table 4 reports log-likelihood ratios ( $G^2$ ), degrees of freedom, and  $P$  values for models of the pattern of spouse choice across blocks. Following the logic of the section-to-section models, I begin by testing a moiety (independence) model. However, this model does not fit the observed data and can clearly be rejected ( $G^2 = 158.1$ ;  $df = 18$ ;  $P < .001$ ). The exchange of women across blocks is not random; this means that, unlike the section-by-section model, there is structure in the pattern of exchange beyond exogamy by moiety.

In an attempt to uncover this structure, I test all possible patterns of pair exchange (quasi-symmetry models) to see if either a Kariëra or an Aranda structure fits the observed data.<sup>19</sup> Recall that some norms on

<sup>19</sup> The equation for the sister exchange (quasi-symmetry) model (Agresti 1990, p. 354, eq. [10.11]) is

$$\log \pi_{ij} = \mu + \lambda_i^X + \lambda_j^Y + \lambda_{ij}^{XY},$$

where  $\lambda_{ij}^{XY} = 1$ , when  $i$  and  $j$  are paired, and  $\lambda_{ij}^{XY} = 0$  for all other values of  $i$  and  $j$ .

TABLE 4  
MODELS OF THE STRUCTURE OF EXCHANGE OF WOMEN ACROSS  
BLOCKS

Model	G <sup>2</sup>	<i>df</i>	P
Independence (random association) .....	158.1	18	0.00
Paired dyadic exchange .....		14	
1/5; 2/6; 3/7; 4/8 .....	180.3		0.00
1/5; 2/7; 3/8; 4/6 .....	144.5		0.00
1/5; 2/8; 3/6; 4/7 .....	150.2		0.00
1/5; 2/6; 3/8; 4/7 .....	254.8		0.00
1/5; 2/7; 3/6; 4/8 .....	129.2		0.00
1/5; 2/8; 3/7; 4/6 .....	139.6		0.00
1/6; 2/7; 3/8; 4/5 .....	155.7		0.00
1/6; 2/7; 3/5; 4/8 .....	122.4		0.00
1/6; 2/8; 3/7; 4/5 .....	149.5		0.00
1/6; 2/8; 3/5; 4/7 .....	122.0		0.00
1/6; 2/5; 4/7; 4/8 .....	170.5		0.00
1/6; 2/5; 3/8; 4/7 .....	133.9		0.00
1/7; 2/8; 3/5; 4/6 .....	104.5		0.00
1/7; 2/8; 3/6; 4/8 .....	121.2		0.00
1/7; 2/5; 3/8; 4/6 .....	118.1		0.00
1/7; 2/5; 3/6; 4/8 .....	127.5		0.00
1/7; 2/6; 3/8; 4/5 .....	135.9		0.00
1/7; 2/6; 3/5; 4/8 .....	128.4		0.00
1/8; 2/5; 3/6; 4/7 .....	120.4		0.00
1/8; 2/5; 3/7; 4/6 .....	123.9		0.00
1/8; 2/6; 3/7; 4/5 .....	128.1		0.00
1/8; 2/6; 3/5; 4/7 .....	142.7		0.00
1/8; 2/7; 3/6; 4/5 .....	118.9		0.00
1/8; 2/7; 3/5; 4/6 .....	111.3		0.00
Generalized exchange (cyclic) .....	5.8	10	0.59

Groote Eylandt suggested the possibility of a Karia or Aranda system, although the section-by-section exchange pattern did not show evidence of either system. Dyadic sister-exchange marriage insists on a symmetry such that, if women from block 1 go to men in block 7, then women in block 7 should go to men in block 1. An eight-block partition yields 24 possible cross-moiety block pairings, none of which fit the data ( $P < .001$  for all pairs). These models are reported in table 4. There is no evidence that dyadic exchange of women occurs across blocks of structural equivalents: marriage on Groote Eylandt is neither governed by a Karia nor an Aranda system.

Inspection of table 3 clearly shows that the structure of marriage across blocks on Groote Eylandt is a cycle. This cycle is represented in figure 6.

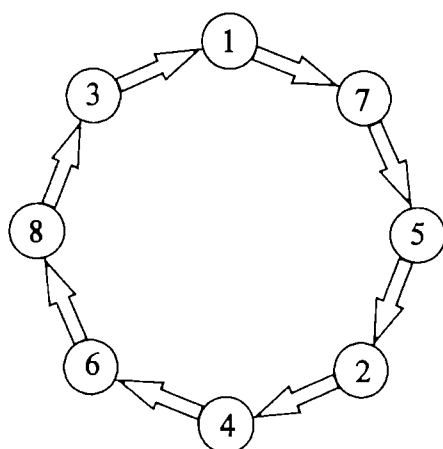


FIG. 6.—The exchange of women across blocks

It is possible to capture this cycle with a model that tests for the presence of generalized exchange. The generalized exchange model is a log-linear model that fits one additional parameter to the random association model to represent cyclic exchange. It is a quasi-independence model in which the marginals and cells corresponding to the cycle are fitted uniquely. The model posits no association among rows and columns for the remaining cells. This model is similar to the "topological" model introduced by Hauser (1978), which fits the contours of a mobility table. Viewed this way, the "contours" are the cells identified as receivers and givers under generalized exchange.<sup>20</sup> This model fits the data extremely well ( $G^2 = 5.8$ ;  $df = 10$ ;  $P = .59$ ). The improvement of the model fit over the independence and paired sister-exchange models is tremendous: the model reflecting chain generalized exchange accounts for most of the total association between origins as "sisters" and destinations as "wives."

### Inconsistent Relations

There are some clear inconsistencies in the exchange pattern reported in table 3. Four women from block 2 (moiety 2), marry a male in block 1,

<sup>20</sup> The equation for the quasi-independence model (Agresti 1990, p. 132, eq. [5.4]) is

$$\log m_{ij} = \mu + \lambda_i^X + \lambda_j^Y + \lambda_{ij}^{XY},$$

where  $\lambda_{ij}^{XY} = 1$ , when  $i = 1, j = 5$ ;  $i = 2, j = 6$ ;  $i = 3, j = 7$ ;  $i = 4, j = 8$ ;  $i = 5, j = 2$ ;  $i = 6, j = 3$ ;  $i = 7, j = 4$ ;  $i = 8, j = 1$ ; and  $\lambda_{ij}^{XY} = 0$  for all other values of  $i$  and  $j$ .



where, in order to yield a perfect cycle, the "correct" marriages ought to be with males in block 4. These four women are married to one male (identified numerically as #81) reported by Rose (1960, pp. 84, 80) as "by no means sure of his position and [he] apparently lived in daily fear that he would lose the girls, a fact which was apparently responsible for his living in virtual isolation." While #81's marriages (the result of stealing wives from male #46) did not violate stated norms (all were called "O," or wife), clearly they were perceived to be incorrect by natives. My model suggests why #81 was so uncomfortable.

Likewise, one of the two exchanges from block 8 to block 6 (where the "correct" exchange should have been to block 3) involved a woman (#68) whose husband (#168) was murdered by another Aborigine (#154) as he attempted to steal her. Rose notes that #168 called #68 "daughter's daughter" before she was stolen by #154. Subsequently, #154 was tried by ordeal for killing #168. This trial seems to have involved most of the males on the island and consisted of their throwing their spears at #154 while he stood facing them, unarmed, a short distance away on the beach.<sup>11</sup> That #154 survived was an unlikely outcome in a community of skilled spear hunters, unless community sentiment was on his side. While not prohibited, the marriage between #168 and #68 was exceptional and was "not looked on particularly favorably by the society as there was considerable feeling favoring #154 when he killed #168 in 1943 for 'wrongly' marrying #68" (Rose 1960, p. 149). My model shows that the marriage between #168 and #68 was incorrect. Natives "knew" this even though it violated no culturally stated norms.<sup>12</sup>

<sup>11</sup> Rose shows photographs of armed Aborigines facing #154 on the beach but does not describe the "trial." I conclude that the ordeal must consist of their using their spears.

<sup>12</sup> Clearly, as my *AJS* reviewers have pointed out, these marriages violated unstated "norms." Aborigine #81 was uncomfortable, yet he made marriage choices that were allowed. Likewise, #168's marriage to #68, while formally acceptable, was clearly considered "improper." Two things are happening. First, natives' understanding of local (unarticulated) rules appears in conflict with their formal (articulated) global rules. Second, natives collectively enforce unarticulated rules, even though they do not have the language to articulate them. These kinds of asymmetries between local norms and global rules are frequently observed. Here, by analogy, our understanding of microlevel (local) rules tells us that it is impolite to invite acquaintances over for dinner one day after having dinner at their house, even though inviting them over does not violate the global rule, which is to reciprocate invitations. I show below that a number of micromechanisms can provide the bases for local knowledge, which, when followed, are sufficient to generate the observed macrostructure. Note that in the Kula exchange, natives recognize the global structure explicitly and ignore local errors, as do we when, e.g., we account for promotions within firms as exceptionally sensitive to meritocratic criteria. Note also that there are other inconsistencies from the pure model of cyclic exchange, e.g., the "incorrect" association between a woman

If the exchanges that can be explained from the ethnographic accounts are removed from table 3, the generalized exchange model, as expected, fits much better, accounting for most of the association between origins and destinations. Whether one works with the "corrected" table or the original data, it is clear that a system of cyclic exchange occurs between unrecognized blocks of the tribe. This system ensures solidarity by binding all members into a chain of univocal prestations, embedding each block in a network of debt and obligation.

### Implications for Classical Kinship Theory

Aborigines who are blocked together because they share structurally equivalent patterns of kinship relations appear to be members of culturally unrecognized sections that participate in a system of generalized exchange. These induced sections have no connection to the cultural bases for social identity on Groote Eylandt; age and sex, section, or totemic group.<sup>22</sup> Still, they function as subsections of an elaborate exchange system in an underground economy of kinship. Implications of this finding are discussed below.

Discovering a generalized exchange system among real people in the absence of explicit cultural support is problematic for classical kinship theory. Little in the Groote Eylandt culture suggests, or points toward, generalized exchange. Ethnographers trained to see rules missed it even while natives enforced it. Ethnographers were absolutely right to see a formal mess, since models of the recognized normative grammar of Groote Eylandt kinship suggest that there was no clear structure for exchange—beyond that provided by the negative incest taboo (which yields exogamy by moiety). But it was precisely by focusing on natives' norms and prescriptions that classical exchange theorists were able to develop models of social exchange as a basis for human social solidarity. By far the most important setting for these theoretical efforts has been classificatory kinship systems, which have provided the grist for some classical models of human action, from both an individualist (Homans 1962) and structuralist (Lévi-Strauss 1969) perspective.

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in block 3 (#149) and a man in block 8 (#7), which cannot be specifically accounted for from Rose's ethnographic report.

<sup>22</sup> Alone this is an intriguing finding that reminds us that the promise of the network approach to social structure was that it might provide a metric to estimate the salience of cultural categories for tangible behavior. Often the validity of network results are based on overlap with categories. In this case, the validity of the blockmodel rests on the fact that CONCOR induced an exchange structure, which had only a prior theoretical justification. In this way, structural network analysis can be a tool for theory construction.

While neither the structuralist nor the individualist perspective completely works to account for the findings of this article, both Homans (associated with the individualist perspective) and Lévi-Strauss (associated with the structuralist perspective) point somewhat in the right direction. Homans was right when he recognized that norms do not matter as much as practical behavior. Likewise, his recognition that a stable macrostructure for exchange is likely to be the product of stable micro-level mechanisms is one that I am extremely sympathetic to and develop further below. On the other hand, his account of MBD over FZD marriage (preference) in classificatory kinship systems is falsified by the Groote Eylandt case, since Homans's micromodel, built from dyadic exchange alone, explicitly rejects the possibility of what we can observe—indirect, or chain generalized exchange. Exchanges in which giver and taker roles do not alternate within dyads cannot be analyzed as an aberrant form of direct dyadic exchange. Nor can other micromechanisms governing marriage choice, for example, familiarity, play a significant role in structuring exchange outcomes when the culturally recognized exchange units—section, totemic group, and locality—appear as independent of the observed structure. In light of these findings, Homans's theory does not provide a plausible explanation of the actual exchange pattern observed on Groote Eylandt.

Lévi-Strauss fares better than Homans. On one hand, I find an exchange system—chain generalized exchange—that he predicts *should* exist. More striking, Lévi-Strauss's prediction that generalized exchange occurs in harmonic regimes (systems with both patrilineal descent and patrilocal residence) is supported by the results of this analysis. This is significant since the linkage of disharmonic regimes with restricted exchange and harmonic regimes with cyclical exchange is absolutely central to Lévi-Strauss's argument in *Elementary Structures of Kinship*. It is a terrific achievement in science to predict *from theoretical grounds alone* a fact only later observed.

On the other hand, discovering generalized exchange where it is not normatively prescribed creates some significant dilemmas for the Lévi-Straussian argument. All societies prohibit incest, but few prescribe that men must marry women who are in a given relation to them. This is what classificatory kinship systems do. In section systems, it is the kinship terminology that defines the marriage rules and thus the principle of reciprocity (indirect or direct) that governs the society (Korn 1973, p. 15). On Groote Eylandt, chain generalized exchange occurs when these basic normative elements of the kinship system are muddled, contradictory, and incoherent. This should not happen, at least theoretically.

In *Elementary Structures of Kinship*, Lévi-Strauss (1969, p. 45) shows that "the woman whom one does not take, and whom one may not take

is for that very reason, offered up." People do distinguish themselves from dogs and other animals by prohibiting the use of their own values, inducing social scarcity, and forcing themselves to place values in circulation. This is a stunning cultural achievement, and so exchange does rest on the fundamental opposition between nature and culture. But the mere fact of an opposition between nature and culture tells us nothing about the social structure through which values, a bone here or a spouse there, are actually exchanged to yield coherent role relations between persons or groups. For Lévi-Strauss, coherent exchange systems must be built from positive norms governing the circulation of values that extend beyond the negative incest taboo. Because we are able to observe generalized exchange in the absence of such norms on Groote Eylandt, there is little support for the central idea that structures *for* exchange are also necessarily cultural achievements.<sup>24</sup>

In sum, Lévi-Strauss predicted the right thing (generalized exchange) for the wrong reason (normative rules). Homans was right (culturally stated rules do not matter, and local choice governed by some micromechanism does) for the wrong reason (indirect exchange does occur). I take what is right from both and, combining them, argue that there must be some set of nonnormative, yet "cultural" micromechanisms at play that are capable of generating indirect exchange. Discussing these mechanisms is the next goal. The relevant literature focuses on models that can account for emergent structures.

### Emergent Structures

For generalized exchange to occur in the absence of positive prescriptions, there must be one or more micromechanisms acting on individuals to guide their choice of a spouse with sufficient clarity to yield a consistent exchange system. Because neither formal accounts of the system nor chance events adequately tell us how practical behavior is routinely structured, it must be the case that the consistent operation of a micromechanism yields the emergent structure I observe.<sup>25</sup>

<sup>24</sup> Note that the problem is not the lack of fit between culturally recognized sections and the induced blocks yielded from the analysis of Groote Eylandt relational data, for Lévi-Strauss recognizes that natives cannot always name the units that exchange wives beyond the level of moiety. Rather, the problem is the lack of fit between stated norms governing exchange, which do not yield a cycle, and the observed exchange pattern, which is a cycle.

<sup>25</sup> We can manage to avoid accounting for emergent structures by enumerating the chance events whose coupling made it possible. Of course, amazing conjunctures of events and processes may yield robust structures as their products, but as explanation they stretch the limits of parsimony. Consider White's (1992, p. 243) characterization of the structuralist conjecture: "The structuralist conjecture is . . . illustrated by Lévi-

By emergent structures I mean informal structures of interaction that, while unintended and unnamed, are still robust, lasting well beyond the experience of any single individual or interacting population of persons in the cross section. In settings closer to home than Groote Eylandt, we are familiar with emergent structures of this kind: vacancy chains governing promotion opportunities in firms (White 1970; Chase 1991), balanced opposition and ranked strata-systems in high schools and the stability of clique structures within adolescent society (Coleman 1963; Davis 1967, 1970), marginality in informal social networks (Romo 1991), and so forth.<sup>26</sup> We generally observe that these emergent structures within densely interconnected complex organizations, like firms, high schools, and kinship systems, are frequently orthogonal to those posited by norms or cultural representations of underlying processes (Chase 1991; Kontopoulos 1993). Informal structures tend to cut across formal grids (Mayer 1960; White 1992). It should, therefore, be expected that, if we identify an emergent structure organizing spouse choice among Aborigines, it would cut across formal, normatively endorsed, expectations.

Many possible micromotives and micromechanisms have been proposed to explain emergent structures (for reviews, see Kontopoulos 1993; White 1992; Chase 1997). The micromechanism operating on Groote Eylandt must channel individual action into an observable exchange structure at the macro level. There is not much to work with on Groote Eylandt, aside from kinship relations. There is no ethnicity. Inequality is embedded within the kinship system. Beyond moiety, the extent of social differentiation is limited. Classificatory kinship insists that everyone get their kinship relations into line, so that they know who they are. They have to do it consistently since changes in a single relation will reverberate wildly throughout the system, ultimately involving all group members.

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Strauss's theory (1969) of marriage alliances in which the whole social organization of a tribe plays out within one of a few overall structuralist schemes of balancing among splits and alliances. Genesis is always the problem. . . . [By] what staggeringly unlikely concatenation of constituents did some such marvelously singular system of integration come into being?" Demographic constraint, chance encounters with new people peddling new ways of doing things, gerontocracy, the possibility of exchange cycles of other values circulating across persons or sections, etc., could all contribute in some unknown way to the appearance of generalized exchange on Groote Eylandt. Maybe an airplane full of trinkets landed, introducing a new currency and setting into motion a new exchange system. Who knows? It could have been this way. But, like native spouse choice, the problem with this style of explanation is ultimately about preserving degrees of freedom

<sup>26</sup> This is because people who are trying to get things done and solve substantive problems, such as whom to marry, whom to talk to in the school cafeteria, or who to line up behind in order to get a promotion, cannot afford to take normative self-representations seriously as guides for action. So they do not

Therefore, the mechanisms have to work on kinship relations, that is, on what Aborigines call one another. The mechanisms have to reflect practical interactions and be tied to practical behavior.

There is evidence that such a practical culture exists in opposition to those formal rules that natives articulate. Consider, for example, Rose's (1960, p. 142) argument that, on Groote Eylandt, "the usual Murngin method of reckoning kin does not always operate within the clan, for, when there have been wrong marriages, the practice of tracing descent through the mother and ignoring the father does not function. This keeps the kinship position of offspring of such a marriage adjusted to the organization of his group." If kinship does not line up the right way, the natives account for kin relations in a new way, thus adjusting, on the fly, the relational structure within the clan, and consequently, beyond it. The extent of local adjustment is significant: roughly 20% of all relationships observed by Rose had changed by the time of Worsley's fieldwork, just a few years later.<sup>27</sup>

I now turn to the identification of possible microlevel sources of cyclic exchange, focusing first on identifying operators that work to reproduce existing cyclic exchange structures, and second, on identifying operators that may be tied to the generation of such systems. The obvious starting points are two social-psychological theories that posit a clear relationship between microlevel behavior and macrostructure: rational choice and balance theory.

### Rational Choice and the Reproduction of Exchange Cycles

In an important article, Yamagishi and Cook (1993) show that, in experimental situations, actors in exchange cycles corresponding to a perfect system of chain generalized exchange will cooperate with others by offering univocal exchanges in expectation of subsequent cooperation of others in the cycle. Yamagishi and Cook (1993) show that cyclic exchange structures similar to that observed on Groote Eylandt assume the form of an *N*-person assurance game and are thus extremely robust, since it is always in each individual's interest to cooperate (by exchanging) rather than to defect (by refusing to place values into circulation). They thus establish that rational action is consistent with continued cooperation in cyclic exchange structures. Once established, cyclic exchange structures are self-reproducing from the rational actions of individual participants.

An especially important aspect of Yamagishi and Cook's (1993) experimental findings is that actors in an exchange cycle need not recognize the

<sup>27</sup> Personal correspondence with Douglas White, May 1996.

macrostructure in which they are embedded, in order to act strategically by cooperating through univocal exchange. The local views they have of the system (focused only on the receiving and sending pair) are sufficient to block free riding in a context of familiarity and trust. Local rationality can thus catenate exchanges into a robust global structure independent of cultural norms.

While rational choice can provide a plausible micromechanism for the reproduction of cyclic exchange structures, it cannot help us understand the etiology of such systems, for "strategic action" as a micromotor can yield any number of different social exchange structures.<sup>28</sup> Beyond this, cooperation, which generates trust as its by-product, is only rational in a context of mutual trust. Climbing out of this circle from within rational choice is not easy. What is needed is a dynamic model capable of accounting for the evolutionary emergence of cooperation across multiple iterations (Macy 1991; Kim and Bearman 1997).

#### Balance Theory and the Reproduction of Exchange Cycles

Rational choice theory is consistent with cyclic exchange once a cycle is produced. Perhaps not surprisingly, balance theory, which states that individuals (or interacting family units, as seen here) will structure their social relations with other actors in order to avoid cognitive dissonance (Davis 1970; Wasserman and Faust 1995), is also consistent with a pure exchange cycle. Given a cycle of size  $N$ , it follows definitionally that all of the constituent triads embedded in the cycle will balance. More strikingly, the preservation of balance across all  $N$ -sections of an exchange cycle is sufficient to prevent subgroup cleavage, as well as other forms of individual free riding. As long as each actor in the cycle locally adjusts his or her kinship relations to fit those within their moiety, they will jointly yield a macrostructure that reproduces the initial cycle. As with rational choice, actors may, but need not, see the macrostructure in order to achieve this. All that is required is the ability to organize one's within-moiety kin relations consistently. The exercise of balance at the local level—making sure, for example, that the person one calls "brother" calls one's sisters "sister," and one's mother "mother," and so forth—yields the reproduction of an existing exchange cycle.

<sup>28</sup> See, e.g., the work of numerous exchange theorists who show how different games yield different rational strategies and hence outcomes. It is worth noting here that, despite implicit claims to the contrary, one cannot infer microlevel strategies from observed outcomes, as multiple strategies that are rational within a single game can often yield the same outcome. Granovetter (1978) identified the same problem in his analysis of threshold models in collective action contexts.

On the other hand, as with rational choice, balance across the relevant sections of a kinship system does not have to yield a pure exchange cycle as a macrostructure. Thus, for example, both Aranda and Kariera kinship systems are locally balanced, yet neither yields a cycle similar to that identified in this article. Cycles are thus necessarily balanced, but balance does not provide by itself—any more than does rational choice—the sufficient condition for the generation of a pure exchange cycle. Neither micromotor by itself helps make sense of how such systems emerged.

### The Etiology of Cyclic Exchange

Neither balance nor rational choice alone provides a plausible and efficient mechanism for the evolution of trust and cooperative behavior necessary to generate an exchange cycle. Although both, once in place, provide an efficient micromechanism for the reproduction of an exchange cycle. How do cycles get started, then? One idea is that kinship cycles emerge out of a gerontocratic social structure.

In a brilliant article, "The Matrilateral Implications of Structural Cross-Cousin Marriage," Hammel (1973, p. 145) shows that, "independent of motivations, preferences, culture, symbols, or ideology," a subtle nonkinship age bias in the selection of spouses carries huge implications for the kinship systems that emerge. Specifically, Hammel demonstrates from simulation models of over 14,000 marriages that the mean age bias between spouses is strongly correlated (.79) with a matrilateral bias (here, MBD over FZD marriage) in cross-cousin marriages. Just focusing on societies in which the mean age difference between spouses is 10 years or greater, Hammel shows that on average, the matrilateral bias is .67. Furthermore, Hammel (1976, p. 164) notes that "an increase of one year in the mean absolute age bias increases the proportion of matrilateral skewing by 2 percent." Recall that the mean age difference between spouses on Groote Eylandt is strikingly large, averaging 18 years for women in moiety 1 and 17.5 years for women in moiety 2. Such age biases (with 24–27-year-old men marrying girls aged 0–1 year old) are the product of pronounced gerontocracy. Hammel demonstrates, and Rose confirms, that such age biases may generate a substantial matrilateral bias, itself a necessary component of a cyclic exchange system. Consider, in this light, Lévi-Strauss's (1969, p. 194) comment that "to claim, as Leach does, that a system of matrilateral marriage is not necessarily circular . . . is the same as asserting that a cyclist who kept the handle-bars of his bicycle turned in the same direction would not go round in a circle. . . . For a matrilateral system to be totally devoid of circularity, the number of 'local lines' would have to be infinite." Cultures need not recognize the impact of age skewing on marriage choices explicitly. On Groote Eylandt, all they



have to do, for example, is adjust such choices to the local kinship system, either by following the demands of local balance or by participating in a "strategic game" of rational alliance formation.

### Why Generalized Exchange?

All exchange systems yield solidarity as their by-product, as they embed actors in chains of mutual obligation and debt. But different systems provide different levels of solidarity. In direct dyadic exchange, exploitation can occur because of the norm of reciprocity. Skilled actors build on ambiguity over valuation in exchange and thereby profit from within the exchange relation. Those exploited by reciprocity norms may appeal to the group for redress, but other actors may be indifferent to exchange outcomes among other pairs and therefore fail to sanction the exploiter.<sup>29</sup> The inherent tendency in restricted exchange systems is toward increased inequality and differentiation between and within exchange pairs. Beyond this, the structure of a society bound together by dyadic exchange is at risk of subgroup cleavages. Thus the roots of endogenous change processes lie *within* restricted exchange systems. These processes heighten social differentiation on bases other than kinship and hence fuel their own breakdown.

In chain generalized exchange, on the other hand, where roles as giver and taker are fixed at the dyadic level, exploitation *within* exchange guided by the norm of reciprocity is limited. Equals exchange, and only a violation of reciprocity norms allows exchangers to obtain more values. Since these violations affect all actors equally, social solidarity is protected from subgroup cleavage and free riding, yielding a more secure form of social solidarity.<sup>30</sup> A macrolevel structure of chain generalized exchange, crafted from microlevel behaviors that reproduce balance, build social solidarity and trust, and serve individual as well as group interests, can presumably last for ages. Such structures appear immune to endogenous

<sup>29</sup> Note that, since bilateral exchange among sections is less an exchange between two individuals than between two groups, members of a group contemplating exploitation may be put under pressure from within their own group to conform. This kind of internal governance mitigates against pressures from within the exchange relation pushing toward subgroup cleavage. I am indebted to an *AJS* reviewer for raising this point.

<sup>30</sup> It might appear that in cyclic exchange, if block A sends women to block B, and block B to block C, and block C to block D, the failure of block D to send women to block A affects only block A (since A is then a net loser of women). But, D's failure to send women to A ultimately affects B, C, and D, since A lacks the means to send women in subsequent generations. Therefore, all actors are affected equally when the temporal order of exchange is ignored.

sources of change. Change, when it is observed, is likely to come from an exogenous shock. No wonder we found generalized exchange in a "primitive" society insulated from the chaotic events of modern life occurring beyond its borders. But we should not be too heartbroken by modernity's path, for it is not simply ironic that the most elegant structural systems for group cohesion seem to appear in social systems dominated by familiar structures of inequality (gerontocracy and patriarchy), which stand in an uneasy relation to our own (modern) normative claims.

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## Commentary and Debate

To conserve space for the publication of original contributions to scholarship, the comments in this section must be limited to brief critiques. They are expected to address specific errors or flaws in articles and reviews published in the *AJS*. Comments on articles are not to exceed 1,500 words, those on reviews 750 words. Longer or less narrowly focused critiques should be submitted as articles. Authors of articles and reviews are invited to reply to comments, keeping their replies to the length of the specific comment. The *AJS* does not publish commenters' rebuttals to authors' replies. We reserve the right to reject inappropriate or excessively minor comments.

### WHOSE SOCIOLOGY IS IT? COMMENT ON HUBER<sup>1</sup>

Many are troubled by sociology's status as a discipline, pointing to its apparent fragmentation, disintegration, lack of a core body of knowledge, the underemployment of its Ph.D.'s, the demise of select departments in the 1980s, and the challenges to others in the 1990s. Joan Huber ("Institutional Perspectives on Sociology," *AJS* 101 [1995]: 194–216)<sup>2</sup> offers a sobering, detailed, and informative analysis of the discipline, suggesting that the field has certain self-destructive characteristics that "increase the probability of attracting negative attention," including a weak core, some "affinity for antirationalist ideas" (p. 200), and a tendency to recruit reformists. Huber offers four basic strategies to address these self-destructive practices: develop a central core of knowledge, stop tolerating the antirationalists,<sup>3</sup> collect solid facts about society that will make us useful to the

<sup>1</sup> I thank Katherine E. Ryan, Norbert Wiley, and the *AJS* reviewers for comments on earlier versions of this statement.

<sup>2</sup> An earlier version of Huber's article was given in the report of the Task Group on Graduate Education (TAGGE 1992). ASA President William Gamson (1992) wrote a dissenting opinion. Huber's article essentially reproduces the original 1992 document (TAGGE 1992), without reference to Gamson.

<sup>3</sup> She uses the term "postmodernism" (p. 205) to encompass this antirationalist, relativist approach to sociology. Of course this is misleading. Nonpositivist, relativist, rela-

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welfare state, and follow a norm of civility. Following these steps will, she hopes, make sociology less vulnerable to budget cuts, show that we are keeping our house in order, and prove that we are doing "something that somebody wants and doing it better than our competitors" (p. 213). These four proposals require comment.

## Two Sociologies

First, of the nonpositivists, Huber states, "We should resist our admirable tendency to tolerate differences when it requires tolerance of perspectives that involve aggressive efforts to undermine everyone else" (p. 212). Not only are these people (the constructionists) uncivil, but they do not believe in truth or in the scientific method. She is clear on this: "An unknown proportion of sociologists feel that there are no standards of rationality, objectivity, or truth" (p. 204). Of course this is a caricature, and there is good reason why Huber names no representative of this position: no one holds it.<sup>4</sup> This unknown, unnamed group is then compared to another sociological group, those who believe "that sociology has a viable academic niche only as a science" (p. 204).

Thus is the field divided into two camps, the nonpositivists and the positivists. This division creates another, those who believe in science and who also hold to a conception of a disciplinary core consisting of demography, social organization, and stratification (pp. 203–4).<sup>5</sup> Persons working in these three core areas (plus statistics; p. 210) also produce "replicable data most needed to understand how societies work" (p. 204), they "supply the knowledge needed to run welfare states" (p. 213).

The constructionists resist this definition of a core (even perhaps of a society and its problems) and presumably contribute little to their understanding. Huber counters the nonpositivists with her version of positivism, arguing that sociology cannot be a science if it tolerates challenges to the belief in the "idea of the disinterested observer seeking objective

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tional, contextual, constructionist, interpretive approaches to science and social life existed before postmodernism appeared on the intellectual scene. Huber's conflation confuses the multiple meanings of postmodernism; i.e., as a movement in the arts, a new form of theorizing historical transformations since World War II, and a description of social life under late capitalism (see Mills 1959, p. 166).

<sup>4</sup> Even the most radical relativist holds to some set of criteria concerning truth and reason (see Schwandt 1994, pp. 130–31).

<sup>5</sup> Gamson (1992) suggests that the "core of sociology is political sociology, social psychology and sociology of culture" (p. 4), indicating that "the core of sociology is defined by the particular interests of the definers" (p. 4).

truth with universal validity that is based on the notion of a reality independent of human thought and action" (p. 204).<sup>6</sup>

For Huber, positivism is presumably equated with logical empiricism, a belief system that distrusts philosophical and moral inquiry and believes in a disinterested social science observer who applies rational rules to research, using methods to produce findings (Schwandt 1996). Constructionists question these rules and engage in moral inquiry. The chief rhetorical accomplishment of logical empiricism was its attempt to ideologically separate moral discourse from empirical inquiry (Schwandt 1996). This is what Huber attempts.

This is a questionable strategy. It again divides the field into two camps, shutting down complex, subtle arguments in the process, for example, those between positivists, postpositivists, critical theorists, and constructivists (see Schwandt 1994). At the same time, it valorizes one approach to truth and to science, thereby ignoring the many criticisms that have been brought to the positivist approach to data collection, hypothesis testing, and theory construction (Cronbach 1989; Rorty 1991). These criticisms include the problems of context stripping, the exclusion of meaning, the etic/emic dilemma, fitting general data to specific cases, emphasizing verification over discovery, ignoring the theory and value-ladenness of facts, verification versus falsification, the interactive nature of the inquirer-inquired dyad, and a problematic ethical system (Guba and Lincoln 1994, pp. 106–7; 1989, pp. 120–41).

During the 1980s, mainstream American, but not European (Giddens, Habermas, Bourdieu), sociology turned its back on the methodological controversies surrounding positivism, postpositivism, critical theory, and constructivism that were sweeping across neighboring social science fields, including anthropology (Geertz 1983; Rosaldo 1989), psychology (Cronbach 1989; Smith, Harre, and Van Langenhove 1995), and economics (McCloskey 1985; Heilbroner and Milberg 1995). These controversies challenged the presuppositions of objective social science, as well as traditional ways of bringing authority to that research, including the use of terms like "reliability" and "validity." Many came to reject the ontological, epistemological, and methodological presuppositions Huber appears to endorse. Gone were beliefs in ontological realism, objectivist epistemologies, and the use of quantitative methods to verify hypotheses. The notion of knowledge as accumulation was replaced by a more relative, constructionist position (Guba and Lincoln 1994, p. 114).

During the same time period the constructionists developed complex criteria for evaluating interpretive work, including credibility, plausibil-

<sup>6</sup> Even the most ardent logical empiricist rejects this belief. It is now understood that every observation is theory laden (Cronbach 1989; Rorty 1991).

ity, context embeddedness, dependability, confirmability, authenticity, dialogue, narrative truth, emotional verisimilitude, and so on (see Lincoln 1995; Altheide and Johnson 1994). Regrettably, Huber does not address this complex literature, except to dismiss it as relativistic (p. 205).

Indeed, Huber's stance effectively rejects this literature and its contributions to this discourse. But her perspective does not address the methodological biases that adhere to positivism. Who is to police those who claim they have the correct view of knowledge, truth, or science, and knowledge for whom? She lacks a way of determining the so-called objective truth of any assertion about the world. Her methods are directly implicated in the way the world is created and examined. These methods carry political and social biases, and she has no strategy, other than rhetoric, for guarding against them (see also Huber 1973).

### Value-Free Sociology

Second, under the guise of practicing objective, nonpartisan, value-free social science, Huber would take democracy out of sociology. Her model is taken from ecology, it has Darwinian overtones: "Administrators . . . must choose . . . between a semistarvation diet for everyone, or the starving of weaker units in order to give stronger ones a chance to flourish" (p. 195). This stance leads to her criticisms of those partisans who advocate intellectual relativism and the inclusion of students on administrative committees (pp. 201, 204–5, 207–9). These people and their ideas have brought "unclear standards, doubtful course rigor, a smorgasbord curriculum, and inappropriate graduate student participation" (p. 206) into sociology.

The very people she wishes to quiet, those drawn to nonpositivist approaches, came into the field, she argues, under suspicious circumstances. They have given sociology the image of being partisan. Huber wants them gone if they cannot contribute to sociology's mission (p. 213). These dissenters are calling for a pluralistic field that is broader than statistics, demography, social organization, and social stratification. They embrace alternative interpretive models of social science work, including the notion that science is a social institution that has values that often exclude or distort the perspectives of minorities, women, the poor, and the powerless.

These partisans suggest that a science must reflect multiple, interpretive perspectives, even, perhaps, a successor feminist science (Harding 1991) or a science that embodies the interpretive and epistemological standpoints of different racial and ethnic groups (Collins 1991; Smith 1993; Clough 1994). Acting as democratic communities, these new collectivities called for more ASA sections that would represent particular standpoint interests. They also suggested that departmental affairs include input

from graduate students.<sup>7</sup> These proposals, Huber argues, had the effect of decentralizing sociology's core (p. 208).<sup>8</sup>

The partisan's values make the field vulnerable to attack by administrators.<sup>9</sup> But when have we ever been value free? Furthermore, how are we to admit new persons into the field? Is there a new test that can be administered that will measure the degree to which a person adheres to Huber's values? In contesting these democratic moves, Huber seeks to reinscribe an organizational and disciplinary orthodoxy that will return the field to some historical moment when one view of sociology, its goals and central interests, held steady. This is nostalgia, for when did that state ever exist?

But it is not clear how Huber's proposals would address, except through unilateral imposition, the core problems she identifies, including consistent standards of teaching and research, norms of civility, course rigor, an extreme ideology of democracy, and the lack of a central core in the field. Furthermore, it is not clear that the problems she identifies are the problems that others (e.g., Gamson 1992), who take a more pluralistic view, would locate in the discipline. Indeed, Stinchcombe (1994, pp. 290–91) finds virtue in a disintegrated discipline, suggesting that it “represents the optimum state of affairs, both for the advance of knowledge and for the expansion of the minds of undergraduates.”

Of course, pluralism in sociology is nothing new; openness, even radical dissent characterized the discipline in its most fruitful moments (the 1950s and 1960s).<sup>10</sup> Ironically, there is even a radical pluralism within Huber's core that is embedded in the field's disciplinary history (p. 213), a minimalist canon of Weber and Marx (p. 213) that merges a qualified scientism (Weber's methodological writings) with Marx's radical reflexivity. These two paradigms have coexisted productively within the discipline throughout its history. Paradoxically, extremely narrow visions of sociology like Huber's are new. They can be seen as threatening the “traditional” pluralistic core of the discipline.

<sup>7</sup> Huber (pp. 209–10) appears to endorse the major recommendations of the TAGGE report: exclude graduate students from department committees, reject design-your-own doctoral programs (a special problem with students from abroad), develop a core program, undergo research training before the dissertation, require training in statistics for all program participants.

<sup>8</sup> During the decades in question (1960–90), all of the human disciplines experienced pressures from these same groups, which Huber contends helped to destroy sociology. One wonders, e.g., why anthropology, a field that embraced these diverse theories and epistemologies, flourished during this period, while sociology languished.

<sup>9</sup> Gamson (1992, p. 1), asks if this means that those sociologists who are critical “must either shut up, or adapt the political line of administrators?”

<sup>10</sup> I take the arguments in this paragraph from an *AJS* reviewer



### Sociology as an Interdisciplinary Project

Third, it can be argued that Huber's version of sociology is what got us into this mess in the first place. While no theory group dominated sociology during the decades she discusses (Wiley 1995, p. 152), it is clear that mainstream, empirical, middle-range sociology held sway. The discipline divided itself up into a series of subspecialties, including stratification, social organization, and demography. It is the work of these sociologists, not the nonpositivists, that has drawn negative attention to the discipline (e.g., Huber's anecdote about the two-sex life table; p. 202).<sup>11</sup> Huber engages in scapegoating by blaming the field's problems on the constructionists. Indeed her examples (pp. 202–3) of trivial research come from the mainstream, not the interpretive margins of the field.

Fourth, Huber's proposals stem from a period when departments and disciplines were exclusively "mission or generalist oriented," at which time their functions could be dictated by the needs of society as defined by a specific discipline. Over a period of decades, as disciplinary boundaries have blurred, departments and disciplines have become increasingly "domain oriented." This new emphasis reflects the more specific demands of a society increasingly dominated by, and dependent upon, knowledge that can no longer be provided by a single discipline. The opening up of sociology to diverse interests and to interdisciplinary programs addresses this shift in focus.

This democratic, pluralistic model of science, which Huber objects to, explicitly addresses one of her central concerns. It creates the conditions for producing a wide range of empirical materials that bear on "nontrivial social problems" (p. 213). Such diverse materials can no longer be provided by a single discipline or by a single paradigm within a discipline. Social problems-based, interdisciplinary research has achieved wide acceptance in mainstream sociology (Clemens et al. 1995, pp. 483–84). This interdisciplinary model better describes the actual workings of the field where "the advance of knowledge goes on with many different methods, many different theories, and with many different relations to ideological, granting agency, and theoretical objectives" (Stinchcombe 1994, pp. 290–91).<sup>12</sup>

Dichotomous (science, antiscience) and stereotypical thinking will not solve sociology's institutional problems. Nor can sociology's long legacy of

<sup>11</sup> A current example of this negative attention can be found in the media controversy surrounding the recent study (Laumann et al. 1994) of the sexual practices of Americans (see Lewontin 1995; Cassel, Schuman, and Lewontin 1995). This study has been criticized for its theories of human conduct and sexuality and its survey research methodology (see Chancer 1995).

<sup>12</sup> Consider, e.g., the list of books nominated for the distinguished scholarly publication award of the ASA in 1989 (listed in Clemens et al. 1995, pp. 484–88).

radical democracy be quieted. There is too much at stake to allow Huber's reading of the field to go unchallenged. Perhaps these new voices she fears will bring sociology back home to that vital core of concerns that C. Wright Mills (1959) called the "sociological imagination." I hope this will happen, for it is clear that objections to the nonpositivists in sociology involve more than disputes over epistemology or getting one's house in order. The material existence of an entire discipline is at stake.

It is too soon to shut down the conversation Huber feels can no longer be tolerated. Surely a common ground for reasoned, civil discourse can be found.

NORMAN K. DENZIN

University of Illinois

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# OF FACTS AND FABLES: REPLY TO DENZIN<sup>1</sup>

Norman Denzin's comment provides a welcome occasion to discuss an important issue. Sociology will benefit from inspecting his views. Basically, he holds that fables are superior to facts because appropriate interpretation enables one to make anything of the facts that one chooses. This questionable view, which animates antirationalist thought in academia and threatens and concerns many sciences, was among the issues raised by the ASA's Task Group on Graduate Education (TAGGE).<sup>2</sup> Although Denzin misrepresents a number of specific points raised in "Institutional Perspectives" (hereafter, the centennial essay), it is more important that we address the fallacies in his argument rather than review his inaccuracies in detail.<sup>3</sup> We first discuss Denzin's narrative strategem, then we discuss

<sup>1</sup> As coauthors, the order of our names is alphabetical. William Form and Barbara Reagin provided useful comments. Correspondence should be addressed to Joan Huber, Department of Sociology, 300 Bricker Hall, 190 North Oval Mall, Ohio State University, Columbus, Ohio 43210

<sup>2</sup> Task group members were Michael Aiken, James Coleman, Lois DeFleur, Barbara Heyns, Joan Huber, Kenneth Land, Stanley Lieberman, Franklin Wilson, William Julius Wilson, and Mayer Zald. The report, intended as a basis for discussion, was never distributed to ASA members

<sup>3</sup> This review is available upon request from the authors.

what it would mean if sociologists abandoned the attempt to conduct replicable research.

### Narrative Strategem

Denzin's argument follows his own dictum: If you want to change how things are, change how they are seen (Denzin 1992, p. 126). His response to the centennial essay demonstrates his mastery of postmodern sophist methods. For this argument he chooses the tool of specious innuendo. The derogatory implications, if stated clearly, are patently false. Let us look at each of them.

First, Denzin suggests that quantitative mainstream methods are an exercise in immoral repression of free thought masked as impartial scientific inquiry. In his view, people who reject intellectual self-disciplines such as measurement, reproducible method, logic, and mathematics are moral heroes; those who practice these self-disciplines and expect them of others are repressive villains. In fact, sociologists who practice scientific method often are motivated by deep moral commitment. Demographers worry about unchecked population growth. Stratification researchers feel anguish at the thought of racism, sexism, and poverty. What drives a person to spend decades acquiring the skills, the resources, and the knowledge required to advance a science? As often as others, perhaps more often, the scientists in sociology are driven by deep moral concern. But unlike others, scientists accept and practice a method based on the assumption that their personal beliefs may be false and must be tested. Where is the morality in demanding the right to have one's ideas accepted as valid regardless of how vague, contradictory, or unsupportable they may be? Who benefits from such a definition of morality? The answer is that it benefits ideologues, spin doctors, charlatans, and the cognitively diffuse. It benefits all who fail when their ideas are put to the test.

Second, Denzin suggests that quantitative, mainstream methods are thoughtless traditions formalized and transmitted from one unreflecting generation to the next. In fact, those methods are the product of a century or more of thought, criticism, and experimentation. This is not much compared to the centuries or even millennia spent developing the methods of other sciences but it is a start. The whole point of an explicit, formalized method is to aggregate the insights and discoveries of hundreds or thousands of researchers working in many places and times. Quantitative research arose as the solution to the evident weaknesses of earlier forms that lacked reproducible assessment, explicit comparison, and representative sampling. Who is it that dodges methodological criticism? The person who says, "Here's what I did; do the same and you should get similar results" or the one who says, "My case is special; my insights are unique"? It is

arrogance to see one's own impromptu methods as more thoughtful than standard methods. It is false to say that positivists ignore methodological criticism and hypocritical to argue that antipositivists need hear none. We think they should hear this: the "complex criteria for evaluating interpretive work" that Denzin recommends—such as credibility, authenticity, narrative truth, and emotional verisimilitude—treat an artful fiction as if it were true. They define an effective gloss, not a valid fact or theory.

Third, Denzin suggests that objective, nonpartisan, value-free social science is undemocratic or antidemocratic. What he means is that people have a right to think and do anything they choose. Thus, in his view graduate students have a right to study whatever they want any way they want and come to any conclusion that suits them without fear of being treated as lost or incorrect. This ignores the role of objective social science in a democratic society and misapplies the procedures for creating political legitimacy to the practice of research and teaching. Survey and demographic research are essential to effective democracy. Without them anyone can assert anything about what the public does, believes, or wants and fear no test of truth.

The public needs information in order to make informed decisions. Without it they are at the mercy of interpretive handlers. People need information that they cannot produce themselves on a variety of topics from crime rates to sexual practices to attitudes on abortion. Our job is to provide the information they need about their society in order to make their own decisions about their lives and communities. To do that job right, we must strive for relevance, clarity, honesty, and balance. It is not our job to displace their preferences or attitudes with our own. But when their ideas find little or no confirmation in empirical fact, we are obliged to say so. We should be polite and politic but we must be candid. To make this system work, the information we give them must be worthy of their trust. If we are nothing more than manipulative partisans of various causes, that trust will rightly disappear. And if it does, democracy will be weaker in America, not stronger.

Fourth, Denzin suggests that demographic and survey research undermines the interests of minorities, women, and the poor and that requiring quantitative skill and ability of all students oppresses those who identify with the abused, the disenfranchised, and the downtrodden. This is the sorriest falsehood of all. No interpretive commentary speaks of racism, sexism, and inequality with the effectiveness and power of facts. Nothing is sadder than to see a student who has struggled against a disadvantaged background, who has been given the hopes and resources of well-wishers along the way, fall into the trap of elective incapacity. Nothing is more unscrupulous than a professor who works students' insecurities, beguiling them into the trap. Denzin and others like him fight a ceaseless battle

against a science that reveals and condemns the falseness of their product. They demand the right and resources to generate a flood of quantitatively disabled sociologists primed with resentment and hostility to the self-discipline of the scientific method.

### Antirationalism and Scientific Research

The fundamental issue in this exchange concerns the part the external world plays in the confirmation of scientific findings. Can humans obtain reliable (albeit imperfect and tentative) knowledge by following the methods of science? Or is science impossible because human accounts of the world are only social and linguistic constructs? Is it fruitless to debate who got the facts right? Does every version of an event have as much validity as every other version? This last question is crucial because if the answer is yes, then scientific confirmation or replication is pointless. This question thus sorts out two camps far more effectively than do "positivist" and "nonpositivist," words that were not actually used in the centennial essay because a more sophisticated version of realism is emerging in the history and philosophy of science (Cole 1992, p. 237; see also Galison 1987; Giere 1988; and Hull 1988).

Difficulties in sorting out ideas in this area are compounded currently by the presence of two sociologies of science.<sup>4</sup> The older, influenced by Robert K. Merton, assigns external reality the most important role in determining the content of science; the newer tends to see the content as negotiated, influenced by power, political ideologies, and a host of social factors.

That social factors play a part in science is undeniable. Merton's ([1938] 1970) early work in the sociology of science demonstrated how social context helped to explain choice of topic and rate of advance. But Merton saw external reality as ultimately responsible for content (Cole 1992, p. 5).

Then, in the 1970s, after observing laboratory scientists at work, some British sociologists concluded that negotiation primarily or solely determined the content of science (e.g., Latour and Woolgar 1979).<sup>5</sup> This view, in many ways consonant with postmodernist thought about science, tends to be more popular in the humanities than the social sciences. In contrast, scholars in areas like physics or biology tend to see the notion that scientific findings are the result of negotiation as incomprehensible, alien to their experience. The main trouble with the idea of science as negotiation

<sup>4</sup> Lowell Hargens discussed these issues with us

<sup>5</sup> For a description of the extreme constructivist position, see Hamilton (1996, pp. 3–6).

is its utter failure to explain the obvious success of science—especially the success of science-based technology—since the 17th century (Giere 1988, p. 2).

This criticism is deadly. Is gravity only a social construction? Or, a less extreme position, is a physicist's discussion of gravity just a text?<sup>6</sup> In the context of sociology can, say, an observational study be assigned some degree of validity, or is it forever only one observer's construction of reality?

It is a matter of some importance to a social science discipline whether researchers can obtain reliable (if tentative) knowledge by using certain procedures. The centennial essay noted that an unknown number of sociologists feel there are no standards for such judgments. Denzin responds that the number is unknown only because no one holds such a position. But his answer is disingenuous, as the following tale will demonstrate.

The tale concerns an imputation of falsifying data brought against a distinguished sociologist and past president of the ASA. In his autobiography, William F. Whyte (1994, pp. 321–23) notes that he was informed in 1991 that the *Journal of Contemporary Ethnography* was considering a special issue on *Street Corner Society* because an article submitted to it had called into question the data on which the study was based and Whyte's interpretation of those data. The author of the submitted article had gone to the North End years later and interviewed some people Whyte knew and others he did not know. The editors would print the article if Whyte agreed to write a rejoinder and if they could recruit some behavioral scientists as critics. Denzin was one of the critics they recruited.

In his response, Whyte (1992) pointed to the critique's factual errors, its quotations out of context, other misrepresentations that reflected on its quality, and to the serious charge that he had falsified his data.<sup>7</sup> But Denzin never addressed the question of errors, misrepresentations, and data falsification. The editors (Adler, Adler, and Johnson 1992, p. 7) explained that Denzin, hewing to existential sociology and the postmodern creed of multiple realities that can never be fully reconciled, found the broad question of validity neither important nor answerable.

<sup>6</sup> Neil McLaughlin called our attention to a test as to whether a leading journal of cultural studies would print an article liberally salted with nonsense, e.g., that quantum physics is profoundly consonant with postmodernist epistemology (Sokal 1996a, p. 62). The fact that *Social Text* was taken in by "Transgressing the Boundaries: Toward a Transformative Hermeneutics of Quantum Gravity" (Sokal 1996b) made the front page of the *New York Times* (Scott 1996). A *Wall Street Journal* article (Kimball 1996) claims that the issue may finally convince college deans, presidents, parents, alumni, legislators, and trustees to look hard at the "politicized nonsense they have been conned into subsidizing."

<sup>7</sup> In our view, all charges against Whyte's scholarship lacked merit.

Denzin himself (1992, p. 124) began by noting that Whyte assumed that a social world exists and that events, meanings, and activities can be accurately recorded by skilled observers. Then Denzin asks readers to consider some "troubling alternatives": The ethnographer's text creates the subject. Subjects exist only insofar as they are brought into written texts. Each inscription on the memory disk is but another version of the event, hence one multiple inscription has as much validity as another, rendering fruitless the debates over who got the facts right. Denzin (1992, p. 131) concluded that it is now appropriate to ask if we any longer want this kind of social science, the kind of classic sociology that Whyte produced.

Responding to Denzin, Whyte (1993, p. 371) says that if his own approach, social realism, is only one of many possible strategies for telling stories about the world out there, then the critic must depend on a judgment of the author's persuasive power. Scientific arguments are thus transformed into literary criticism. The Denzin position, Whyte concludes, would bring the development of the behavioral sciences to a dead end. Credible interpretation depends on getting the facts straight (Whyte 1996, p. 225).

Sociologists have much to think about. The belief that social research is only one of many possible narratives takes sociologists altogether out of the business of trying to gather valid data. What would be the point?

JOAN HUBER AND JOHN MIROWSKY

*Ohio State University*

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# Review Essay: The Soviet Descent into Capitalism<sup>1</sup>

*How Russia Became a Market Economy.* By Anders Åslund. Washington, D. C.: Brookings Institution, 1995. Pp. xviii+378.

*Management and Industry in Russia: Formal and Informal Relations in the Period of Transition.* Edited by Simon Clarke. Brookfield, Vt.: Ashgate, 1995. Pp. xii+244.

*Workers of the Donbass Speak: Survival and Identity in the New Ukraine, 1989–1992.* Edited by Lewis H. Siegelbaum and Daniel J. Wal-kowitz. Albany: State University of New York Press, 1995. Pp. xvi+226.

Michael Burawoy  
*University of California, Berkeley*

Russia is making its third historic attempt to leap from backwardness into modernity. Each time the inspiration has come from the West; each time it has been propelled from above with draconian ruthlessness; each time the new would be born only by demolishing the old; each time it tried to avoid the costs of modernity, and each time experienced them all the more traumatically.

Does the same fate await the neoliberal revolution as attended the first two projects? Peter the Great's 18th-century Westernizing fantasy is memorialized in his great city, which was to diffuse modernity into the feudal hinterland. This ran aground on the sands of popular inertia and state-strangled development. Modernization from above generated skirmishes from below, culminating in the revolutions of 1905 and 1917. In the second leap forward, Western ideology, this time in the form of Marxism, spawned an extravagant vision of transformation. At the price of a holocaust visited on society, the Bolsheviks heaved this huge body forward. The project was shipwrecked on the shoals of the industrial classes that were to be its agent. The violent and mobilizing energies of communism finally dissipated, causing the Soviet regime to collapse under its own weight.

Reversing 70 years of isolationism, long-suffering Russia has embraced, albeit ambivalently, the ideology of markets and democracy as a third recipe for launching into a Western orbit. Will this third leap forward

<sup>1</sup> Send correspondence to Michael Burawoy, Department of Sociology, 410 Burrows Hall, University of California, Berkeley, California 94720-1980.

be any more successful than the previous two? Will it too run aground? Will it involve less sacrifice? As Trotsky asked, in his famous treatise of the same name, "What Is the Soviet Union and Where Is It Going?"—so 60 years later we must ask the same of Russia.

In evaluating the Soviet order, Sovietologists were right to distinguish ideology from reality, and we should do the same for the post-Soviet order. That the ideology is our own should make us more rather than less attentive to the dangers of arrogant self-delusion: universalizing the elixir of market and democracy, dismissing alternative visions as so much humbug, confounding theory and practice, mistaking intentions for consequences, reducing means to ends, confusing symbols of modernity with its content. Equally dangerous is to recognize the discrepancy between ideology and reality, only to make light of it as the inevitable accompaniment of transition, an ephemeral lag that will evaporate or the by-product of irrational resistance. To recognize that ideology and reality are at odds and then to proclaim so much the worse for reality—that would be to repeat mistakes of the past. Here I will entertain the possibility that ideology may be at fault.

Accordingly, I first consider the neoliberal vision behind shock therapy, as presented in Anders Åslund's *How Russia Became a Market Economy*, and then contrast this with experiences from industrial shop floors, as presented by Clarke and his collaborators in *Management and Industry in Russia*. In the third part, I locate this "plant sociology" in its broader context of political transition and economic involution, drawing on Siegelbaum and Walkowitz's *Workers of the Donbass Speak*. Finally, I ask what role Western social science might play in the unfolding Russian drama.

## NEOLIBERAL ILLUSIONS

Anders Åslund is one of neoliberalism's leading exponents and executors. A Swedish diplomat in Moscow from 1984 to 1987, he later became identified with "shock therapy" and "big bang" when he joined Jeffrey Sachs and David Lipton to advise the first post-Soviet Russian government. *How Russia Became a Market Economy* describes the first three years of shock therapy, its inspiration, its trajectory, and its consequences. Although never doubting the correctness of the original vision, Åslund is not uncritical of the way it was implemented. His conclusion, however, is clear: "Russia could (and did) reform, and it has become a market economy" (p. 316) and therefore—what is not even stated because it is so much taken for granted—the conditions for genuine economic development have been established. Once the market is in place, the radiant future is assured. Let us consider the argument in more detail.

Neoliberal theory begins with the defects of the old order: the primacy of politics, an allocationally ineffective economy awash with price distortions. Markets, on the other hand, balance supply and demand through rational prices and at the same time put politics in their place. To make

the transition to a market economy under conditions of a collapsed party state, a recalcitrant elite of state enterprise managers, and a devastating economic crisis, it is necessary to deal swift and comprehensive blows to the old order. First, the new government must dissolve the legislature and call for new elections while enthusiasm for liberal reform is at its height. Second, there must be immediate liberalization of prices followed by stringent stabilization to prevent inflation and budget deficits. Third, in order to destroy the old managerial elite and set up the conditions for a market economy, there must be comprehensive and rapid privatization. This policy package constitutes the three legs of the inaugural big bang that will give the reforms sufficient forward momentum to overcome the inevitable resistance as fortunes are lost, living standards fall, and production is restructured.

How does Russia measure up to the program? On the first leg, Åslund condemns President Boris Yeltsin for having no political program. He blames Yeltsin for not dissolving parliament immediately after the failed coup in August 1991, as though the deputies were already predisposed to obstruction because of their "communist" past. However, Åslund overlooks the fact that these deputies had emerged in the anticommunist elections of 1990 and that their confidence in Yeltsin was so great that, after the abortive coup, they gave him powers to rule by decree. It was not so much the "communism" of the representatives as it was Yeltsin's own Bolshevik proclivities, as well as the policies he pursued, that led to confrontation. Indeed, Yeltsin made repeated and unconstitutional attempts to dissolve parliament, pouring salt into open wounds. The denouement came in October 1993 when Yeltsin ordered the military assault on parliament—the same parliament he had valiantly defended against communist putschists only two years earlier. The elections that followed in December 1993 did not bring the expected enthusiasts of neoliberal reform to the legislature but instead enhanced the power of the communists and Vladimir Zhirinovskiy's "Liberal Democrats." There was no way around it: shock therapy was bound to court resistance from a popularly elected assembly.

On the second leg, liberalization of prices and trade began very successfully on January 2, 1992, but by April, opposition from congress, through its instrument the Central Bank, upset stabilization. Credits were disbursed to agriculture and industry, and the government was caught up in a whirlwind of spiraling inflation and budget deficits. Without resolving these problems, it moved on to the third leg: privatization. Celebrating the first anniversary of the failed August coup of 1991, Yeltsin announced the program to privatize through vouchers, what he called "the ticket to the free economy." All Russian citizens would receive 10,000 ruble vouchers to be invested in newly corporatized state enterprises of their choice. This was to be a people's capitalism. Of the three legs, Åslund considers privatization to be its sturdiest. "The transfer of half of the economy from the state to private hands in just two years was an extraordinary achievement" (p. 266). One is reminded of an earlier prop-

erty transfer in the 1930s, the collectivization of agriculture, which was celebrated with the same programmatic triumphalism. To be sure, privatization was not executed with violence, but that does not make fanatical implementation of theory any more economically rational.

By Yegor Gaidar's own admission, fanaticism was necessary to make irreversible the transition to a market economy. Once a market was established, power could be handed back to apparatchiki, such as the new prime minister, Viktor Chernomyrdin, who would have no option but to continue where the radicals had left off. Åslund comments, "Both radical liberalism and communist insurrection had come to an end. Russia entered a stage of normal politics—that is, one in which interests take precedence over ideas" (p. 2). One might say this was a rather rash prediction, but the following three years, until Yeltsin raided government coffers for his 1996 reelection campaign, did see interest rates increase, the ruble exchange rate stabilize, inflation brought under control, and the budget deficit more or less contained—the neoliberal dream realized. But the economy, subject to increasing domestic prices, converging on and even going beyond world prices, has shown few signs of recovery. On the contrary, output has fallen, wages are months in arrears and lag behind prices while unemployment has increased. Outside the sphere of finance and trade, capital investment has slowed to a virtual standstill. That Russia has been able to sustain these blows, so far without enormous destitution even as inequality has soared, reflects the richness of its inherited resources and the resourcefulness of its population accustomed to devious modes of survival.

The three-legged stool definitely wobbled and, at times, even looked like toppling over. The neoliberal program was held up by strategic errors, by mounting political opposition, and by Western renegeing on promised financial support for stabilization, but the stool is in place. If it looks precarious, so the argument goes, that is because the ground underneath has not been leveled. If it supports a disintegrating economy, this is because the old has yet to disappear and the new has yet to be born. If services, trade, and finance expand at the expense of industrial production, these are signs of "normalization," that is, convergence with the modern Western capitalist economy. If inequality has increased, this too is a healthy convergence on the West. The decline in living standards has been exaggerated because so much private sector activity goes unrecorded and because Soviet figures were inflated. Mortality rates have increased, but this is the result of bad diets and lack of exercise inherited from the past, combined with the inevitable stresses and strains of transition, and so on. As with Bolshevism, necessity becomes virtue, defeat appears as success.

From one point of view, of course, shock therapy *has* been a colossal success—from the standpoint of international capital. With trade barriers down, Russia has been deluged with cheap foreign goods. I have seen meat from China, salmon from Norway, vodka from Poland, potatoes from Hungary, bananas from Ecuador, kolbass from Denmark, cheese

from Holland, confectionery from everywhere. Even those who cannot afford such largesse aspire to it, while making do with local bread and milk products, supplemented by home or dacha cultivation. And who now buys Russian furniture, refrigerators, washing machines, televisions, cigarettes, and cars, especially if they cost as much as foreign ones? Home industry has been decimated except in perishable goods and a few protected enclaves. Profits from the natural resource industry, particularly fuel, pour into foreign bank accounts. Capital flight has been estimated at \$10 billion a year. In short, modern patterns of consumption and distribution have taken root—if not in reality then in the imagination—while modern modes of production have been repressed. Here we have the classic formula for Third World underdevelopment.

Åslund recognizes that Russia has not fared as well as the countries of East and Central Europe, but this is because its neoliberal program was more needed and yet suffered bigger setbacks. "Russia's weak state and strong rent-seeking elite called for a more robust and radical approach than that taken in east central Europe, where both the state and civil society were comparatively stronger" (p. 11). The Czech Republic is Åslund's exemplary case of democratization, privatization, and macroeconomic policy. Poland, on the other hand, bungled its privatization program by taking a gradualist approach (on bad advice from European advisors), although it had been more successful in its macroeconomic strategy (where it had followed the ideas of Jeffrey Sachs). As long as the universe of experiments is the former Soviet Union and the countries of Eastern Europe, failures of the neoliberal program can be attributed to mistakes of execution.

China, however, challenges the entire project. Its economic reforms supervised by the party state have given China growth rates only equaled by Russia's rates of decline. China is celebrated by critics of big bang as the case for gradualism. In a telling three pages, Åslund pleads Chinese exceptionalism: there was no economic crisis, the managerial bureaucracy had been softened by the Cultural Revolution, and the state possessed a strong central leadership. There are no lessons to be learnt from China since the historical circumstances were entirely different. Mikhail Gorbachev had tried the Chinese road and brought down the entire order, leaving the Soviet Union in "a state of utter and complete collapse" (p. 52). It is difficult to defend this position. First, it was the "political shock therapy" of 1988—a phrase coined by Yegor Ligachev, Gorbachev's archenemy in the Politburo—introducing direct elections to a new Soviet parliament that unraveled the Soviet system. Second, the economic reforms of perestroika were directed at industry, very different from the Chinese stimulation of the profit motive in agricultural cooperatives, township and village enterprises, and local state corporatism. The Chinese road of state-guided marketization from below was simply never tried in Russia.

*Empirical anomalies* can be used either to push a theory forward into deeper waters or to rechart the boundaries of its relevance. We can either

reconstruct neoliberal theory to accommodate the Chinese experience or simply limit the applicability of shock therapy to industrialized societies (as opposed to agrarian), extricating themselves from socialism. *Logical contradictions*, on the other hand, internal to the structure of a theory, are more difficult to handle. They too come home to roost when theory is put into practice. Thus, at the heart of Lenin's theory of transition to socialism was a state that destroyed the old order while creating the new, a state that would magically wither away without developing interests in its own perpetuation. By starting and ending with a weak state, Åslund avoids this trap but falls into another. It is not the state but the market that is endowed with magical properties, destroying the old order while creating the new.

Behind the contradictory functions of the market lies a mutating managerial elite. On the one hand, Åslund regards Soviet directors as reactionary defenders of socialism, an "unsocial class" that has to be destroyed. On the other hand, because there is no other agent of capitalist revolution, Åslund ends up calling on this selfsame class to forsake its interests and become agents of the neoliberal revolution. "It is vital to break inflationary expectations and to turn state enterprise managers from the parasitical seeking of rent or subsidies from the state to profit-seeking endeavors. They must be convinced to shift their attention from supply to demand. They will only do so if they both face a demand barrier because of strict budgetary and monetary policy and if they believe it will last" (p. 9). Just as the Bolsheviks demanded that the working class sacrifice itself for the greater goals of socialism, so the neoliberals demand that the directorate transform itself for the greater glory of capitalism. Neither ideology wants to examine real interests.

When Åslund briefly attends to the managerial class in his conclusion, he notes with satisfaction that the military industrial complex has been largely crushed but the energy sector is still crawling with rent seekers. Profit maximizers are hard to find in his account, yet the transition depends on them. In fact the distinction between profit maximizers and rent seekers obscures the more fundamental one between those who seek profit in the sphere of exchange and those who seek profit from production, between a "merchant class" and an "industrial bourgeoisie." Russia's success in creating an unlimited market, the commodification of everything, has turned everyone into a trader but impeded the formation of a bourgeoisie that seeks profit by adding value. Indeed, Russia has never had an autonomous, self-reproducing, indigenous, propertied bourgeoisie, and it is not something created overnight. Here, once more, is the Achilles' heel of neoliberalism—its failure to comprehend class.

Institutional or "evolutionary" economics pays much more attention to the social and political conditions of entrepreneurship and criticizes neoliberal theory for its obsessional focus on *allocational efficiency*, through the balancing of supply and demand, even though in this regard markets are not markedly superior to planned economies. Evolutionary economics argues that the salient property of capitalist markets is their

*dynamic efficiency*, that is, their innovative pressures. But, so the argument continues, such "creative destruction" requires a stable institutional environment—contract enforcement, rule of law, stable rates of taxation, and so on—to promote risk taking and long-term investment. Evolutionary economists consider shock therapy and its wanton destruction of the old order as inimical to nurturing a modern bourgeoisie, that is, markets are necessary but not sufficient for economic development—a point that is standard fare in all strands of economic sociology.

Beyond evolutionary economics are bleaker hypotheses in which markets are neither sufficient nor necessary but under certain circumstances obstruct economic development, indeed become the armature of underdevelopment. Shock therapy becomes a Western formula for making Russia an entrepot economy. Loans from the International Monetary Fund (IMF) and World Bank are "free cheese in a mousetrap"—a phrase coined by Alexander Rutskoi, Yeltsin's chosen vice president, later to become his implacable rival. In this third view, explored below, Russia's regression is not transitional but permanent.

#### SHOP FLOOR REALITIES

To adjudicate among these competing hypotheses, we must turn from the lofty realm of ideology and central policy-making to the effects of those policies on the everyday operation of the economy. We must pay attention to the fault line that divides ideology from experience by turning directly to that experience.

Here we are fortunate to have at our disposal a unique series of case studies conducted by Russian sociologists working under the guidance of Simon Clarke of Warwick University. Against the drift of Soviet sociology that relied on social psychology and surveys, Clarke brought together teams of sociologists from all over Russia to study the post-Soviet enterprise through participant and nonparticipant observation. *Management and Industry in Russia* is the first of four volumes. Reading these studies, conducted in the period covered by Åslund's treatise, one is struck by the abyss separating neoliberal ideology from the everyday world of the enterprise.

Simon Clarke's introduction draws on the well-worn framework of "informal" versus "formal" relations to explain the persistence of Soviet forms of enterprise. While the *formal* structures of party, planning, and, to a much lesser extent, trade union may disintegrate, the *informal* relations of the shop persist to reproduce the old order. Thus, in the first two chapters, Sergei Alashev insists that Russian workers, now and before, contrary to stereotype, love to work. He shows their ingenuity in breaking bottlenecks, in coping with crises that beset production, in developing chains of collaboration, and in creating their own norms of cooperation and obligation.

These chapters are among the best overviews of life on the Soviet/post-



Soviet shop floor. Yet they must leave the Western reader wondering whether there is anything specific to these informal relations, for Alashev, unwittingly perhaps, reproduces the classic shop floor accounts of American industry. We can find here Alvin Gouldner's indulgency patterns, Donald Roy's goldbricking and quota restriction, patronage in recruitment and promotion. Like the "plant sociology" of the 1950s, these Russian studies of the 1990s tend to isolate the shop floor and downplay the very external forces that produce its distinctiveness.

Unable to pinpoint the peculiarities of Russian shop floor custom and practice, Clarke turns from the informal to the formal: "In the Soviet enterprise the formal system was far more rigid, far more remote from everyday reality, and far more impervious to change than is the formal system of the Western enterprise" (p. 21). But this overlooks what is really of interest, namely the way informal relations are a response to and a compensation for the unintended consequences of rationalities imposed from above. Seen in this light, shop floor relations of the socialist factory *are* quite different from those of the capitalist firm since they respond to different contingencies. The capitalist workplace is typically driven by *demand side* pressures stemming from process and product transformation, impelled by competition among firms to secure profit. The immediate problem for the capitalist firm is to sell what it produces at a profit. When it fails, it must either innovate or shut down. For workers this means expropriation of control and redundancy.

The socialist workplace, on the other hand, is driven by *supply side* constraints, arising from the continual bargaining for resources under "soft budget" constraints. The socialist economy is a shortage economy in which materials are of poor quality, machinery continually breaks down, labor supplies are inadequate, and so on. Shop floor autonomy is not only required for efficient production it also arises from the power of workers, who cannot be easily fired. The socialist workplace challenges workers to be more than appendices of machines. Against their formal job description, they reunify conception and execution. Willy-nilly they become skilled workers by virtue of the uncertainty of production. We can find this in capitalism but not as its central tendency, where deskilling still prevails.

As described by Lena Lapshova and Irina Tartakovskaya, this has fascinating consequences for gender relations on the shop floor. It turns out that despite their discipline, their attention to detail, their sobriety and regular attendance, managers still did not prefer women over men. Managers regard women's commitment to work as instrumental. They do not love their work like men. They may be more industrious, but they do not exhibit the same intrinsic attachment, the same initiative as men—so crucial to the effective functioning of the Soviet factory. Even worse women are more easily incensed by injustice while men are more prepared to compromise; women are more principled and men more inclined to bargain. While Lapshova and Tartakovskaya recognize the interests of men in excluding and dominating women, they nevertheless give over-

riding weight to immutable psychological and cultural factors that predispose women to subordinate positions (p. 166).

However, for the most part, the book is refreshingly free of the Russian penchant for cultural essentialism. There are no sweeping claims about *Homo Sovieticus*, or the eternal Russian hankering for a strong leader, or the Russian worker's obstinate egalitarianism. Instead, the studies pay close attention to concrete relations of production. In this regard the two discussions (chaps. 4 and 5) of "paternalism" are useful in characterizing the overall political order of the Soviet factory. Petr Byuzikov regards paternalism as growing out of the coercive factory regime of Stalinism. Managerial guardianship, he argues, can easily slide back into authoritarianism as well as move toward more instrumental market models of governance. The Samara Research Team (chap. 5) goes further by filling the concept with specific attributes: the existence within the enterprise of a wide range of nonindustrial activities, charismatic leadership, strong hierarchical management, closed information, egalitarian principles of wage and distribution, nonmonetary relations, ideology of paternalism. One wonders how distinctive this is to the *Soviet* factory, which of these factors are essential, which are contingent, and what is the relation among them. Are there not different forms of paternalism? Engagement with other studies of the socialist enterprise, such as Andrew Walder's *Communist Neotraditionalism*, would have given their analysis more bite.

Closer attention to variation between and within factory regimes might also have forced them beyond the shop floor, away from a static, state-as-guardian perspective and toward a hierarchical bargaining model of the Soviet shortage economy. Enterprise directors spend their time not only queuing, hoarding, and searching for materials in short supply but bargaining with higher authorities for loose plans, that is for targets easily achieved and indices easily manipulated. In these pursuits they have a common interest with their employees, laying the basis for factory paternalism, cemented by the factory's social welfare regime, which provided kindergartens, vacations, housing, and other scarce goods. The result of both improvisation on the shop floor and unity vis-à-vis planning authorities leads to strong collectivism both on the shop floor and at the level of the enterprise—hence the importance of the labor collective, a signal feature of the Soviet factory that Clarke and his collaborators stress.

But how then might one explain continuity of paternalism and labor collective in the face of an ascendant market economy, profit maximization, and the hollowing out of enterprise welfare? Here the studies are unclear. Cultural inertia would be the easy answer—consonant with Åslund's view of enterprises as bastions of irrational conservatism, impeding the enlightened policies from the center. An alternative explanation would focus on the external relations among enterprises and between enterprises and the state. To be sure the economy is privatized but still the state holds the key to enterprise survival by distributing credits at low interest rates. Budget constraints are not hardened but monetized. Directors manipulate the labor collective, instigating strikes in order to sub-

stantiate their appeals for financial aid. When credits are not forthcoming from the state, then enterprises simply do not pay their taxes and utility bills, or enter into chains of debt with one another, or avoid the monetary circuit altogether by reentering barter transactions. Enterprises appropriate control over assets while externalizing liabilities, especially to the state. In this view the informal—whether paternalism or the strength of the labor collective—is not some cultural leftover that lives on under its own inertia but is reproduced by the hierarchical bargaining that continues even under a market economy.

So much for continuities, but surely there has been change too? According to Pavel Romanov (chap. 7), based on his observations of a number of workplaces, power is being delegated to line management, which now has the power to hire and fire. Valentina Vedeneeva draws the opposite conclusion from her own case study, where the introduction of time wages, designed to retain skilled workers, leads foremen to lose control over workers. To understand these contradictory tendencies, one must go beyond plant sociology to the economic and political relations in which each factory is embedded. When the context is stable, one can talk of custom and legacy, but in the transition, external uncertainties disrupt custom and strategic action prevails. We must, therefore, turn to the study of social processes, the succession of events, and explore the strategies of different actors who manipulate rather than unreflectively execute norms.

In this regard the most interesting chapter in the book is the one written by Marina Kiblitiskaya, entitled, "We Didn't Make the Plan." Kiblitiskaya describes the situation at a plant that repairs and maintains electric trains. Early on in the reform process, the enterprise was leased from the state, giving management much more autonomy. Yet things remained much as before—the enterprise was still under pressure to maintain a planned output. In May 1993, however, it failed to meet the plan, and the puzzle Kiblitiskaya wrestles with in the paper is why. She recounts all the problems in making the plan—shortage of workers, shortage of equipment, shortage of supplies, an ineffectual manager of supplies. But there was nothing new there. She investigates further and shows that there was an informal deal that a proportion of the carriages were returned untouched each month. A system of mutual bribing made this possible. But in May the customers refused to accept the terms of the informal agreement and turned back the unrepaired carriages. Added to this, workers decided to restrict output themselves because the bonus system did not encourage plan fulfillment. The final reason was the absence of the deputy director, who normally ensured plan completion. When management met to allocate blame for the failure to meet the plan, it was the chief inspector who was publicly humiliated for holding up production for a week. Rather than focus on the real relations, management operated in its customary ways, seeking to apportion blame rather than recognizing a structural problem. Nothing changed as a result.

Still, the question that Kiblitiskaya does not pose is whether there was

anything uniquely post-Soviet about the event. There are intimations that things are changing. First, one wonders why suddenly the customers insist on real rather than fictional repair. Could it be that they are subject to hard budget constraints and can no longer afford to allow carriages to deteriorate? Second, why do workers suddenly decide to restrict output when the payment system had not changed? Could it be that the monetary incentives are now more important than before, that cash is more important as social benefits have become scarcer? Workers get less and so they give less. And why was the deputy manager not there, guaranteeing the plan? Was it because he had a business trip of his own?

In emphasizing legacies, Clarke and his collaborators provide an important corrective to neoliberal optimism. In starting from experience rather than ideology, their research is pathbreaking. By looking into the hidden abode of production, these studies show that, while there may be much "reactive adaptation," there is not much "proactive innovation." We should not be looking to manufacturing for dynamic growth. However, by leaving external forces unexamined, their plant sociology inclines Clarke and his colleagues toward cultural or inertial explanations of continuity and backwardness. Confined to the shop floor, their analyses miss the very real changes that are taking place in the spheres of distribution and consumption—changes that impinge on enterprises in different ways and at different rates. Plant sociology is a good point of departure but by itself cannot capture the dynamism of a transition in which "everything is pregnant with its opposite": modern banking and finance flourish alongside Soviet production; profits are maximized but without investment; wages are not paid but people still work; enterprises are bankrupt without being liquidated; and generally market liberalization sends Russia scurrying toward the Third World. We need to take the perspective of the totality in order to grasp the overall character of the transition.

#### POLITICAL TRANSITION AND ECONOMIC INVOLUTION

In *Workers of the Donbass Speak*, Siegelbaum and Walkowitz address the question of transition directly. Their book is subtitled *Survival and Identity in the New Ukraine, 1989–1992*. Both authors are social historians, the first of the Soviet working class and the second of the American working class. They led a team of labor studies experts and Soviet specialists, who arrived in the Donetsk as part of a joint Donetsk-Pittsburgh oral history video project. They arrived in July 1989 on the heels of the momentous all-union strike of miners, which had begun in the Kuzbass and spread to the Donbass and Vorkuta. As a result of the exceptional circumstances, Siegelbaum and Walkowitz were given complete freedom to conduct interviews not only with old timers but leaders of the strike too. They returned with their film, soon after the second major strike in 1991, and stayed to conduct further interviews. They arrived once more the following summer. The book brings together a selection of the inter-

views and two major commentaries, one from Siegelbaum and Crowley and the other from Walkowitz, which analyze the changes in those tumultuous three years through the voices and eyes of the community.

The interviews make clear just how traumatic the change has been for the mining community in this city of 1.2 million in southeastern Ukraine. Walkowitz's essay presents the miners as they see themselves—the dynamite that brought down the old regime. They fought for greater control over the product of their labor and over their enterprises by demanding a market economy; they fought for democracy and greater control over their lives by demanding Ukrainian independence. Amazingly, within the space of three years, they had won virtually all their demands but found themselves much the worse for the change. They had vanquished the regime that had made them into a labor aristocracy. The collapse of the command economy and its replacement by markets jeopardized the mines and future employment. Ukrainian independence did not bring greater control to Donetsk but shifted the center of political power from Moscow to Kiev. Economic levers remain in Russia, and Ukraine becomes a dependent satellite. As the Ukrainian economy deteriorates ever more rapidly, hopes for a postcolonial renewal turn into “neocolonial” resentment. So far, Ukrainian nationalism has been restrained, but Russians are always potential scapegoats for post-Soviet underdevelopment.

Siegelbaum and Walkowitz are not content to simply display the familiar irony (or at least, familiar to the Russian working class) that those who bring down the old order become the first victims of the new order. Crowley and Siegelbaum root the experiences of miners in the context of their enterprises. They go beyond the shop floor studies of Clarke and his collaborators by exploring differences between mines—both the strategies of the directors and the response of the miners. They compare October Mine, where there develops a strong, almost syndicalist, militant workers movement headed by the newly formed independent trade union, with Kuibyshev, where the director constructs an effective paternalism, co-opting opposition and bolstering the official trade union. Whereas all miners of the Donbass were united in the 1989 strike, in 1991, October goes out on strike but not Kuibyshev. The interviews show that the director of Kuibyshev managed to convince his workers that they had too much to lose, economically, from striking, while October was a poorer mine with a history of militancy and more to gain.

Without disregarding the importance of the distinctive traditions of the mine collective and the legitimacy of the director, the authors argue that the rise of the market, especially price liberalization, had the effect of dividing the interests of workers on the basis of the profitability of their mines. Only when common interests can bind workers together again, as when central price controls were reimposed in 1993 or when miners faced months of wage arrears in 1996, does a unified strike against Kiev become feasible once more.

The comparison between mines underlines how misleading is Åslund's distinction between profit maximizing and rent-seeking enterprises. Each

mine tries to do both—to seek profit in the market and extract concessions from the state. More to the point is *how* enterprises seek profit—whether through investment in new machinery, through new forms of work organization, through reducing labor costs, or through channels of exchange. Take the case of Zasiad'ko, an unusually prosperous mine that had built an entire trading and industrial complex, which included “a department for housing construction, two vacation centers, greenhouses, a livestock station replete with slaughterhouse and smokehouse, and its own farm whose produce was exchanged for consumer goods. Contracts with trade organizations brought in clothes, shoes, furniture, and automobiles, while barter deals provided the miners with cassette players, VCRs, and televisions at subsidized prices” (p. 82). The focus is not on rebuilding the mine but on profit from trade. The mine becomes a center of merchant and not industrial capital, exploiting the large difference between domestic and international prices that still existed in 1991. The strength of the labor collective described by Clarke and his colleagues only further discourages managers from investing in or transforming production.

Shock therapy has indeed created a market but not economic development. The dynamism lies in the sphere of exchange—in the expansion of banks, insurance, services, advertising, trade—precisely the redistributive sphere that broke down with the collapse of communism. Neither revolution nor evolution captures the contradictory degradation of the Russian economy, indeed of Russian society—the unique blend of the modern and the ancient. I prefer to speak of *involution* to capture the process of economic self-destruction and self-consumption, in which expansion of the market undermines rather than promotes production.

The transition is a succession of involutionary phases. In the first phase, the dissolution of the party state stimulates the expansion of trade and barter and the conservation of the Soviet enterprise. In the second phase, liberalization and privatization produce a flourishing financial sector with cheap credits, negative interest rates, uncontrollable inflation, and huge budget deficits. In the third phase, stabilization seeks to bring the economy to heel but intensifies the barter of the bankrupt and imperative coordination through “the mafia.” At each phase escalating transaction costs eat away at an ever-diminishing productive base.

As enterprise disintegrates as a source of livelihood and the state cannot take up the slack, so the mass of the population falls back into a primitive, intensified domestic economy of dachas and subsistence plots and trading and buying their wares in the thriving bazaar. Everyone makes the most of resources inherited from the past, not just cultural and economic capital but also social capital. Kin networks, in particular, assume the redistributive burden of state and enterprise. Involution from above finds its complement in involution from below. Every step forward is accompanied by an equal step back. Modernity swirls around in an archaic world. Even in politics Russia is caught between nostalgia for past egalitarianism and security and illusions of a radiant future of plenitude. Here hope is born from desperation.

## WESTERN SOCIAL SCIENCE IN A POST-SOVIET WORLD

These three books forcefully raise the dilemma of how Western social scientists can and should relate to Russia's incorporation into the world economy. The neoliberals, even more than the Bolsheviks before them, dismiss indigenous thought and celebrate their universal truths with all the confidence of a triumphant imperial power. Searching for an ideology to justify their rule, apparatchiki of old provide fertile terrain for converts and disciples. Neoliberals and their epigones claim that the new order can only be built on the ruins of the old. Only great sacrifices bring the fruits of the modern market. When leaders bend to the skepticism of their voters and offer temporary relief, foreign advisors deny responsibility for the outcome. The IMF and World Bank loudly proclaim: Follow the recipe or be damned.

Seeking neither converts nor disciples, offering neither program nor policy, Simon Clarke nurtures a community of critical Russian sociologists who investigate what is actually happening within the crucible of post-Soviet economy. They work collaboratively across Russia, holding regular seminars, taking sabbaticals in Warwick, and all along maintaining continual contact. They monitor the effects or noneffects of neoliberal policies promulgated from the center. They pursue the gap between promise and reality, ideology and experience. Rather than imposing truth from above, they propose to begin where Russia already is; rather than fanatically destroy, they work from what exists. Development will not appear through immaculate conception, from theoretical proclamations, but through harnessing popular imagination and energy, amply displayed on the shop floor.

Finally, Siegelbaum and Walkowitz offer a third model of involvement devoted to giving voice to the working classes directly in interviews and films. By bringing them to the United States and organizing visits to the mines of West Virginia, they encourage union leaders and managers to make up their own mind about what they want to take from the West. They describe the tragedy that has befallen the miners of the Donbass, their struggles for survival, and the identities they seek to forge, but they offer no solutions.

With the Bolshevik Revolution, Russian leaders saw themselves as the vanguard of world revolution. Lenin, Bukharin, and Trotsky took over the reins of Marxism as brilliant practitioners and original thinkers. They were feared and revered. When the Bolshevik Revolution degenerated, Western intellectuals (until the advent of the Cold War) often turned a blind eye to Stalinist atrocities. We should learn from our history. With the neoliberal revolution, Russian leaders have already become an imitative, parasitical elite, cynically manipulating the new world order for their own aggrandizement, deftly deploying the ideology of markets and democracy to present their narrow interests as the general interest. They decry helplessness before laws of their own self-created market, denying accountability for Russia's plight while stashing away their ill-gotten

gains in Western banks. One is reminded of Fanon's African bourgeoisie—corrupt, profligate, voracious, and unscrupulous—more than Weber's sober, austere bourgeoisie.

Western social scientists should not be complicit in naturalizing history or the economy, as though their force were immutable and incontrovertible. We should not cower behind market euphoria, cultural essentialism, or even *vox populi* but reveal the real interests and forces at work in order to expand the horizon of possibilities and decipher the direction of change. We should not indulge the illusion of the time nor shrink from unpopular prognosis—that Russia is carving out its own road to underdevelopment.



## Book Reviews

*Down from Bureaucracy: The Ambiguity of Privatization and Empowerment.* By Joel F. Handler. Princeton, N.J.: Princeton University Press, 1996. Pp. ix+269. \$29.95.

Michael B. Katz  
*University of Pennsylvania*

Since the late 1970s, governments in the United States have shed responsibility for operating many services. All levels of government more often use private contractors to accomplish public purposes; the federal government has delegated power to the states; and state and local governments have devolved authority to local councils and institutions. Joel Handler equates decentralization, deregulation, and privatization as instances of this localizing impulse. In *Down from Bureaucracy*, he asks, "What are the consequences of these moves for citizen empowerment?" (p. 5).

Handler has a clear thesis: "Decentralization—the deliberate allocation of authority to lower bureaucratic units, whether public or private—is a major technique for managing conflict," and it "often works to the advantage of most of the important stakeholders." This, he believes, is why decentralization in its many guises has become "ubiquitous throughout public programs" (p. 10). He illustrates his thesis with discussions of theory and wide-ranging case studies: the history of welfare, Reagan-era deregulation, the response of Arizona's government to expanding social needs in the 1980s, the local control of educational financing and exclusionary zoning, and privatization. (He concludes that what matters is not the form of ownership—public or private—but the "institutional arrangements," mainly the presence of competition, without which privatization just creates new bureaucracies.) The book's last section asks how empowerment starts and is maintained. Here his case studies focus on worker safety and the frail elderly ("empowerment by invitation") and current-day Chicago school reform ("empowerment by conflict").

I do not find Handler's thesis convincing—for three reasons. First, "managing conflict" remains a cloudy idea. Is it displacing conflict from one arena to another, avoiding the eruption of conflict in the first place, or co-opting disadvantaged stakeholders into structures that do not disrupt existing relations of inequality? Second, a variety of goals, scrupulously described by Handler, drive each of his examples. In only a few cases does managing conflict appear the most important. (Advocates of contracting out, e.g., would describe their goals as cutting labor costs and increasing productivity.) Why, then, privilege one explanation?

Third, in one instance Handler has the story backward. A more accu-

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rate reading contradicts his thesis. Handler argues that higher levels of government historically managed potential conflicts by delegating the controversial care of the "undeserving" poor to lower levels. To the contrary, despite persistent local variation and control, from the late 18th to the late 20th centuries, the administration and financing of relief (the older term) and welfare lurched toward higher levels of government—in the 19th century to the states and, with the New Deal, to the federal government. The recent devolution of authority to states reverses a trend of nearly two centuries' duration.

Handler views one major branch of current-day school reform as a vivid and instructive attempt to advance empowerment through decentralization. He calls this effort to give parents and local communities more power over schools "democratic localism," a term he apparently is unaware I coined. (See, e.g., *Class, Bureaucracy, and Schools: The Illusion of Educational Change in America* [Praeger, 1971].) Handler rightly finds the most dramatic instance of democratic localism in Chicago school reform, which is the most radical attempt to restructure an urban school system in over a century.

Handler's account flattens the experience of Chicago school reform because it lacks firsthand immersion in the schools and the city's exciting reform community and rests on a narrow range of sources. (Starting in 1990, two colleagues and I spent about five years periodically observing Chicago school reform.) A wider view would show the role of the Chicago Teachers Union as more complex and constructive than in Handler's account. Although he draws sensible conclusions from early research, Handler misses the significance of local schools' dependence on what he labels "outside resources," a catch-all term he does not dissect. "Outside resources" do not negate empowerment, as Handler implies. True, they are necessary to redress the balance of power between local schools and central authorities, but they also constitute an exciting new branch of civil society dedicated to educational improvement—a great source of strength in Chicago and an example and inspiration to cities elsewhere. "Outside resources" also includes redefined roles for experts or, as I like to call them, "alternative professionals," who now must earn the trust of the teachers, principals, and parents that purchase their services.

*Down from Bureaucracy* describes a major trend in the history of public organizations. This is its primary contribution. Handler's case studies provide provocative examples and a wealth of useful data. His subtitle catches the book's main message exactly—"the ambiguity of privatization and empowerment." As he shows clearly, the meaning and results of these movements—which are redefining government, the welfare state, and the roles of public and private agencies—remain riddled with ambiguities and contradictions. Handler is at his most useful and convincing when he highlights this complexity and avoids the temptation to wrap it in a conceptual straightjacket.

*The Modern Corporation and American Political Thought: Law, Power, and Ideology.* By Scott R. Bowman. University Park: Pennsylvania State University Press, 1996. Pp. ix+436. \$55.00 (cloth); \$15.95 (paper).

Eric W. Orts

*University of Pennsylvania*

This book addresses an important topic that deserves careful consideration by political theorists, legal academics, and other social scientists. The rise of large business corporations counts as one of the most important organizational transitions of the 20th century. *The Modern Corporation and American Political Thought* aims to make sense of this transition by proposing "a political theory of the corporation" (pp. 15, 237–38, 284). This project is worthy, but the book fails to deliver completely on its ambitious promise. It is well researched but unevenly edited and lacking in thematic coherence. Scholars working in the field will nevertheless find aspects of this work useful, especially its rich collection of sources given in the text and extensive footnotes.

The book first outlines the uncontroversial proposition that large corporations have become very influential in our society. It then attempts to place this social development of large corporations in the context of political theories of liberalism. Again, the topic is an important one, but my guess is that most political theorists will not find that this book advances their understanding of the relationship between business corporations and political liberalism much beyond previous treatments, such as the one by Robert Dahl (*A Preface to Economic Democracy* [University of California Press, 1985]).

Most useful is the author's straightforward and clear discussion of the primary texts of six significant American critics of the business corporation, most of whom may be correctly labeled "progressive." The first three are in fact early-20th-century political progressives: Herbert Croly, Walter Weyl, and Thorstein Veblen. The book provides a very useful service in reminding us that these early critics have much to say about business corporations and their influence in society that remains relevant and thought provoking. Their basic message is that society as a whole and its political system must take the phenomenon of large corporations seriously and regulate some aspects of their structure and behavior for the general welfare. The second trio—Adolf A. Berle, Peter F. Drucker, and John Kenneth Galbraith—appears later in the 20th century. The author's choice of these figures provides a helpful, if somewhat eclectic, focus for the contemporary study of the influence of business corporations in society. Berle in particular, who is the only legal academic among the group, deserves close attention. The author reminds us of Berle's important contributions to the understanding of the social issues of "corporate power" (p. 203).

Unlike Berle, however, the author does not bring the same level of

legal sophistication to the task of understanding the nature of business corporations. He focuses on the difference between "internal" corporate governance and "external" regulation of corporations (e.g., pp. 11-12). But this distinction is not new, and the author does little to add to its critical potential. In addition, his analysis of both internal and external aspects of the corporation omits some major developments. The analysis of internal corporate governance, for example, ignores the significant change in corporate culture occasioned by the rise of large and powerful institutional investors. On external matters, the author spends a great deal of time on antitrust regulation and the first amendment rights of business corporations but omits any mention of the development of federal securities law. These oversights undermine the author's broader claim to lay an interdisciplinary foundation for a new political theory of the business corporation.

The most significant substantive weakness of the book lies in its reliance on the idea of "corporate power" without stopping to analyze clearly what this term means (pp. 1-5, 14-27). The author says repeatedly, and accurately, that the legal form of the corporation is pervasive. But the author's relatively unreconstructed Marxian class analysis does little to advance our understanding of this important social phenomenon.

Still, scholars interested in the important and complex topic of corporate governance will wish to consult this book, as well as other recent treatments of the political dimension of business corporations given by J. E. Parkinson (*Corporate Power and Responsibility: Issues in the Theory of Company Law* [Clarendon Press, 1993]) and Mark Roe (*Strong Managers, Weak Owners: The Political Roots of American Corporate Finance* [Princeton University Press, 1994]). The book points the way toward new directions in interdisciplinary research, but it leaves the task of building an adequate political and social theory of business corporations to future scholars who will find this work a helpful resource from which to draw.

*Constitutive Criminology: Beyond Postmodernism.* By Stuart Henry and Dragan Milovanovic. Thousand Oaks, Calif.: Sage Publications, 1996. Pp. 288. \$79.95 (cloth); \$26.95 (paper).

Mark Colvin  
*George Mason University*

Henry and Milovanovic develop their "constitutive" theory of criminology by critiquing modernist thought and drawing upon some recent post-modernist conceptions. The authors reject the nihilism, subjectivism, despair, and anarchy of knowledge that pervade "skeptical" post-modernism. While they view deconstruction as an important analytic technique and an essential component of social change, they reject post-modernism's obsession with deconstructionism. Instead, they point to-

ward an "affirmative" postmodernism that emphasizes "reconstruction," in which humans actively constitute a less harmful world.

The book is organized around discussions of human nature, social structure, law, definitions of crime, crime causation, and justice policies and practices. For each of these, the authors critique modernist conceptualizations (which range from positivist empiricism to structural Marxism) for their deterministic models and constructions of discrete categories, both of which become reified. To move beyond these constructions, the authors draw on Jacques Lacan's discourse analysis, Anthony Giddens's structuration theory, chaos theory, and phenomenology (as well as other postmodernist insights) to assert the indeterminacy of social forms and human subjects and their interconnectedness through human agency and discourse.

It is not possible in a short review to cover all of the themes, arguments, and insights of this important book. The most provocative discussions are those concerning the definition and causes of crime. Crimes "are nothing less than moments in the expression of power, such that those who are subjected to them are denied . . . their worth, and simultaneously reduced and repressed in one of several ways" (p. 116). The behaviors we usually think of as criminal would be included in this definition, as would "pervasive hidden crimes of domination" involved in "symbolic violence," especially those connected to race, class, and gender in which the disempowered are denied expression of their humanity. Many routine institutionalized activities of industrial society involve the exercise of power over others that denies their humanity. This is why both capitalism and hierarchical state socialism are criminogenic. Crime, then, is "redefined in terms of the power to create harm (pain) in any context" (p. 118).

According to Henry and Milovanovic, "Crime is not so much caused as discursively constructed through human processes of which it is one" (p. 170). These processes comprise relationships that are not deterministic but dialectical, a dialectic that assumes nonlinear development and a movement, through human agency, toward instability of social forms. Crime "is 'caused' through institutionally situated and discursively ordered processes, when people lose sight of the humanity and integrity of those with whom they interrelate and whom their actions and interactions affect. It is the outcome of language and thought processes which objectify others as separate, dehumanized entities" (p. 175). In our current historical period dominant discourses of race, gender, and class permeate the totality of social relations and give rise to objectifications that can be the bases for criminality (as now broadly defined). Whether a particular situation or interrelationship will result in criminality cannot be determined with any precision since the dynamics of human relations are indeterminate, can be altered by seemingly small events, and are part of an historically situated, ongoing process that is also indeterminate. The subject who is invested in a power relationship that does harm to others has the potential (in concert with others through deidentifying with the

constructed discourse of power) to reconstitute himself/herself and the social relation. Such a conception of the changeable nature of human action, which reflects David Matza's (*Delinquency and Drift* [Wiley, 1964]) concept of "drift," opens the possibility for transformation of human subjects and the social structures we construct. Thus the authors discuss "recovering subjects," who include not only "criminals" but all of us who in one way or another are invested in social forms and constructed discourses that do harm. The final chapters focus on various policies and practices that promote recovering subjects and simultaneously transform the political economy into an empowered democracy in which harm is reduced.

This book should become the focus of debate and discussion in the years ahead because it provides the basis upon which criminology can indeed be reformulated in new and imaginative ways. The reader who is not already familiar with postmodernist work and its often obscure language, however, will find it exceedingly difficult to extract the authors' ideas. This is unfortunate because the authors hoped to "introduce the basic ideas of postmodernism" (p. 1) and engage readers in a new discourse. The authors could have done a much better job of guiding the uninitiated through the linguistic labyrinth of postmodernism, thereby making it a more inviting book. Even though this is at times a difficult text, it is nonetheless important reading precisely because it challenges us to examine critically our assumptions about human subjects, the social world, crime, and justice policies.

*Aramis, or the Love of Technology.* By Bruno Latour. Translated by Catherine Porter. Cambridge, Mass.: Harvard University Press, 1996. Pp. x+314. \$45.00 (cloth); \$19.95 (paper).

Thomas F. Gieryn  
*Indiana University*

A French personal rapid transit system named Aramis, which was long deliberated but never built in concrete and steel, is the occasion for Bruno Latour once again to turn sociology on its head. Aramis began in the late 1960s with a seductive idea to combine the different virtues of cars (autonomy, flexibility) and trains (less pollution, no gridlock). Independent modules holding about six people each move along fixed rails, but the itinerary is point-to-point. Passengers program the module to arrive at a chosen destination, and then it goes there with no intermediate station stops or transfers. Development of Aramis lasted for almost two decades and at times showed signs of becoming real: patents were secured; scale models and prototypes were built; research was conducted on the innovative technological bits; the French state, a large corporation, Parisians, and their public transportation agency all got interested in the dream. But Aramis grew by fits and starts. The Center for Technological

Experimentation (CET) was created in 1984 to work out bugs, yet, deep-pocket supporters became increasingly skeptical about its viability. Even when the project was zeroed-out in 1987, however, many were on hand to declare Aramis a success. In the end, Aramis died when—like Frankenstein's monster—no one loved it anymore.

Evidently, Bruno Latour was called in to do a postmortem on Aramis, perhaps for some nervous government agency. If this book is indeed that commissioned autopsy of a technological failure, Latour probably made no friends among the bureaucrats. Not only does the book refuse to assign cause or blame, but even Latour himself seems to disappear in a kind of "distributed authorship." Five voices tell the story of Aramis, each in its own distinctive typeface. The first-person "I" is a young engineering student who takes an internship at the *École des Mines* (where Latour corporately hangs out) with a Professor Norbert H., to sort out Aramis's demise. Interleaved with their conversations, voices of protagonists in the Aramis story are given life in interview transcripts and in the documents and reports they wrote—and even Aramis speaks for itself. An unnamed analyst intervenes throughout—trying neither to bring into being a personal rapid transit system nor to account for the failure of one attempt but just hoping to persuade sociologists to change their ways.

Norbert and his intern seek blame and cause by following the actors back upstream to a time when Aramis was still a dream to believe in. The characters in this love story identify themselves by pointing fingers at others when things go bad: Matra (an industrial development corporation), RATP (which operates the Paris Métro), the French Ministry of Finance, regional government officials, engineers at CET. Perhaps Aramis died because its survival compromised their interests: Matra could lose profits, the Finance Minister had more urgent uses for the millions Aramis would cost, Parisian commuters are afraid of getting mugged. Or perhaps Aramis died because of congenital defects in its technology, an explanation initially preferred by the engineer-cum-sociologist intern. The two sleuths follow Aramis into its guts: maybe "non-material couplings" (the autonomous modules sometimes moved together as a train, linked only electronically by sensors that would maintain a minimum distance between them) were simply infeasible from the start. Or perhaps the culprit is neither these human nor nonhuman players but the context in which they lived (or died): markets and politics. Aramis was buoyed by plans for the 1989 World's Fair in Paris that would showcase French technological ingenuity (and sunk when the fair was abandoned), just as it was buoyed by the arrival of socialist Mitterand as president in 1981 (then sunk in 1986 when he was forced to share power with the Gaullist Chirac, who had been elected Prime Minister).

A list of 21 interpretations of Aramis is eventually assembled, but the authors resist the temptation to reduce the story to either interests or fatal technological flaw or context. None are sufficiently stable throughout the project to bear the burden of cause or blame: profits, microprocessors, and politics are incessantly interdefined and renegotiated. For example,

"mechanisms cope with the contradictions of humans" (p. 206): telescoping bumpers on both ends of the modules are superfluous, except for doubts by some about the ability of on-board sensors and microprocessors to keep the cars from bumping. The materiality of Aramis—along with its feasibility—changed in response to the diverse wants of those humans with stakes in its achievement, and to its own needs. In the same way, actors themselves do the "contextualization" of Aramis rather than leave this job to the sociological analyst. Norbert and intern follow Aramis from Paris to Lille where they are introduced to its older brother "VAL," an up-and-running fully automated (no drivers) rapid transit system also built by Matra. The success of VAL was crucial for RATP's interest in Aramis (Paris simply could not be eclipsed by Lille) and for their confidence in Matra.

Why should sociologists *follow* those people and things they seek to understand? Because "they're doing our sociology for us, and doing it better than we can" (p. 10). The "classical" sociology that Latour seeks to bury assumes that sociologists know more than actors—empowered as they are with a putative master discourse of norms, interests, structural contexts, and common sense powerful enough to explain what actors themselves cannot see and sufficiently above the fray to be scientific, objective, authoritative. Latour would rather efface the distinction between what sociologists classically do and what engineers, politics, and managers did with Aramis: "Technology is sociology by other means" (p. 210). And what does Aramis become in his alternative "relativist" sociology? A hybrid "quasi-object," a cascade of translations, delegations, and displacements. Latour loves to bash ontological categories: Aramis is a hybrid of human and nonhuman, of social and technical. It died when its enthusiasts began to treat it as "pure," as capable of bringing itself into being without the love (interests, politics, power, masses, money) that all machines must have to live. Aramis is a "quasi-object" suspended between text and thing, shifting along a gradient of stabilization (blueprint, prototype, memos, reports, journalists' news, scale model, embarrassment): a "fiction seeking to come true" (pp. 18–19). Aramis is the translation of commuters' impatience, French technological superiority, and engineers' fun and games into wheels, tracks, and computers. It is, as well, the delegation of competence from a human driver to a microprocessor and a displacement of the latter.

Whether one prefers the language of structures and interests or hybrid "quasi-objects," Aramis exists now as an archive of documents no one else will look at, a few technological wonders (e.g., great seats) that could get built some day but not under the name of Aramis, and some old tracks and rusting steel consumed only by vagrants and graffiti artists. The odds of a more stable and lasting utility are better, I think, for Latour's sociology.



*The East in the West.* By Jack Goody. Cambridge: Cambridge University Press, 1996. Pp. x+295. \$54.95 (cloth); \$18.95 (paper).

John A. Hall  
*McGill University*

Jack Goody is one of the most eminent postwar British social scientists, the social anthropologist who gave his subject an historical turn—thereby providing sociologists with key monographs on literacy, on military technology and state development, and on the assault by the Catholic Church on extended kinship links. The present sustained meditation will certainly gain readers on account of its antiorientalism—although it should be stressed immediately that Goody is an old-fashioned empiricist, keen to tell us both about other societies and ourselves, and so decidedly averse to the view that we cannot understand “the other.” Much is to be learnt from Goody’s negative critique of Max Weber, about whom he writes with great insight and knowledge, and of such academic colleagues as Ernest Gellner and Alan MacFarlane—with some of his venom being left for this reviewer. More important, the book contains the fullest account available of Goody’s own view of social development, and it will become a standard work on that account alone.

Attention is initially given to the Weberian notion that occidental development depended upon the possession of some unique sort of rationality. Goody will have none of this: he argues persuasively that logical thought was present in all the great oriental civilizations and further insists that systematic bookkeeping was equally widespread—there being nothing special, in his view, about double-entry bookkeeping *per se*. Equally powerful chapters then demonstrate that there were no psychic barriers to commercial activity in oriental civilizations: Goody makes this point particularly forcefully with reference to Gujarat, but he adds some attention to both China and Japan. A final pair of truly brilliant chapters consider the notion that a more nuclear family, home to “individualism,” might have aided development in the West, with extended kinship networks elsewhere being seen as some sort of obstacle to development. Goody notes, to begin with, that family size did not vary vastly across civilizations, except during moments of demographic explosion, and he insists that economic development has always—in the past and present of the West as much as in that of the East—been aided by extended kinship links.

Goody goes beyond these particular arguments to suggest that occidental and oriental civilizations share a common Mesopotamian social portfolio, whose base in literacy can be observed by the examination of Africa, on which he is an expert, where it was lacking. For most of the historical record, oriental civilizations have led or surpassed the achievements of the West, and Goody believes that they may be set to do so again. Far too much has been made of early modern achievements—even granted the impact of the printing press, about which he writes with great in-

sight—given that they represented recovery from disaster more than any radical transformation. This does not mean that he denies the importance of industrialization, but a lead for the West of less than two centuries scarcely matters from his lofty vantage point, especially given the ease with which this new productive style has been copied. So there simply is no justification, in his view, for any search for “miraculous” “uniquenesses” inherent within European history.

There is—indeed, always was—everything to be said for the view that both rationality and capitalist mentality were widespread. Further, I stand corrected in some of my own speculations, drawn upon his own earlier work, about the impact of the European family pattern. But it is possible to accept these detailed arguments and yet to consider the rise of the West as a crux to world history desperately needing explanation: Max Weber’s problem remains, for all that his own solution to it was both flawed and ethnocentric. For it is a fact that the West—alone among the agrarian civilizations that it resembled in very many ways, as Goody rightly stresses—so increased its power that it changed the terms of conduct of others. I suspect that Goody downplays this obvious point out of contempt for the conflation of development with virtue. But the two can be considered separately. For one thing, Patricia Crone’s *Pre-Industrial Societies* (Blackwell, 1989) properly rewrites social evolution so as to recognize that change comes from social formations unable to secure a settled way of life—which is to say that the West moved forward because it had been a relative *failure*. For another, occidental power was often used utterly viciously. In this whole matter, it should be possible to study a power breakthrough without it being presumed that one endorses it. And I fondly continue to believe that a key element that does explain the rise of the West, and which is utterly overlooked by Goody, is its multipolar state system. Capitalism may have been present in many places, but it only survived and triumphed when it had more than a single political center.

*Calamity and Reform in China: State, Rural Society, and Institutional Change since the Great Leap Famine.* By Dali L. Yang. Stanford, Calif.: Stanford University Press, 1996. Pp. xv+351. \$49.50.

Victor Nee  
*Cornell University*

The memory of famine, experienced in the aftermath of Mao’s Great Leap Forward, explains why peasants and local elites sought far-reaching institutional change at the outset of the post-Mao reforms. This is the essence of Dali Yang’s argument developed in his historical analysis of Chinese Communist agrarian policies. The book is the best in a series of studies of the political economy of state and rural society in contemporary China, superseding earlier studies by virtue of its conceptual clarity and

quality of evidence. Yang's book is notable for definitively moving beyond the state-centered analysis influential in the China study field (e.g., Oi, *State and Peasant in Contemporary China* [University of California Press, 1989]). The explanatory framework he employs instead locates the causal mechanisms of institutional change in the interaction between political actors and ordinary peasants. The result is a compelling multilevel institutional analysis of political competition in a state structure characterized by multiple interest groups and ideological differences within which central elites contend for dominance and local elites both bully and ally with ordinary peasants. Local actors in poor provinces hard hit by the Great Leap famine, according to Yang, pushed effectively for discontinuous institutional change—decollectivization—which in turn paved the way for further liberalization in China.

Previous to Yang's study, the politics of the Great Leap Forward was shrouded in thick ideological mist. To be sure its failure was well known, and the extent of the calamity in its toll on human lives was highlighted by demographers who documented the extraordinarily high mortality and decline in birth rate in the aftermath of the Great Leap famine. But the political impact of the Great Leap famine was not well understood, although it was known that differences arising from the Great Leap foreshadowed the bitter elite conflicts of the Cultural Revolution. The book documents the extent of the Great Leap Forward disaster, contending that Mao's Great Leap policies produced the worst famine in history, exceeding in mortality the most severe famines elsewhere in the developing world. Yang interweaves analysis of recently released internal documents with anecdotal accounts from the Chinese press and statistical analysis of provincial-level data. His foil are arguments that point to the horrors of the Cultural Revolution in explaining the elite decision to set China on a course of economic reform.

Yang carefully penetrates the mist to dissect skillfully the elite response to the Great Leap famine, pinpointing the effect of political pressures imposed by the central state on ambitious local leaders, which in turn exacerbated the severity of the ensuing famine. He documents not only Mao's crisis of confidence but the reactions of central and local elites to the unfolding of the catastrophe. Although China's elite drew important lessons from the failure of the Great Leap, and Mao never again approved of another Great Leap in agriculture, the lasting effect of famine was not to discourage Mao in his single-minded quest for revolutionary immortality, but to discredit state-imposed collective institutions in the minds of ordinary peasants and local leaders especially in poor provinces. Provinces that experienced the greatest loss in lives in the aftermath of the Great Leap Forward and located furthest from Beijing were least likely to support radical agrarian policies in the Cultural Revolution.

Although Yang's argument and evidence are persuasive, nonetheless it is worth quibbling with him over limitations of his causal model. His singular focus on the Great Leap famine as the cause of economic reforms is overstated. Although Yang points to correlations that are consistent

with his "great famine" hypothesis, his models do not control for the effect of the Cultural Revolution nor for that matter rule out alternative arguments pointing to the interaction between declining economic performance and interstate competition (e.g., Nee and Lian, *Theory and Society* 23 [1994]: 253–96). Hence, he is unable to reject competitive theories. Rather than emphasizing only the effect of the Great Leap famine, a more plausible causal account points to the cumulative effects of failed state policies in explaining both the elite's decision to initiate reform and bottom-up support for economic liberalization. Such a cumulative causation theory of economic reform would include Yang's "great famine" hypothesis as the first in a sequence of causes, leading up to and including the Cultural Revolution and "ping pong" diplomacy's unintended consequence of awakening the Chinese elite to the extent that China had fallen behind its modern nemesis, Japan (not to mention Taiwan). In other words, the cumulative effect of a sequence of political failures shattered the legitimacy of Maoist institutions, while the "open door" policy sharply contrasted China's technological stagnation with the rapid economic development of rival states on the Pacific Rim. China's modern humiliations at the hands of more powerful rival states is entirely overlooked in Yang's explanation of the decision to launch far-reaching institutional change. The memory of such national humiliation—not simply the memory of famine—combined with a different kind of hunger, the hunger for reclaiming China's rightful place in the community of nations, figured prominently in the quest for power and plenty through market-oriented reforms.

*Sugar Island Slavery in the Age of Enlightenment: The Political Economy of the Caribbean World.* By Arthur L. Stinchcombe. Princeton, N.J.: Princeton University Press, 1996. Pp. xvii+361. \$45.00.

Dale Tomich  
*Binghamton University*

In *Sugar Island Slavery* Arthur Stinchcombe addresses the complexity of Caribbean society and history from 1750 to 1900 through a comparative political sociology of the sugar plantation and racial slavery. He proposes a historical study of the "sociology of freedom" that is intended to reveal the sociological conditions under which possibilities for individual or group choice and exposure to the liberties of others are either enlarged or restricted. For Stinchcombe, the Caribbean sugar plantation represents an extreme case of repressive labor relations and racial domination. Its presence, together with a high degree of political autonomy for local planter elites, results in slave societies and in racism in daily life and politics. He uses this theoretical proposition to explain variation in politics, economy, and race among islands and historical periods.

In the first half of the book, Stinchcombe delineates distinct and inde-

pendent island social systems and identifies the geographical and historical factors shaping their political economy. He establishes conditions for the development of the sugar plantation and analyzes variations in economic organization, social and demographic structure, planter political power, and race in different island societies. In the second part of the book, Stinchcombe examines the abolition of slavery throughout the Caribbean during the 19th century. He analyzes the transmission of democratic and emancipatory movements from different European metropolises and the ways that the reactions of local groups transformed the social meaning of race, ethnicity, and citizenship in particular islands. Stinchcombe argues that although social and political outcomes differed throughout the Caribbean, nowhere did emancipation bring democracy and citizenship rights, and that the degree to which it was accompanied by civil rights for free people in the labor market, in the courts, in family law, or in freedom of migration varied.

In *Sugar Island Slavery*, Stinchcombe effectively identifies relations between key variables, establishes the range of their variation, and constructs a systematic typology that accounts for social, economic, and political differences among Caribbean island systems. Nonetheless, the very success of his comparative method raises questions about its adequacy for comprehending complex, long-term processes of historical change.

Stinchcombe's approach systematically eliminates from theoretical consideration historical processes and qualitative distinctions among social relations. In order to establish variation, he treats individual islands as independent units of analysis. Each local space is conceived independently of broader regional, imperial, and world processes and of its relation to other such local spaces. Similarly, he conceives of variables—the sugar plantation, slave society, and racism—as discrete and independent entities and regards them as attributes of individual island systems. Such variables are treated as already given, while the processes producing them are viewed as secondary and conjunctural. Stinchcombe establishes their commensurability across units by treating them as quantitative variations of qualitatively identical phenomena, a greater or lesser degree of the same thing. Comparison identifies their distribution and correlation across cases, and difference is understood in terms of distinctive constellations or combinations arranged along a continuum between opposed types.

This strategy constructs apparent similarities between phenomena by viewing each in isolation. It reduces global, imperial, and regional processes of labor organization, market formation, colonization, and race making to the recurrence of specifically local phenomena, and treats historical time simply as the linear repetition of particular local effects. It thus restricts sociological reflection to the correlation of repetitive social phenomena that are taken as given in each instance. Historical relations are reduced to logical relations among abstract cases and qualitative difference to quantitative variation.

Stinchcombe's methodological approach compromises his "sociology of

freedom." In order to avoid the problems created by posing a sharp dichotomy between slavery and freedom, he emphasizes the variability of slave systems. He conceives of slavery as the extensive and intensive deprivation of rights in familial, social, and economic relations and organizes his treatment of the variation among slave systems along a continuum ranging from socially organized freedom to the extreme restriction of rights of a slave society. This formulation defines slavery by the absence of rights and differentiates slave systems by the extent to which they deny such rights. It thus presumes the potential availability of such rights independent of specific social and political relations. By implication, it treats freedom as the normative condition and reduces the distinction between slavery and freedom to a matter of degree. Possible development is restricted to a limited repertoire of variables (and therefore to a limited set of outcomes) located along a predetermined continuum.

Such an approach not only underplays the enormous difference between the "almost free" slave, whose circumstances are at best tenuous and conditional, and the most oppressed freeman, but also eliminates from consideration the specific historical conditions and experiences through which actors conceive of freedom and struggle to achieve it. Ultimately, it fails to come to grips with the complexities of Caribbean history. The postemancipation Caribbean appears simply as the repetition of slavery, the continued domination of a white planter elite over a "slave-like" nonwhite laboring class. Thus, the very success of *Sugar Island Slavery* indicates the limitations of its approach and makes it a point of departure for an alternative historical sociology that focuses not simply on outcomes but on the frameworks and processes that generate those outcomes.

*Honor and Slavery: Lies, Duels, Noses, Masks, Dressing as a Woman, Gifts, Strangers, Humanitarianism, Death, Slave Rebellions, the Pro-Slavery Argument, Baseball, Hunting, and Gambling in the Old South.* By Kenneth S. Greenberg. Princeton, N.J.: Princeton University Press, 1996. Pp. xvi+176. \$24.95.

Bertram Wyatt-Brown  
*University of Florida*

According to the national library database, Sociofile, more than 500 titles refer to honor. Some of them are festschrifts to venerate old Ph.D. mentors and colleagues, whereas most explore the concept as it applies to various circumstances, from the management of corporate business offices to street corner machismo in the inner cities. In the field of history, on the other hand, the term not only seldom appears in titles, but most historians have not the faintest idea what the ethic of honor might mean in their own or past societies. Students of Western Europe have been less remiss than American scholars, with, for instance, Robert Nye (*Masculin-*

ity and Male Codes of Honor in Modern France [Oxford University Press, 1993]), Kevin McLeer (*Dueling: The Cult of Honor in Fin-de-Siècle Germany* [Princeton University Press, 1994]), and James Kelly (*That Damn'd Thing Called Honour: Duelling in Ireland, 1570-1860* [Cork University Press, 1995]), reinterpreting the *code duello* in modern France, Germany, and Ireland. In 1982, Harvard sociologist Orlando Patterson, in *Slavery and Social Death* (Harvard University Press, 1982), brilliantly depicted the functions of honor and shame in worldwide slave-holding societies, both ancient and modern. Over the last 15 years, however, the ethic of honor, as defined in *The International Encyclopedia of the Social Sciences*, has gradually but most unevenly penetrated American historical scholarship. Kenneth Greenberg has been among those foremost to contribute to this welcome trend. His *Masters and Statesmen: The Political Culture of American Slavery* (Johns Hopkins University Press, 1985) joins Edward Ayers's *Vengeance and Justice* (Oxford University Press, 1984) as an excellent source of enlightenment about Southern early- and mid-19th-century manifestations, the only period and section reasonably well covered. This collection of intriguing essays is a worthy addition to the literature, and sociologists will find it most helpful.

As if adopting an approach along lines of Erving Goffman's work, Greenberg's preface suggests that he seeks to recover the "'dead' language" of honor and proposes that the ritualized discourses accompanying the oratory and actions of gentlemen in the Old South rasp noisily against modern sensibilities. In conscious imitation of Clifford Geertz's technique of "deep play," Greenberg makes the unfamiliar clamor accessible by exploring such curious matters as an antebellum newspaper war in Charleston, South Carolina, over the authenticity of a mummified monkey's upper body and head sewn to the body of a fish. For a fee to the gullible, showman P. T. Barnum was then displaying the object as the "Feejee Mermaid." Partisans in the dispute nearly came to blows because, the author observes, the difference between perceptions of personal opinion and one's integrity was very slight in a society where principles of honor prevailed.

In subsequent essays, Greenberg notes the special popularity of the duel in Southern life; the significance of gifts and hospitality whether lavished or denied; the preferences of honorable as opposed to ordinary hunters for certain creatures to snare, shoot, or post (although the ubiquity of cockfighting is all but omitted); the incompatibility of baseball with the temperaments of honorable men; and the style of death most admired under the code's by-laws. He offers telling reflections on these subjects that sharpen the reader's appreciation for how different the world of slavery and honor was from our own. In an impressive chapter that also deals with the dangers (and excitement) of cross-dressing, Greenberg shows how the relationship of power and authority for Southern whites affected their relationship with slaves. Sometimes masters resorted to rites of public humiliation as punishment, and a common signal of masterly control was the forced undressing of slaves under probing

inspection by prospective buyers. Shame and honor, as Greenberg argues, were the polarities between power and authority on the one hand and abject impotency and mockery on the other. Moreover, as he acutely discerns, Southern whites and blacks studied facial expressions, gestures, tone of language, and other external features with the greatest care because everyone, regardless of color or condition, lived in a chronically mistrustful environment.

Fascinating though *Honor and Slavery* is, several criticisms must be registered. First, Greenberg does not explain exactly what honor is when revealing definitions are available. Moreover, by tying slavery too intimately to the code, he misses the international and even national pervasiveness of this means of guiding social behavior, and he cannot satisfactorily account for the many Southern habits of honor that lingered long after the institution's demise. Furthermore, the author offers little comparative context: even Yankees had their notions of honor and shame. While duels were never popular in the free states in contrast to the Old South, other forms of male rivalry and confrontation did take place with higher reputation and social standing being the reward of vindication. Finally, the brilliance of his storytelling sometimes distracts him from deeper meanings. For instance, he says little about how patronage, clientage, and honor functioned together within Southern political culture. These are very minor complaints. Instead, we should acknowledge the vitality and versatility of the author's exemplary handling of a topic too long dwelling in the historical shadows.

*Of Borders and Dreams: A Mexican-American Experience of Urban Education.* By Chris Liska Carger. New York: Teachers College Press, 1996. Pp. xi+161.

*Con Respeto: Bridging the Distances between Culturally Diverse Families and Schools.* By Guadalupe Valdés. New York: Teachers College Press, 1996. Pp. xvi+237.

Norma Williams  
*University of North Texas*

These books provide data about the problems first-generation Mexican children and their families encounter as they confront the school system in the United States. The authors provide us with important information on the aspirations and failures of people in everyday life. These works challenge a number of stereotypes regarding Mexican-American immigrants, and they should assist social scientists in more effectively interpreting the statistical data on this widely debated group.

Carger has written a partial biography of Alejandro Juárez, whose parents came to Chicago in the late 1970s. The author met Alejandro, who at the time was in the fifth grade, when she was a coordinator of a second



language program sponsored by a local college. She follows his struggles with the school system until the author is informed by his mother, via telephone, that Alejandro has become another dropout at the age of 16.

In this biography we learn not only about Alejandro but also about his family, especially his mother. For instance, Carger presents a brief sketch of the mother's account of crossing the border (on several occasions) into the United States. The first time she crossed she was a young bride of four days (she was 16 years of age); she and her husband were seeking to escape the poverty of their home region in Michoacán, Mexico.

Valdés's work is different from that by Carger. Originally, Valdés's research "was designed to follow 10 children as they started school in a community close to the Mexican border . . . over a 3-year period" in the 1980s (p. 6). This community was located within the larger El Paso-Juárez region. However, as often happens in ethnographic research, Valdés changed the focus of her study. She came to investigate "the ways in which children were being prepared by their parents to function within the family, in the outside community, and in the school setting" (p. 7). The author emphasizes the role of multiple factors in contributing to the "failure" of Mexican-origin children in the school system.

In a narrower sense, Valdés has another objective. As a result of her research, she questions the programs advanced by educators that call for involving parents in the school system. As an educator herself, she suggests that efforts to intervene in the family may undermine the fabric of the immigrant family. Immigrant parents know how to parent; however, their type of parenting does not conform to that which school officials assume should exist.

Having outlined the general contents of these books, I will consider some of their larger sociological implications. First, both, in different ways, challenge the widespread stereotype that immigrant parents are uninterested in the success of their children in the school system. In the case of Alejandro's mother, she was devoted to helping her child become a successful student.

What emerges from both books is a gendered pattern within the family that deserves careful attention by future researchers. Mothers are on the front line in seeking to cope with the school system. This does not mean that fathers are uninterested in their children's educational success. Rather, it is the mothers who are most directly involved in interacting with members of the school system. However, they do not know the language, and even more important, they lack knowledge about the bureaucratic rules that teachers and administrators take for granted.

Second, the school system's failure to address cultural diversity in the United States is implicit in both books. The school system adheres to what Valdés calls mainstream standards (i.e., those of the Anglo middle class), and this commitment makes it almost impossible for the school to adapt to the cultural background of Mexican-origin children. The resulting harshness of school officials and teachers comes to light in brief sections of both books. However, neither author devotes sufficient atten-

tion to how standardized testing within the bureaucratic school system reinforces the orientations of administrators and teachers. The process of standardized testing must be placed in the context of the language, cultural, and class backgrounds of these immigrants, for they have limited formal education and are poor.

Third, both of these books deal, in a general way, with the border between Mexico and the United States. But one must pose the question: What does the border mean? During the past few decades a considerable amount of research has been conducted on the border region. Many researchers, when discussing the border, view it in terms of the political economy, not education.

Typically, scholars take a top-down view of the border region. Thus, we find that bankers and privileged scholars in Mexico City, Los Angeles, and Houston have more in common with one another than they do with the less privileged sectors in both societies. For the most privileged, trans-cultural linkages are emerging as a result of common organizational ties. However, these linkages do not hold for most of the population. For most persons the political border is a major divide, especially with respect to the educational system. The educational systems in the United States and Mexico differ considerably from one another. Until researchers who study the border begin to clarify this situation, they will be unable to understand the issues that are raised, either explicitly or implicitly, by these two books.

In my judgment, more detailed qualitative studies that follow up these works are needed. Only then will we come to understand the high drop-out rate among Hispanics. Sociologists concerned with education, immigration, and race and ethnic relations must not continue to neglect this important social issue.

*What Employers Want: Job Prospects for Less-Educated Workers.* By Harry J. Holzer. New York: Russell Sage Foundation, 1996. Pp. ix+182. \$32.50.

Kathryn M. Neckerman  
*Columbia University*

In *What Employers Want*, Harry J. Holzer looks at the demand side of urban labor markets, illuminating a central question in research on inner-city joblessness: To what extent are less-educated workers disadvantaged by limited skills, by geographic distance from employment, and by race or gender discrimination? Holzer, an economist, has a long record of work on employment and racial inequality. His latest study presents new and unusually systematic evidence on the character of the low-skilled labor market.

Holzer's book is the first to be published from the Multi-City Study of Urban Inequality (MCSUI), a research effort coordinated and partly

funded by the Russell Sage Foundation. MCSUI includes employer and household surveys in the Atlanta, Boston, Detroit, and Los Angeles metropolitan areas. Holzer conducted a telephone survey of some 800 employers in each area.

Holzer's most interesting results concern the role of education and skills in the noncollege labor market. Even for unskilled jobs, he found, employers commonly required high school diplomas or gave preemployment tests, and most noncollege employees, including manual workers, interacted with customers or used basic literacy, math, or computer skills daily. In fact, fewer than 10% of noncollege jobs involved none of these tasks. Central city employers had higher skill demands: their skill requirements for any given occupation were higher, and a larger share of their job openings were white collar. It is likely, Holzer concludes, that the supply of low-skilled workers far exceeds the demand for them, particularly in city labor markets.

The skills mismatch Holzer finds is not good news, of course, for the poorly educated workers who are concentrated in central cities. It is also a problem for central city employers, who cannot fill vacancies as quickly as their suburban counterparts do. Given these conditions, Holzer points out, current welfare reform proposals are likely to fail. Requiring welfare recipients to seek work will overwhelm a small and already crowded segment of the labor market, leading to higher unemployment and/or downward pressure on wages for this largely unskilled population.

Employers' task and credentials requirements are important to understanding racial inequality, Holzer argues. Employers were less likely to hire black men for jobs that involved customer interaction, arithmetic, or computer skills. Black women were hurt less by these task requirements; they were disadvantaged when jobs required literacy or arithmetic, but (like white women) they were favored when jobs required computer skills or customer interaction. Task and credentials requirements accounted for most of the wage gap between white and black men and somewhat less of the wage gap between white men and black women. If race discrimination contributes to the economic gap between blacks and whites, it does so mostly through employment, not wages, and affects men more than women. In addition, bias against black workers may be stronger in the suburbs than in the central city. While black workers are hurt by their concentration in central cities, Holzer suggests, helping them move or commute to the suburbs will not solve all their problems.

Holzer's findings highlight the interaction of race and sex in determining the economic status of low-skilled workers. Men and women mostly work in different labor markets—as research on occupational sex segregation shows—and race matters differently for them. For instance, white men have a disproportionate hold on the dwindling supply of lucrative manufacturing jobs, with incumbents often recruiting members of their informal networks to fill vacancies. While black men are disadvantaged in this labor market, black women are unaffected because women so rarely enter this labor market at all. *What Employers Want* sketches the

characteristics of these different labor markets, usefully informing work on race and gender stratification.

Sociologists may be frustrated by some limitations of Holzer's data. The brief telephone interviews provide little insight into the organizational context of low-skilled jobs and are skimpy even on hiring procedures. We learn, for instance, how many employers have affirmative action programs but not of what these programs consist. More broadly, Holzer will not satisfy skeptics who wonder why it takes a high school diploma to push a broom. He stresses the importance of skills for noncollege workers but spends little time on why employers value these skills. Do changes in work organization now require broom pushers to read and write, or do employers value credentials as signals of other desirable attributes? Could employers' apparently neutral requirements be a way of discriminating against inner-city or minority workers? In trying to make the book accessible, or perhaps simply to make it short, Holzer has glossed over questions like these. Nonetheless, *What Employers Want* provides important new evidence about the job prospects facing low-skilled workers, and sociologists interested in poverty, race, or urban labor markets will want to read it.

*The Cultures of Cities.* By Sharon Zukin. Cambridge, Mass.: Blackwell Publishers, 1995. Pp. xii+322. \$49.95 (cloth); \$21.95 (paper).

Rob Shields  
Carleton University

Sharon Zukin's book, *The Cultures of Cities*, is one of the fruits of the "cultural turn" in much sociological theory, which gathered pace through the 1980s and 1990s. This is by now a school that recovers the responsiveness of the Chicago school and the social engagement of later Marxian sociology. This book does this by being neither a theoretical monograph nor a book of case studies. Instead, successive chapters present a coherent set of "tales," illustrated and given depth by both political-economic and ethnographic research. The result is a *cultural economic* study of the use of art by local business boosters and of strategies for manufacturing public space at Disneyland. The role of art festivals and museums in rural towns (North Adams, Mass.) and global cities (New York) alike as simultaneous expressions of economic elites and tools for selective economic growth centered on the service industries is examined along with artists' and immigrants' employment patterns and the changing ethnic relations of the retail streetscapes of New York City.

Zukin's work is as accessible as an undergraduate textbook as the result of her facility in drawing on the experiences of everyday life and memory. Stylistically, it breaks with the tradition of the critical essay. Interpretive tales draw on the legacies of Walter Benjamin, Gerald Suttles, and feminist cultural theorists such as Meaghan Morris. Methodo-

logically, the approach borrows from Benjamin's fascination with the texture and codes of urban landscapes without surrendering to his romanticism. At the geographic scale of specific streets and sites, the cultural unfolding of economic restructuring processes is captured lucidly. Zukin presents a coherent and detailed demonstration that the cultural sphere has become integrated with the economic. This challenges both Marxist and Weberian approaches to culture and to everyday processes of social reproduction. As Zukin notes, learning from public culture and urban spaces is "a humbling experience, for it upsets many of the assumptions and values on which a critical understanding of modern society is based" (p. 52). Beyond stylistics, the book is an expression of cultural economy's (as in the Culture and Economy Symposium, sponsored by the Center for the Study of Cultural Values and Center for Political Economy, University of Lancaster, 1996) break with the theoretical paradigms of political economy in favor of a form of grounded theory and extended case study (Michael Burawoy et al., *Ethnography Unbound: Power and Resistance in the Modern Metropolis* [University of California Press, 1991]).

The strength of *The Cultures of Cities* is to make clear the logic of theoretical positions that are often poorly illustrated. It so often reveals the aptness of contemporary urban sociological theory better than one could oneself. The essential difficulty that this book overcomes is that of finding suitable examples of, for example, the interrelatedness of the expansion of new security measures, business improvement districts, and high cultural exhibitions and institutions in a nonreductionist manner. With its illustrated examples and maps, it is an invaluable teaching aid.

Zukin surveys the reshaping of public spaces by surveillance and security procedures, by the preservation of historical public sites such as Times Square or Central Park, and by the use of strategies by local businesses to privatize control over public spaces through business development associations. These extend to forms of local justice, private policing, "workfare," and management of activities through control of the visual environment. A central argument of the book is that manufacturing visual coherence according to certain cultural rules and codes by managing the visual environment is one of the primary "cultural" strategies of urban elites. In these spaces, history and nostalgia, plus national symbols and myths, are harnessed to replace the bricolage of urban public spaces with versions of advertising and cultural industry images of idealized public spaces. Rather than being the result of everyday social life, these spaces are fabricated as ready-made urban environments to be consumed as common goods by the public. Not only do visions of public space derive from the commercial sphere but they are conceived as "consumption spaces." These spaces draw on the tradition of Disneyland's "Main Street USA" and the world's fairs' futuramas together with the experience of the shopping mall. These consumption spaces are quite different from the multipurpose politico-economic-cultural spaces of the classical European

city. Social control is now "fun" and is offered as something not just to be "bought-into" but to be purchased outright.

The production of urban streets is more than an architectural or stylistic issue. It includes the construction of lives lived "on the streets," whether as shoppers, retailers, or as homeless prostitutes. Zukin excavates the processes that create the "sameness-in-difference or the difference in sameness," of ethnic consumption spaces. She examines the qualities of public spaces born of insularity and born of exclusion as much as the public spaces that offer the illusion of equal access to all.

In her study, Zukin does not explore the cultural codes that guide, for example, the construction of visual coherence: What is it that makes a set of forms, colors or an arrangement of objects (or buildings) "visually coherent"? But, she opens up such issues by revealing the interests and classes that sponsor such sociotechnical projects and demonstrating the one-sidedness of their impacts and advantages. *The Cultures of Cities* is a significant book that begs for further elaboration and debate, a book in the best tradition of American sociology.

*New Ethnicities and Urban Culture: Racisms and Multiculture in Young Lives.* By Les Back. New York: St. Martin's Press, 1996. Pp. xii+288. \$49.95.

Li Zong  
*University of Saskatchewan*

This book examines how the formation of new ethnicities, culture, and racism is manifested among urban young people in Riverview Estate and Southgate Estate—two neighborhoods in the northern half of a South London borough. Between 1985 and 1989, the author collected ethnographic data from 203 young people and interviewed 56 adults in the two communities. Using empirical data collected using participant observation and interviews, the author analyzes the cultural dynamics of new ethnicities and the formation of racism in a social and historical context as articulated in young people's everyday lives.

The author compares different social and cultural characteristics and ethnic composition between Riverview and Southgate and uses the concepts of "community discourse" and "semantic system" to analyze the different social identities among young people and the formation of racism in the two areas. According to the author, Riverview is a more heterogeneous place where a relatively affluent white working class is dominant in the community. In this community, the predominant semantic system is what he refers to as "white flight." This system is composed of three discourses, and their organizing themes range from the assertion that the area has lost its sense of community, to the idea that the neighborhood is being swamped by minorities and that as a result established residents are being forced to move away (p. 239). In this context, local racism is

expressed. People believe that the decline in the housing and economic circumstances of these residents is caused by the presence of variously defined "problem families," black people and Vietnamese refugees (p. 239). The author finds that despite the prevalence of racist explanations in the community, young people do not passively reproduce the ideologies of their parents (p. 98). There is another partially developed discourse—a racially inclusive localism or "neighborhood nationalism" in the community developed by young people. This discourse regards the community as a harmoniously multiethnic district where belonging is determined not by color or race, but by commitment to the area (p. 239). According to the author, young people create "a syncretic working-class youth culture" that is neither black nor white but somehow a celebration of shared experiences. "This constitutes a volatile working-class ethnicity that draws on a rich mixture of South London, African American and Caribbean cultural symbols" (p. 98). However, as the author argues, this has not led to the complete rejection of racism. Incidents of racist name-calling continue to occur, where racism is used as a strategic resource by white young people against black peers. In addition, young people of Vietnamese origin in this area and black people from other areas are still discriminated against by white young people in Riverview (p. 179).

Southgate, on the other hand, is an intensely multiracial area where black cultural institutions have been well developed. According to the author, this neighborhood is mainly characterized by the "our area" semantic system, which is composed of "harmony discourse" and "black community discourse." The author suggests that "harmony discourse" rejects the legitimacy of racism, while "black community discourse" regards the area as a site of black organization. In Southgate, different from Riverview, the inclusive notions of local belonging enable a greater degree of cultural syncretism to develop amongst young people (p. 179). White young people have developed "a notion of identity free of racism," while black young people have explored "the interconnected nature of the situation within African diasporas elsewhere and a critical reworking of the relationship between Englishness and blackness" (p. 179). Although the "our area" semantic system has played an important and predominate role in formulating young people's social identity and in rejecting racism in Southgate, a "white flight" semantic system is also presented in some accounts, which leads to a situation where racism is not completely banished. Based on the above findings, the author concludes that despite their differences, "it is possible for egalitarian processes to exist alongside the most brutal forms of racism" (p. 249) in both communities.

Overall, this is a good piece of scholarly work that provides insightful ideas about new ethnicity and the formation of racism in the context of young peoples' everyday lives. It is well written and an excellent use of interview data in supporting the author's arguments. However, the model and arguments developed in the book are tentative and limited, since they are based on a small nonprobability sample (a core of 105 informants; p. 253) and since field materials collected through participant ob-

servation and unstructured interviews are qualitative in nature and therefore difficult to quantify systematically. In most cases, community discourses and semantic systems and the young people's cultural values are framed and interpreted by the author, and they are not directly measured by concrete indicators and systematically derived from respondents' self-reported information. The author should recognize and acknowledge these methodological weaknesses and theoretical limitations. Obviously, survey research is needed in order to test and refine the author's arguments.

The author approaches new ethnicities from a cultural perspective, but the concepts of "culture," "urban culture," "culture of young people," "black culture," and "syncretic working-class youth culture" are not clearly defined in the book. Culture as a dynamic and complex phenomenon includes not only linguistic expression and music culture but also a set of values, beliefs, norms, and ideology. It seems that the author does not explore far enough into cultural factors that contribute to new identity and new racism among young people.

Since World War II, many countries, including England, have witnessed the enforcement of antidiscrimination legislation, affirmative action programs, and constitutional guarantees of human rights and freedoms. These changes help to promote a democratic and tolerant society, and the value of equality is widely propagated, especially among young people. However, economic, political, and social inequalities along with racial and ethnic lines remain, and they become the structural bases of prejudicial attitudes and discriminatory actions. The contradiction between democratic principles and racial inequalities at the structural level is reflected in the conflict between the egalitarian values of justice and racial discrimination. In addition to the community discourses and semantic systems described in the book, it is possible that white young people in both Riverview and Southgate have generally developed a new ideology in which two conflicting sets of value are made congruent to each other. Commitments to democratic principles such as justice, equality, and fairness contradict with ideas and practices that discriminate against black people and Vietnamese refugees but such contradictions are accepted as unproblematic in the lives of the young people. Living in a society that believes in democracy and equality, young people recognize that racial discrimination is socially unacceptable. In order to maintain their racist beliefs while championing democratic values, young people have developed a set of justificatory arguments and mechanisms that permit these contradictory ideologies to coexist.



*The Heartland Chronicles*. By Douglas E. Foley. Philadelphia: University of Pennsylvania Press, 1995. Pp. viii+228. \$15.75.

Sylvia Rodríguez  
*University of New Mexico*

*The Heartland Chronicles* is an ethnographic study of Mesquaki Indian-white relations in Tama, Iowa. Anthropologist Douglas Foley returns to study race relations in his hometown of Tama, after 30 years away, upon learning at his twenty-fifth high school reunion that the local Mesquaki tribe has started a high-stakes gambling operation. The recent development piques his interest and leads him into fieldwork informed by his reading of Mesquaki ethnology and history on the one hand, and by memory and familiarity on the other. He brings to this endeavor an approach honed by research on Chicano-Anglo relations in South Texas, about which he has published two books, *From Peones to Politicos* (University of Texas Press, 2d ed., 1988) and *Learning Capitalist Culture* (University of Pennsylvania Press, 1990). Here, however, Foley embarks on a more experimental path not only with respect to his community of study, but also in terms of the kind of ethnography he has written. Ethnography in one's own hometown makes reflexivity less escapable than research in someone else's hometown. Foley's account of Tama is thus more personal than his treatments of Tejano "North Town," and his portraits of individuals are more vivid. The result is an engaging, readable fusion of two stories about how Indians and whites get along in a small Iowa town and how one white, male anthropologist takes a careful look at where he came from.

The book is organized into seven chapters plus an epilogue. It begins with an account of Foley's anthropological homecoming and the contrast between boyhood memory and professional observation of Mesquaki-white relations. His subjective encounter with a dual native/anthropological or insider/outsider perspective is intensified by reading through the archives of the well-known Fox Project in "action anthropology," conducted in Tama between 1948 and 1960 under the supervision of Sol Tax at the University of Chicago. On the one hand the Fox Project records help to inform Foley's professional understanding of the Fox-Mesquaki settlement. On the other, they jar with his own recollections and insider perspective on the people and events Tax and his students described. The first chapter is followed by a history of Tama during the decades he was gone, including changes signaled by the arrival of the American Indian Movement (AIM). Chapter 3 looks at how a change in race relations has been played out in the local schools and school sports activities. Next, Foley examines Indian and white journalistic, literary, popular, and anthropological portraits of Mesquaki culture. In chapter 5 he focuses on ceremonies and individuals located within "the cultural borderland," or "political space in which ethnic groups actively fuse and blend their culture with the mainstream culture" (p. 119). Chapter 6 details recent tribal

politics, and chapter 7 describes how the Mesquaki finally came to build a large casino in Tama. The epilogue quotes local responses to Foley's manuscript about Tama, circulated among informants much as he did in his book about Texas. He concludes with a disclosure that further rounds out this ethnography as a narrative of self-discovery.

Although *The Heartland Chronicles* partakes of the postmodern experimental moment in critical ethnography in several respects, it remains resolutely retrograde in others. Foley stops short of incorporating some of the most important contemporary insights within anthropology, especially those that have come from feminist and postcolonial perspectives, into his reflexive account of race relations and cultural hybridity in Tama. Even though his cultural description incorporates a plurality of voices and viewpoints and thereby works to challenge outdated notions of a pure and bounded ethnocultural essence, his ethnographic gaze nevertheless remains firmly fixed upon the exotic Indian other. By focusing exclusively on Mesquaki ceremonial life, for example, Foley's account inevitably reinforces the notion that only Indians have culture, while whites remain the unmarked, unexamined, implicitly essential given against which all things ethnic take on their interest and meaning. Another feature of Foley's approach, which similarly characterized his accounts of North Town, is its androcentrism. Although he does interview a few women and notes, for example, that the number of female Indian athletes has increased in the schools while male Indian participation has declined, he fails to examine what this might mean or seriously to integrate gender into his analysis of how race, class, and culture interact.

Such drawbacks are hardly unique to Foley's work, however. Indeed, anthropology's endemic addiction to radical alterity may prove even more difficult to overcome than its traditional androcentrism. While these shortcomings will leave some readers unfulfilled, they do not cancel out the book's interest, value, or sheer readability. Foley tells us at the outset that he wants this book to be accessible and instructive to whites and to Indians about one another, as well as to students in college classes. The book stands a good chance of success on both of these counts. *The Heartland Chronicles* also represents a noteworthy contribution to the growing body of postmodern ethnography and to the anthropology of race relations in rural North America.

*American Indian Ethnic Renewal: Red Power and the Resurgence of Identity and Culture.* By Joane Nagel. New York: Oxford University Press, 1996. Pp. xvi+298. \$45.00.

Gary D. Sandefur  
*University of Wisconsin—Madison*

The past 20 years or so have witnessed an increasing amount of attention to American Indians, a group of people who, according to the 1990 cen-

sus, constitute less than 1% of the total population of the United States. This attention has included popular and financially successful movies on American Indian themes, a growing body of critically acclaimed fiction on and by American Indians, and coverage by the national media of the rise and spread of gaming on American Indian reservations. A related phenomenon has been the increase in the American Indian population as enumerated by the U.S. Bureau of the Census from 792,730 in 1970 to 1,364,033 in 1980 and 1,878,285 in 1990. The 70% growth rate of the 1970s and the over 35% growth rate of the 1980s are much higher than can be accounted for by natural increase in the American Indian population or by immigration of indigenous people from Central and South America and Canada. Demographers who have studied these changes have all reached the conclusion that the increase is largely due to changes in self-identification (i.e., people who previously did not identify themselves as American Indians changed their identification to American Indian).

The purpose of *American Indian Ethnic Renewal* is to explain why the increase in self-identification from non-Indian to Indian took place or to document and account for the great resurgence in American Indian ethnicity. The thesis of the book is that the most important factor that accounts for this resurgence of ethnicity is American Indian political activism, especially the Red Power movement of the 1970s. According to the author's analysis, the Red Power movement captured the attention and hearts of Indians and non-Indians alike, leading to a surge in changes in self-identification, an Indian cultural renaissance, and major changes in federal Indian policy. The author pulls together a variety of evidence including census data, primary and secondary sources on the history of American Indian activism, and personal interviews with American Indian activists to construct strong arguments on the connections between American Indian activism, cultural revitalization, and the resurgence in American Indian ethnic identity.

This is a wonderful book. It will be of interest and value to all who are interested in racial and ethnic relations in the United States, as well as to all who are interested in American Indians. The value of the book lies in the care with which it has been written and to its attention to critical theoretical and empirical issues. The first part of the book does an excellent job of discussing recent social science work on the concepts of ethnicity, identity, and culture. It provides a good summary of how those on the cutting edge of social science research on ethnic identity approach these issues. The second part of the book effectively pulls together demographic evidence to document changes in self-identification and explores the rise of American Indian activism. The final part of the book traces the connections among political activism, ethnic renewal, cultural vitality, and changes in federal Indian policy. The book concludes with a useful discussion of the problems confronting American Indian groups today as they deal with the implications of changes in self-identification.

Although I love this book and its analyses, I do not think that the author is completely successful in demonstrating the major thesis of the book—that it is Indian activism that is primarily responsible for the resurgence in Indian ethnicity. Much of the evidence for the role of activism is based on interviews with American Indian activists, for whom activism did enhance their identity as Indians. Most people who changed their self-identification in the past several years, however, were not activists. It is not clear that Indian activism had the same effect on nonactivists as it did on activists. Further, the theoretical work discussed in the first part of the book suggests that the reasons for identifying as an Indian could vary widely across individuals and situations. These reasons could include interest in American Indian music or dance, involvement in a tribal community, interest in national political activism, and/or the financial incentives that sometimes exist to enroll officially in a tribe or identify as an Indian for the purposes of affirmative action. To sort out the role of these alternative reasons, one would have to do interviews with people other than activists to gather information on the processes of ethnic renewal at the individual level. I hope that these persisting questions and the initial answers proposed in this well-done and provocative book will encourage further research on American Indian ethnic identity and ethnic identity and renewal among other groups.

*Against Capital Punishment: The Anti-Death Penalty Movement in America, 1972–1994.* By Herbert H. Haines. New York: Oxford University Press, 1996. Pp. ix+253. \$35.00.

Ed Walsh  
*Pennsylvania State University*

It is hard to think of a less promising nationally organized protest in the United States than the anti-death penalty movement. Not only are its beneficiary constituents destitute and unable to organize themselves from cells on death row, but even its conscience constituents are a tiny coalition of predominantly middle-class whites who, according to a former leading activist, “do not feel comfortable working with poor and minority people on the latter’s own turf and in their own terms” (p. 115). Its primary goal is especially difficult to frame persuasively for a public increasingly frustrated with crime. The major problem Herbert Haines identifies as thwarting its success, however, is the fragility of the bonds among this professional movement’s coalition members preventing them from agreeing upon an alternative to the death penalty.

Despite a temporary moratorium after the 1972 *Furman* decision, the United States had returned to state-sanctioned killing within a decade. Although most western democracies have renounced it, public support for the death penalty in this country is overwhelming if also quite shallow—Haines characterizes it as “a mile wide and an inch deep” (p. 164).

Drawing upon in-depth interviews with anti-death penalty activists and data from a variety of sources, he provides a detailed and thoughtful analysis of the movement's most recent phase.

The central argument of the book is that frame alignment difficulties among anti-death penalty activists—especially the dogged effort to bring about a change of outlook among potential supporters rather than modifying the movement's own diagnoses and prescriptions—are the primary reasons for its political weakness. Although focused primarily on the period since the 1970s, Haines begins with an analysis of the rise of lawyers to movement leadership positions during the 1960s and their temporary success when the Supreme Court declared in the *Furman* decision that the death penalty was a violation of the Constitution of the United States. The unprecedented political reaction this strategy brought from Nixon's White House, state governors, police chiefs, and popular opinion led in 1976 to the *Gregg v. Georgia* decision effectively reversing *Furman*.

New sentencing laws made defendants the beneficiaries of an enhanced version of due process, greatly increasing the costs of capital punishment for state authorities. Activists found that the movement's robust national organizations such as the American Civil Liberties Union (ACLU) and Amnesty International USA (AIUSA) were not able to prevail in battles fought largely at the state level where such movements are typically weak. Its internal factionalism prevented the movement from accentuating alternatives such as the significant economic benefits to taxpayers of substituting life without parole for prisoner executions—estimated to range from \$1.8 million in New York State to \$15 million in California per execution.

Although Haines does not suggest this, the political frailty of the movement is a potential theoretical strength for social movement analysts—providing as it does rich data on an uneasy professional coalition of three social movement organizations (ACLU, AIUSA, and the National Coalition to Abolish the Death Penalty) with a meager conscience constituency focused upon an even smaller beneficiary constituency (184 lives were taken in the United States by the death penalty between 1972 and 1992). And instead of his qualified nod to a political process model among existing social movement alternatives, I found myself wishing Haines would use his fascinating data to emphasize a more holistic perspective. The Kansas anti-death penalty movement, for example, helped create its own political opportunities during 1986 when it successfully framed the issue in terms of the costliness of executions.

This carefully researched and thorough account of anti-death penalty activism in the United States over the past 25 years will be valuable in a variety of traditional graduate and upper-level undergraduate courses including criminal justice and criminology, social movements, and political sociology. For more interdisciplinary courses, it might be mined for useful insights on interaction processes among professional movements, the political economy, the criminal justice system, and public opinion.

*The Militarisation of the U.S.-Mexico Border, 1978-1992: Low-Intensity Conflict Doctrine Comes Home.* By Timothy J. Dunn. Austin: University of Texas Press, 1996. Pp. xii+307. \$35.00 (cloth); \$14.95 (paper).

Manuel Moreno-Evans

*Urban Research Division, Los Angeles County*

Dunn has written an important monograph analyzing the collaborative practices of agencies responsible for enforcing U.S. immigration laws along the U.S.-Mexico border. This is an investigation of immigration and drug enforcement activities of the Immigration and Naturalization Service (INS) in the border region from 1978 through 1992. In my view, the import of the author's contribution is to redirect contemporary research on immigration from studying the characteristics of immigrants to investigating state practices of immigration control planners within an increasingly globalized economy.

Dunn's central thesis is that the introduction of broad policy instruments adapted by federal government agencies to deal with undocumented immigration and illegal drug trafficking in the U.S.-Mexico border region resulted in the gradual militarization of the border from 1978 through 1992. The author argues that INS immigration enforcement practices on the border are conducted not for the purpose of preventing unauthorized migration but to subordinate, discipline, regulate, and shape the flow of Mexican immigrant labor. In support of this argument, Dunn introduces the Pentagon's low intensity conflict (LIC) doctrine as a framework to show how the growing military presence in the U.S.-Mexico border has resulted in a low-intensity warfare against immigrants.

The focus is on demonstrating how various aspects of INS enforcement activities and plans during the Reagan and Bush administrations were consistent with LIC doctrine. He cites the implementation of contingency plans for "immigration emergencies and the roundup of alien terrorists and undesirables" (p. 59) as clear instances of the uses of LIC doctrine as a paradigm for border enforcement.

The author contends that high-ranking officials in the Reagan administration were preoccupied with undocumented immigration as a phenomenon closely connected to the social and political unrest in Central America. After Reagan's reelection, immigration control planners justified the introduction of new technology for border control to facilitate INS's drug enforcement activities. Dunn argues that the increased militaristic approach in immigration during the Reagan years was an expansive strategy of immigration control begun during the Carter years, demonstrating the bipartisan nature of immigration enforcement. The state's ideological emphasis on border security and drug enforcement facilitated the enormous growth of the INS and resulted in the expansion of the mission of the border patrol to include antidrug operations. Readers will find that Dunn does a very good job documenting how, during the Bush

administration, the enforcement of the nation's immigration laws became increasingly a military activity.

By the author's own admission, this monograph is not a neutral piece of research. Dunn is particularly concerned with the disturbing potential for the violation of human rights in the border region resulting from the introduction of low-intensity warfare against immigrants. The monograph carefully documents ample evidence of civil and human rights violations committed by INS agents and concludes that the Mexican population of the border region bears the brunt of a protracted militarized policy of border control.

While the author concludes that the use of militarization as a framework in the study of immigration and antidrug enforcement in the U.S.-Mexico border region "is an open empirical question" (p. 147), there is no attempt to generate empirical hypotheses, although the monograph lends itself to the formulation of empirically testable hypotheses. Using the case study method of research, the author does an excellent job drawing information from a variety of sources, such as congressional documents and press reports. I would have preferred a more extended convenience sampling frame, rather than just relying on interviews conducted with 15 South Texas border-area INS agents and 12 human rights advocates, and a more generous use of tables. The monograph contains only five tables, most of which are placed in the appendixes. Similarly, his research treatment of the U.S. response to immigration in the border region would have benefited from showing demographic changes in the region in the last decades.

In an attempt to explicate the militarization of the border, Dunn is guided by a comprehensive body of theoretical discourse on capitalist societies, including the work of Herbert Marcuse, Alan Wolfe, David Harvey, and Saskia Sassen. Unfortunately, the exposition of the material in the monograph is not sufficiently woven into the theoretical framework introduced in an appendix. Thus the monograph at times becomes a long descriptive exposition of the border enforcement practices of the INS and the way it coincides with U.S. military doctrine.

Dunn offers four explanations for the militarization of the U.S.-Mexico border. First, the taming of the border through militarization has enabled the continuation of unequal power relations between the United States and Mexico. Second, the militarization of the border helped shape the flow of immigration by driving undocumented immigrants into a permanent underground condition. Unfortunately, these interpretations are not sufficiently discussed by the author and remain inconclusive.

The third explanation views militarization as the result of U.S. foreign policy in Central America. While undoubtedly U.S. intervention in Central America resulted in a diaspora of refugees to "El Norte," his treatment of the subject is not sufficiently elucidated. Finally, Dunn believes that militarization of the border was carried out for establishing control over a strategic area (the industrialization of the U.S.-Mexico border) that was "potentially vulnerable to various forms of instability" (p. 160). How-

ever, it seems clear that the militarization of the border can only hinder economic development and economic integration in the region, as stepped-up enforcement of border crossings is likely to harm merchants and commerce on both sides of the border.

In light of the immigration enforcement issues posed in this monograph, the key question becomes how can U.S. policymakers reconcile a continued policy of paramilitary immigration enforcement given the economic integration of the region as a result of the North American Free Trade Agreement (NAFTA). This book brings to the forefront the conflict between militarization of the border and globalization and between contending outdated bureaucratic state structures in the world-economic system. In the wake of the anti-immigrant political climate, the Clinton administration, consistent with the immigration enforcement style of past administrations, has opted for stepped-up border enforcement and the appointment of a military retiree to the post of border czar rather than the pursuit of alternative policy measures. I consider this monograph an invaluable tool for anyone interested in understanding the complexity and implications of contemporary immigration enforcement practices in the United States.

*Resisting Reagan: The U.S. Central America Peace Movement.* By Christian Smith. Chicago: University of Chicago Press, 1996. Pp. xx+464.

James M. Jasper  
*New York University*

In 1981 when he was asked to help a Salvadoran hitchhiker who had been seized by the Immigration and Naturalization Service (INS), Jim Corbett was raising goats near Tucson, Arizona. Corbett filed the proper form so that the hitchhiker could not be deported without a hearing. When Corbett tried to see the Salvadoran, he found that the INS managed to hustle him off to another detention center just before Corbett arrived each time. The INS lied outright to him, tried to intimidate him, and illegally deported the Salvadoran—which Corbett discovered only through the chance intervention of another detainee who had seen his acquaintance. He was stunned and outraged—and ready to fight back. He and his wife borrowed \$4,500 against the trailer where they were living and wrote to 500 fellow Quakers and Quaker meetings around the country. They raised several hundred thousand dollars in two months, but in response the INS simply changed its policies, raising bail for each illegal immigrant from \$250 to \$3,000 and incarcerating Salvadoran applicants for asylum (whose requests, at any rate, were inevitably denied, since the Reagan administration would not admit that torture and death squads were linked to the regime it was supporting). When Corbett began finding local churches that would shelter illegal immigrants in violation of federal law, the Sanctuary movement was born.



This is only one of the moving stories Christian Smith tells in his marvelous new book about the movement against American intervention in Central America, *Resisting Reagan*. This long and thorough work compensates for the scholarly neglect of this movement, especially in comparison to the many works on the peace movement of the early 1980s that partly fed into it. The peace movement is a fairly bland story in comparison to groups who visited Nicaraguan villages destroyed the night before by Contras, an underground railroad for thousands of illegal immigrants, and a determined counter effort that went far beyond infiltration and break-ins to kidnapping and rape. Smith smoothly presents a series of stories: the deterioration of conditions and the rise of protest in Central America; the Reagan administration's mad obsession with the region; the initial involvement of individuals such as Corbett in the United States; the development of formal groups (he concentrates on Sanctuary, Witnesses for Peace, and the Pledge of Resistance) and a vibrant social movement; the battle over public images and opinion; and the intense repression, apparently orchestrated by the CIA, which makes Nixon's burglars look like boy scouts.

Smith has a deft light touch in applying social-movement concepts to his story, but several themes recur. Foremost is the unique ability of religious institutions to consider moral arguments and positions, provide organizational networks if they decide to act, and operate relatively free from pressures of government and business. Another is that people hold a variety of moral principles and sometimes act on them, in a logic quite different from that of weighing costs and benefits. A third is the constant reworking of cultural images, including activists' own self-images, which political activity both depends upon and shapes. Smith also nicely applies two staples of social-movement theory: competition among the different packages through which the Central American controversy was framed and the contribution of existing and new social networks to the growth of the movement.

A study this rich is bound to suggest lines of inquiry beyond those of the author. For example, in dozens of places Smith mentions the emotions that protestors and policymakers felt, but he does not explicitly recognize or use these in his theorizing. He could have linked them to moral sensibilities, used them to elaborate on motivations, or criticized the cognitive emphasis of existing approaches. With his well-wrought stories of individual activists, Smith might have done more to point out the organizational bias of most scholars of protest, who tend to dismiss individuals as exogenous or unimportant. Other readers are sure to find similar materials for their own theorizing.

The book's only disappointment is a brief concluding chapter on the movement's implications for social-movement theories. Some of the lessons—about the importance of religion and morality—are straightforward enough, although superfluous after the rest of the book. His first lesson, that political opportunity structures are crucial variables explaining the emergence and effects of protest movements, is actually at

odds with some of the evidence in the book. Most notably, Smith documents the increased, massive repression that Central American governments inflicted on their citizens in the 1980s but does not explain why protest nonetheless increased. At a time when almost every aspect of protest has been labeled a "political opportunity structure," Smith goes furthest along this tautological trail by labeling the very grievance that sparked the movement as an "opportunity." Thus Reagan's policies on Central America are a political opportunity structure helping to explain the rise of the movement, and Bush's abandonment of many of those policies are a political opportunity structure that helps explain the movement's decline. But theorizing like this hardly mars a terrific case study.

*Evangelism and Apostasy: The Evolution and Impact of Evangelicals in Modern Mexico.* By Kurt Bowen. Montreal: McGill-Queen's University Press, 1996. Pp. xiv+270. \$39.95.

Elizabeth E. Brusco  
*Pacific Lutheran University*

Studies of Latin American evangelicalism have tended to be polarized and polemicized, and Bowen's recent book on conversion in Mexico joins the ranks of a new spate of more balanced correctives such as David Stoll's *Is Latin America Turning Protestant?* (University of California Press, 1990) and David Martin's *Tongues of Fire* (Basil Blackwell, 1990). Bowen's main objective is to assess the growth potential of Mexican evangelicalism. He discusses not only the trajectory of conversion but also rates and causes of apostasy, or the numbers abandoning the faith. Drawing attention to this dual aspect of the movement is among his major contributions in this work.

The book is based on data collected between 1987 and 1993 through survey questionnaires, interviews, and participant observation in four regions of Mexico. Bowen assesses the utility of several well-entrenched explanations of evangelical growth: the deprivation and disorganization models, the primacy of foreign influence, and the return to traditionalism. On the whole, Bowen's data serve to corroborate some of the accepted wisdom about Latin American evangelicalism, although there are notable areas where his data do not fit into existing models.

Bowen provides some useful political and historical background of Mexican evangelicalism. He describes how evangelicals actually benefited from the anticlericalism of the Mexican state, which served to mediate against Catholic antagonism and persecution (p. 213). Constitutional restrictions aimed at the Catholic Church left open space into which evangelicals could move: for example, between 1926 and 1935, the number of Catholic priests in Mexico fell from 4,492 to 111 (p. 36). He also discusses how liberation theology has made little headway among the

poor in Mexico. Evangelicalism has been more successful, and even in the diocese of San Cristobal de las Casas, where Bishop Samuel Ruiz has gained prominence as the leader of Mexico's small liberationist faction, one finds some of the highest evangelical concentrations in the country (p. 220). Bowen's accounts of significant episodes in Mexican evangelical history (such as the San Juan Chamula expulsions, or the history of the Summer Institute of Linguistics involvement), often based on his own interviews with evangelical leaders, are among the most interesting and useful parts of the book.

Bowen takes pains to demonstrate that Mexican evangelicalism is a homegrown movement, and his documentation of the limits of missionary involvement in its growth is thorough and convincing. Mexican evangelicals have been accused of betraying their patrimony for "bread and dollars," a charge for which Bowen finds no support. Missionaries played a largely advisory role, and the largest evangelical group, the Pentecostals, showed the smallest amount of external influence.

The weakest part of the book is Bowen's struggle with the perennial question, "Why do people convert?" In chapter 4, he analyzes data collected from 745 evangelical converts, looking at such issues as occupation, migration history, previous level of religious commitment, types of crises leading to conversion, gender, and marital status. Bowen's treatment of gender in the conversion process is especially problematic. Like practically every other researcher on the subject, he found that "the most distinctive mark of the converts was the preponderance of women in their ranks" (p. 101). Given such a statistic, it would seem that Mexican women's experiences need to be explored to explain their conversion. Yet Bowen's discussion gets caught up in showing that women are excluded from many formal leadership positions in the churches, and does not go much further. Women's influence in the Pentecostal churches, which often arises from their prominence in activities such as speaking in tongues, trancing, and healing, is not mentioned. "Male power in the Evangelical world was absolute" (p. 124), he finds. Yet, he cannot help but conclude in the next chapter, that "no other institution among the popular classes, apart from the spiritist halls provided women with so many opportunities for independence, leadership, and personal expression outside the home" (p. 137). Elsewhere in the book, his discussion of the status of women fails to engage some critical issues, such as those relating to reproduction. Desires to limit family size may play a role in the alienation of couples from the Catholic Church, especially in times of economic hardship such as the period after the 1982 crisis.

Bowen's explanation of male apostasy also falls short. He relates the high male dropout rate to the difficulties men have in giving up worldly pleasures. This conclusion overlooks that evangelicalism may provide a welcome refuge for some men from the dangers and stresses of machismo. In the end, Bowen provides little new insight into why people convert.

It is also disappointing that, despite the book's laudable double emphasis on conversion and apostasy, Bowen did not interview dropouts. His

conclusions regarding why people leave the faith therefore remain speculative and relatively powerless, such as assuming that the strength of their "sectarian zeal" gave Mexican evangelicals "the option of either conformity or complete apostasy" (p. 225).

Despite these shortcomings, the book is important for specialists in the area of Latin American evangelicalism and religious movements. Bowen concludes that "the prospects of Latin America's turning Protestant are slim and remote" (p. 230) but provides ample evidence of the importance of evangelicalism in Mexico and the elusiveness of adequate explanations for its persistence.

*Visionaries: The Spanish Republic and the Reign of Christ.* By William A. Christian, Jr. Berkeley and Los Angeles: University of California Press, 1996. Pp. xxii+544.

Sandra L. Zimdars-Swartz  
*University of Kansas*

William A. Christian, Jr., is well known for his study of local religion in Spain, taking the position that religious phenomena are best understood in the context of geographically specific social and cultural forces. His most recent achievement is *Visionaries: The Spanish Republic and the Reign of Christ*, an impressive examination of the visions of the Virgin Mary at Ezkioga in the Basque country of northern Spain, which began in June 1931 shortly after Spain became a republic and after religious houses in several cities were burned by anticlerical mobs. Two young siblings were the first to report a vision, but soon other seers appeared, crowds gathered, and more than a million pilgrims came to Ezkioga during the next year.

Christian shows clearly how these visions were shaped by social, economic, and political conflicts that enmeshed the Basque people from the years of the Second Republic up to and through the Spanish Civil War, but his concerns really transcend this historical context. He is interested in showing how society structures perception, including the perception of persons claiming religious visions. In the first part of *Visionaries*, Christian reconstructs in detail the events as they unfolded. Here, alongside material unearthed in libraries and archives, Christian places information gleaned from his interviews with persons who participated in the events and their descendants. Christian maintains a critical stance toward the claims made for the religious experiences but always with empathy and respect for his subjects. He lays out carefully the interaction between the visionaries, their supporters, opponents, and interest groups such as clergy, religious orders, doctors, politicians, and reporters. As with other recent studies of apparitions, Christian sees the meaning of these religious phenomena negotiated by and emerging from the interaction of such groups.

In the second part of *Visionaries*, Christian examines the patterns that have emerged in the human dynamics of these mass religious experiences, and here he shows most clearly how social forces shaped such experiences. Over a period of months, through their interaction with clergy, reporters, and doctors, the seers began to manifest the characteristics of a trance state that were appreciated and rewarded by these persons. Although he is careful to note that there was no single pattern of behavior followed by every person claiming visionary experience, he observes that, with few exceptions, the seers manifested signs that seemed to place them in a state beyond ordinary experience, which gave them a new kind of power. By rewarding certain types of behavior and dismissing or decrying others, those around the seers shaped the very nature of the seers' religious experiences. Although believers tend to present visionaries as simple and unlettered, Christian believes that they are intelligent and "quick studies" in response to the demands put upon them and the resources offered them by their devotees.

Christian notes that the theme of the apocalypse, which is often a part of the messages of Marian apparitions, emerged late in the understanding of the meaning of the Ezkioga visions. Examining the vision messages in chronological order, Christian concludes that the messages evolved in part in response to a changing audience. There were no messages concerning universal chastisement, for example, among the first messages reported at Ezkioga, and the meaning of the visions had a local referent, but, as more people came from farther away, the messages became more universal and less fixed in time. Devotees gave the seers books and pamphlets containing information about other prophecies and apparitions, and the seers began to report messages that converged with this information. As opposition to the visions grew among the media and church officials and as the visions began to wane in popularity, the messages became more dire, and their focus shifted from a proclamation of chastisements coming to those who opposed the visions to a proclamation of a great chastisement expected at the end of the world.

If Christian neglects any aspect of this phenomenon, it is the psychological. He acknowledges that personal history may help shape religious experience for an individual, and, through brief biographies, he suggests why certain persons would be an apt audience for the visions, but he does little by way of exploring the role the personal histories of the seers themselves or their particular family circumstances might have played in shaping their visions.

Even before the Catholic Church begins to investigate claims of visionary experience, the public, Christian argues, has already gone a long way toward sifting through competing claims, rewarding some and dismissing others, and toward shaping the very form and meaning of that experience. He makes a cogent case that one can learn as much, if not more, about the role of social forces in giving form and content to religious experience by studying cases such as this one, which failed to win church approval, as from the few success stories that have garnered more scholarly attention.

*The Meanings of Macho: Being a Man in Mexico City.* By Matthew C. Gutmann. Berkeley and Los Angeles: University of California Press, 1996. Pp. xi+330. \$45.00 (cloth); \$17.00 (paper).

Claire I. Viadro

*University of North Carolina at Chapel Hill*

With his ethnography of "what it means to be a man" in Mexico City, Matthew Gutmann embarks on a social science crusade "to imagine and invent new ways of describing, interpreting, and explaining cultural emergence and variation" (p. 13). Acknowledging his debt to feminist anthropology and gender studies, Gutmann ventures into the still largely uncharted territory of male gender identities. Although Gutmann's primary goal is to challenge "exaggerated" depictions of Mexican men as macho progenitors, the book also furnishes fascinating evidence of the ways in which stereotypes propagated by social scientists (e.g., machismo) trickle down and influence ordinary citizens, becoming part of their symbolic capital.

Drawing on a year of fieldwork in the slum neighborhood of Colonia Santo Domingo, Gutmann makes a convincing case for according as much importance to differences *within* gender groups as to those *between* men and women. Antonio Gramsci's concept of "contradictory consciousness" (which Gutmann defines as the fusion of a consciousness "inherited [uncritically] from the past" and a more practical, "transformative" consciousness) and the notion of "cultural creativity" (which recognizes that individuals are both "actors and critics of modernity") serve as Gutmann's two theoretical guideposts. Gutmann's somewhat dense discussion of these concepts in the first chapter gives way in ensuing chapters to concrete illustrations of Mexican men's and women's "creative efforts" to "cope with the gender relations they have inherited from past generations while simultaneously striving to fashion new approaches" (p. 10).

After a second chapter that situates Santo Domingo's birth as a squatter settlement in its historical context, Gutmann devotes his weightiest ethnographic chapter to an exploration of fatherhood. In this enterprise, Gutmann makes creative methodological use of a photograph (taken during an earlier stay in Mexico) of a man holding a baby. Using the photograph to initiate discussions about fathering, Gutmann discovers, in the process, that the photo functions as a Rorschach test, drawing forth widely divergent interpretations of the man in the picture, while also impelling viewers "to summarize what fathering entails for men generally" (p. 57). Gutmann peppers the chapter with references to his experiences as a new father and his infant daughter's inadvertent role as "field assistant," showing how these allowed him to gain further entree into the psychology and meanings of Mexican fatherhood; Gutmann's positioning as a party to the discussion adds to the chapter's interest.

Arguing that Mexican women not only have an intense interest in men's evolving gender identities but that, in many instances, they are serving as the "catalysts for change" (p. 92), Gutmann dedicates a chapter

to transformations among women, paying attention to women's formative role in Santo Domingo's consolidation as a community, and to the implications of female activism for gender relations in the *colonia*. Other chapters focus on sexuality, housework, alcohol consumption, and male violence. Gutmann furnishes plentiful examples of both contradiction and creativity, uncovering a diversity of attitudes and practices that confirm the relevance of examining within-group heterogeneity. In the process of scrutinizing behaviors traditionally "identified exclusively with one gender or another" (p. 190), Gutmann also finds that a "degendering" process is under way in some arenas. Men have affairs but so do increasing numbers of women; many women still work a "double day," but it has become acceptable for little boys to "pick up the tortillas"; considerable drinking takes place in male-dominated settings (e.g., bars), but women frequently join men in drinking at social gatherings. Although Gutmann makes a point of reserving his discussion of machismo for the end of the book, it surfaces as a recurrent theme in these chapters. Above all, respondents' references to machismo strengthen Gutmann's contention that cultural stereotypes "assert themselves not only in scholarly discourse but also in everyday conversation and consciousness" (p. 245).

Throughout, Gutmann elegantly weaves together theory and ethnographic detail, supplemented by often entertaining personal anecdotes and linguistic observations (he notes that the words "handcuffs" and "wives" are synonymous in Spanish). Notably lacking, however, is any organized discussion of the study's methods, which must be inferred from scattered comments. At times, Gutmann also airs his own contradictions, as when he argues in one breath that *colonia* residents would probably recognize four distinct male gender groups but, in the next, reiterates that masculinity "cannot be neatly confined in boxlike categories" (p. 238). A more substantive omission concerns the relative invisibility of international migration as an influence on Mexican gender identities; although Gutmann devotes a short section to migration in his chapter on sexuality, noting that "one out of every seven adult Mexicans" has lived in the United States (p. 134), one is left wondering how residents who migrate and return to the *colonia* affect the cultural creativity that Gutmann otherwise so ably documents. Nonetheless, Gutmann builds a strong argument for viewing gender relations, in Colonia Santo Domingo and beyond, as a series of creative contradictions.

*American Gay*. By Stephen O. Murray. Chicago: University of Chicago Press, 1996. Pp. 337.

Steven Epstein  
*University of California, San Diego*

"No one is interested in your lifestyle," a sociology professor at the University of Arizona informed Stephen Murray some 20 years ago in re-

sponse to a research proposal that Murray had submitted for the professor's approval. Murray recalls the incident in the acknowledgements to his recent book: "I was particularly shocked by this comment, because I didn't know that I had a lifestyle" (p. 277). In the intervening years, Murray has done more than simply prove the professor wrong; he has countered the very presumption that a call for the academic study of homosexuality is to be dismissed as a case of intellectual "special pleading" (p. 14). In his analyses of homosexuality around the world, Murray has demonstrated both that the study of gays and lesbians can be made rigorously sociological and that the development of gay roles, identities, communities, and movements has broader implications for sociological theory and for the sociological understanding of modernity.

Murray's latest book, *American Gay*, presents his analyses of diverse aspects of homosexual behavior, identities, and cultural forms in 20th-century North American societies as well as his critiques—often stinging—of others who have explored this domain. I think it safe to say that absolutely no one has mastered the social-scientific literature on sexuality and homosexuality any better than Murray has; indeed, this book is worth owning simply for the extensive bibliography and the abundance of detailed, often fascinating, footnotes. Beyond synthesizing and critiquing the existing literature, Murray presents his own empirical studies conducted with gay male populations in San Francisco and Toronto. *American Gay* is also consistently and insistently sociological, drawing freely on a range of theoretical perspectives and bodies of empirical literature, from role theory to network theory, from studies of migration to analyses of new social movements.

*American Gay* is really a collection of articles and essays, many of them published or presented previously, and the book lacks an overarching argument. To the extent that there is a guiding theme, however, it is what Murray describes as the "de-assimilation" of gay America—the relatively recent emergence and consolidation of institutionally dense, "quasi-ethnic" communities of lesbians and gay men. Such developments parallel the resurgence of ethnicity in the modern world and point to the limitations of theories predicting a telos of assimilation. To be sure, it is no longer news that in the contemporary world "ascribed characteristics . . . have taken on an importance quite out of keeping with the confident expectations of those in the grand tradition that . . . their significance would decline and eventually disappear" (p. 17). But, while the universalist presumptions of classical sociological theorists have often been taken to task, few have sought to investigate the implications of the emergence of communities organized specifically around sexuality: What does this mean for our understanding of localism and globalism, particularism and universalism, or identity and difference in the modern world? As Murray notes, the question is far from trivial, since, by the late 1970s, gay communities in North America "not only were more institutionally complete than ethnic communities," but were more successfully resistant to processes of assimilation (p. 191).



Part 1 traces the causes, consequences, and implications of the rise of lesbian and gay communities in the postwar era, with two excellent chapters devoted to the impact of the AIDS epidemic. In part 2, Murray critically interrogates a series of key analytical terms, including the "homosexual role," the meaning of "community," and the emergence of a concept of gay "family." Finally, part 3 addresses the complexities introduced into the study of gay "quasi-ethnicity" by the presence of diverse ethnic groups within lesbian and gay communities; chapters focus on African-American, Mexican American, and Asian/Pacific gays and lesbians. Throughout, Murray displays a sensitivity to the historical record and a commitment to relating micro patterns of social interaction to macro processes of social change.

Some of the book's virtues are also its weaknesses, however. Murray is skillful and often quite witty in challenging accepted interpretations—but sometimes he is so pugnaciously intent on proving a minor point that the argument veers off into digressions. He displays a bracing impatience with the tendency of each new generation of scholars to reinvent the wheel—but this useful skepticism at times gives way to overdismissiveness or even crankiness. (For example, Murray is too quick to paint recent queer politics as adolescent "acting out," and queer theory as trendy obscurantism, with little close analysis of the former and practically none of the latter.) Finally, *American Gay* shares the usual problems of books that are collections of independent essays: repetition, loose transitions between chapters, and the relative absence of narrative development. But in the end, these defects do not diminish the usefulness of this book as a critical summary of, and contribution to, the sociological literature on gays and lesbians in U.S. society.

*Am I Thin Enough Yet? The Cult of Thinness and the Commercialization of Identity.* By Sharlene Hesse-Biber. New York: Oxford University Press, 1996. Pp. viii+191. \$25.00.

Karen Honeycutt  
*University of Michigan*

In *Am I Thin Enough Yet*, Sharlene Hesse-Biber explores why and how women are urged to strive for an ideal of thinness that most can never achieve. Using case studies, extensive quotes from 60 in-depth interviews, and limited survey data, Hesse-Biber argues that most women are caught up in the "cult of thinness," in which they perform rituals of diet and exercise that will (they hope) allow them to achieve the ideal body championed by the mass media. She also shows how patriarchy and capitalism work together to reinforce the "mind/body" dualism—in which men are celebrated for their minds, women for their bodies—and how this dualism seduces women into joining the cult of thinness.

While others have used the cult metaphor and have made similar argu-

ments about patriarchy, capitalism, and the mind/body dualism, the strength of Hesse-Biber's work is that she ties the various forces together into a coherent explanation for why her subjects—and, by extension, most American women—are so obsessed with being thin.

In the first two chapters, Hesse-Biber discusses how the cult of thinness has grown in America. In chapter 1, she focuses on Delia, who is bulimic, and criticizes theories of eating disorders for their focus on individual-level explanations. In chapter 2, she describes the historical background that led up to the cult of thinness. In chapters 3 and 4, she powerfully indicts the food, diet, weight-loss, fitness, and plastic surgery industries. In chapter 5, she reviews the literature on why women are so obsessed with their bodies and discusses the difference between two models of desirable weight—the “medical” model (from actuarial data) and the “cultural” model (from popular sources)—and why women are so attached to the latter. This chapter and the next two are particularly rich with her subjects' own words, which makes them fascinating (albeit disturbing) reading. In chapter 8, she examines how previously unaffected groups—preteens, men, and ethnic women—are being enticed into joining the cult of thinness, and in chapter 9, she suggests that empowerment movements (e.g., organizing women to boycott certain advertisers or developing educational programs) may help women deprogram themselves from the cult of thinness.

This book has several strengths. First, Hesse-Biber's focus on a *sociological* explanation for women's obsession is in welcome contrast to the psychological, individual-level explanations (and solutions) often offered in books about weight. Second, her evidence (detailed in 36 pages of notes) is strong and extensive. Third, the many quotes she includes are especially good, from the woman who laments that she “can't go a whole day without eating” (p. 88), to those who constantly compare themselves to other women, to the many whose parents pressure them about their size (including one whose mother put a weight and calorie chart in her Christmas stocking).

One problem with the book, which Hesse-Biber shares with most authors of books and articles in this literature, is that her focus is almost entirely on white middle- and upper-class women. While she tries to remedy this in chapter 8 by exploring how the cult is spreading to preteen girls, men, and ethnic women, this chapter is perhaps her weakest. For example, she argues that given increased marketing to men, “we can imagine the Cult of Thinness spreading to the male population” (p. 106)—but this weakens her argument about the forces of patriarchy playing a large part in women's obsession (although capitalism is still implicated). In fact, she seems ambivalent here (perhaps realizing the contradiction in her argument), and she backtracks a little, noting that even though men may be more concerned with attractiveness these days, they are still not really full-fledged members of the cult.

I would have also liked to have seen more about the women who *do* resist joining the cult of thinness. While Hesse-Biber says in the introduc-

tion that she includes the experiences of several such women throughout the book, I wanted more details about them. For example, in chapter 2, Hesse-Biber argues that many women collude in constructions of beauty that are admittedly harmful to them because molding themselves to fit such constructions *can* bring rewards (a rich husband, a promotion at work). She then states that some women *do* resist, but she gives few examples. In a later chapter, she quotes one of her subjects who dismisses images she sees in women's magazines as "constructed people" (p. 34). This is a wonderful quote, but here and elsewhere Hesse-Biber skirts the obvious question: Just why are *these* women able to resist the message when most other women cannot?

Such minor quibbles aside, however, the book is provocative and worthwhile. Scholars in women's studies, particularly those who focus on beauty and appearance issues, will find the book a welcome addition. Hesse-Biber's engaging, accessible writing style make the book a good choice for interested laypeople as well.

*Tasting Food, Tasting Culture: Excursions into Eating, Culture, and the Past.* By Sidney W. Mintz. Boston: Beacon Press, 1996. Pp. xix+150. \$22.00.

Gary Alan Fine  
University of Georgia

Sidney Mintz is our great scholar of sweetness, though not of light. Mintz's 1985 volume, *Sweetness and Power* (Viking Penguin), depicting the complex relations between the consumption and production of sugar and the development of economies in Europe and the Caribbean, is one of the handful of masterpieces of "culinary" analysis: a banquet of a book. Mintz revealed the often surprising ways in which the production of cane was linked to the development and organization of slavery, as well as to the social structure of the European class system. All sweetness was not light.

If his earlier volume was a banquet, the present volume represents table scraps. I say this without disrespect (it is Mintz's own assessment). *Tasting Food, Tasting Culture* is not a broad work as was his earlier one. *Tasting Food* is a collection of small essays, some published (strangely the published locations are not included), some not. Most essays express intelligent themes included in his earlier work. Perhaps the most striking, memorable essay is the preface in which Mintz elegantly recalls his relationship with his father, a cook and sometime restaurateur. His moving account does more than pay filial tribute; it demonstrates in practical detail how food is linked to one's social and cultural location.

Mintz argues that eating is centrally implicated in both culture and social structure. Each of his eight main essays assert that humans invest their consumption choices with meanings about who they are and about

what they believe. Mintz, a profound student of Caribbean slavery, maintains in one essay that food consumption (and the creation of Caribbean cuisines) is a form of resistance in which slaves carved out a measure of freedom for themselves: what Mintz terms "tasting freedom." Obviously in historical essays of this type, it is a stretch to determine what historical actors felt that they were doing as they made their choices, and there is an underlying romanticism of the creativity of resistance, but there can be no doubt that oppressed people will use the choices that they are allowed within the cultural realm to create images of the self—whether in foodways, music, or oral literature.

Mintz's primary interest is in sugar and related sweet products, notably honey and marzipan. While never forgetting the economic placement of these foodstuffs, Mintz is equally intrigued by their cultural meaning. Sweetness (and similarly other culturally privileged foods) have had a wealth of moral significance surrounding them. As Elizabeth and Jay Mechling describe elsewhere, sugar combines elements of purity and danger, naturalness and indulgence. Recently, with the concerns of health linked to self-restraint (a self-denial society), coupled but contrasted with a push towards individual self-fulfillment (the me society), sugar consumption is bound to be treated with ambivalence.

Historically the cultural implications of sweetness were ambivalent. A "sweet tooth" is a product of the industrial revolution when sugar and sweet products could be mass produced and provided to large segments of working classes. The decrease in the manufactured price of sugar contributed to this taste, notably, though not exclusively, in Great Britain. The "conquest of honey by sucrose" involves the "ousting of a local, small-scale industry by an international large-scale industry," marking "a fundamental change in the nature of European economic life" (p. 65). It is in these seemingly mundane moments of human history that the world system is formed. This economic change is both a result and a shaping force of culinary choices.

Marzipan, a minor, if delightful, confection, composed of sugar and almonds, is similarly a lens through which battles over what constitutes purity could be fought. As Mintz points out, marzipan is a marker of the interchange between Islamic and Christian cultures: it entered Europe both through Near East trade routes and through the Moorish conquest of Spain. Despite an image of sweet purity, it was the remarkable flexibility of marzipan, capable of being shaped as bananas, frogs, or pork chops, colored as the rainbow, that provided its popularity in the shops of Lubeck, Milan, and New York. While we dream of a pure world, it is the imaginative power of cultural transformation by which we make that world our own.

Of all of Mintz's theses, perhaps the most controversial is his claim that cuisines are not national but regional. In the concluding chapter, "Eating American," Mintz disputes the existence of an American cuisine. Most dishes that we consider part of the American bounty belong to regional cuisines or commercial preparations—the first seemingly desirable,

the latter far less so. Indeed, Mintz willingly frets about the spread of American eating habits (and we do have eating habits, if not a cuisine) to the rest of the world. The chapter closes on an odd note from a student of slavery. American society, Mintz concludes, will not be destroyed by "the sweet poison of consumption," but, perhaps, by our "obsessive notions of individual freedom." For a species in which oppression has been all too common, personal freedom might deserve a bit of obsession.

*Kitchens: The Culture of Restaurant Work.* By Gary Alan Fine. Berkeley and Los Angeles: University of California Press, 1996. Pp. xi+303.

Dennis Ray Wheaton  
*Chicago Magazine*

Here is a sociologist who can stand the heat. Fine spent four months in the mid-1980s doing ethnography in the kitchens of four "upper stratum" restaurants in Minnesota's Twin Cities area. He examined the often pressure-cooker working conditions—including the management of time and flow and the negotiation of place and space—the economic forces, and the aesthetic components involved in kitchen work (he is interested here in cooking, not dining). He succeeds impressively in the backstage part, but Fine is less convincing in melding his interactionist microanalysis to the macroinstitutional environment of restaurants because of the limitations inherent in the time and place of his study.

Fine is a clear writer who weaves his own insights with those of thinkers from George Orwell to Ludwig Wittgenstein in analyzing what happens as people cook professionally for other people. He explains persuasively how the skill of organizing "multiple tasks under intense pressure" requires "sub-rosa techniques" (pp. 22–23): approximations, shortcuts, and tricks of the trade—some of which many diners will not enjoy hearing about. He offers observational gems, such as how restaurants often serve diners best at peak hours when a full staff is working at an efficient, smooth flow.

Fine's backstage observations will endure, but when he moves to the institutional environment of restaurant organizations, his picture becomes outdated. He saw only a thin network of ties in the culinary world of Minneapolis–St. Paul in the mid-1980s. He acknowledges that if he had looked at New York or Paris, "a tighter network would surely have been discovered" (p. 134), but he concludes that in the restaurant industry, "an overarching community is not evident. . . . Whether this will be overcome remains to be seen, since today restaurants operate as if they were isolated islands, rather than part of an archipelago" (p. 137).

Currently, the organizational networks among restaurants, purveyors, and the public have gotten dense everywhere. A community of restaurants in voluntary and professional associations now extends from internationally acclaimed temples of gastronomy to small town pizza joints.

The National Restaurant Association (mentioned only in the introduction) is a powerful Washington lobby that attracts to its annual show in Chicago 100,000 members hungry for the latest marketing angles, trendy foods, and expense-lowering techniques. An important part of the social world of many chefs is volunteering to cook for charity benefits and food and wine festivals across the country; just in Chicago, Meals-on-Wheels fund-raisers bring together and offer publicity to dozens of chefs from Durham to Phoenix. Education-oriented groups like the burgeoning American Institute of Wine and Food, public relations firms, advertising, and an increasing appetite by the public for restaurant reviews and food journalism—covering “cheap eats” as well as haute cuisine—have all become dynamic linkages between restaurant kitchens and the public. None of these were as important a decade ago, but today they connect restaurants nationwide into an archipelago invisible on Fine’s map.

The study also misses how back and front stages have become interpenetrated. A recent widespread trend has been the installation of kitchens open to the gaze of diners as a form of theater for a public enthralled with the glamor of chefs and cooking. Chefs costumed in their kitchen whites are expected to go out front and interact with customers. Fine also neglects the growing complexity of market niches, including those defined by big restaurant corporations such as Hooter’s, Wolfgang Puck’s expanding empire of casual fine dining, or Morton’s Restaurant Group, whose 34 steakhouses sell \$30 porterhouses and cabernet sauvignon with the same level of standardization that McDonald’s uses for \$3 burgers and colas.

Fine’s treatment of restaurant aesthetics will be of interest to cultural sociologists: “Central to my analysis is the artistic character and definition of work, a rare concern in much social-scientific discourse. . . . Food involves more sensory dimensions than any other art form, except, perhaps, the ‘art’ of love. This aesthetic richness allows vast leeway in choices of food preparation, a diversity that may have hindered the development of a formal aesthetics of cuisine: a theory of eating” (p. 13). But the argument is not as developed as promised, and the ethnography does not support it as strongly as it should. While Fine states that his aesthetic argument applies to the steakhouse as well as to the rest of his sample, over half of the examples in the two chapters on aesthetics come from his most “explicitly ‘artistic’” French restaurant.

His aesthetic analysis is also at times undermined by his economic analysis. To argue that there is substantial elasticity in pricing restaurant food because “a steak is essentially a steak” (p. 156) is tantamount to claiming the essential sameness of ketchup and morel sauce. This denies the real aesthetic differences between the menus of different restaurants and the factors behind their pricing. *Kitchens* is a minor book by a major sociologist, full of interesting tidbits but leaving the reader hungry for what Fine has served up in better studies.

*Home and Work: Negotiating Boundaries through Everyday Life.* By Christena E. Nippert-Eng. Chicago: University of Chicago Press, 1996. Pp. xviii+325. \$48.00 (cloth); \$16.95 (paper).

Marjorie L. DeVault  
*Syracuse University*

What is a boundary? How are boundaries sustained and how are they crossed? These are questions raised in provocative ways in Christena Nippert-Eng's study of the home-work routines of a group of working urban Americans. Key chains, calendars, morning and evening drinks, commuting routes and practices—such physical details of daily practice provide fodder for a lively analysis of the constitution and implications of "home" and "work" as cognitive categories.

Nippert-Eng asks how people "draw the line" between home and work. She shows, first, that there are many ways to do so: some individuals tend more to integrate these realms, while others segment them. Integration and segmentation occur along many dimensions. Some people work at home and conduct personal business at their jobs (if they are allowed to); some display family photos at work and work products in their homes; some combine their calendars while others keep separate business and family records. There are finer distinctions to be made as well: those who work at home may spread their supplies throughout the house or contain them in a single room, "commuting" regularly from the residential parts of the house to a home office. And one man who sometimes reads the newspaper at his job gets up from his desk to do so and moves to a side table. All of this suggests, most convincingly, that the apparently stable categories "home" and "work" are artful constructions arising from daily activity.

Beyond calling attention to the "cognitive engineering" involved in boundary marking, Nippert-Eng considers how people bridge the boundaries thus created and how they are constrained in these matters by the structure and demands of their jobs and families. The analysis is based on lengthy, open-ended interviews with 72 employees at "the Lab," a large scientific research facility in the northeastern United States. Interviewees were drawn about equally from three occupational groups: personnel workers (mostly women except for supervisors), machinists producing lab equipment (all men), and research scientists (half men and half women). I did not find any information about the informants' racial/ethnic identities.

Grounding the study in a single, large organization is a productive strategy: it permits inclusion of a range of working lives, without opening the field so broadly as to make it difficult to interpret differences that are found. Scientists, not surprisingly, are most likely to adopt integrationist practices: for many of them, home and work merge almost seamlessly—in time, space, and interpersonal contacts (one man was eager to tell about sexual encounters in his office). Machinists segment most starkly:

the organization of their work produces segmentation through a highly controlled work day and rigid enforcement of rules about personal business on the job. These features of the job are also related to a long tradition of union resistance to employers' demands and union machinists' strong sense of entitlement to "their own time." Whatever desires they may have for integration appear in the few niches available—family photos attached to tool boxes, for example. White-collar workers in personnel are less likely than scientists to integrate, partly because of their more structured jobs, but they have more freedom than machinists to devise individual adaptations and correspondingly more varied practices. Varied family situations also affect home-work routines, though in more complex ways that seem less amenable to summary analysis.

Nippert-Eng uses evidence quite skillfully to support her interpretations of mundane practices. For example, she explicates not only the most typical preferences for caffeinated drinks in the morning and alcoholic ones later in the day but also the atypical case like the woman who stops for coffee before heading home to an especially demanding family situation. She develops extended metaphors—of sculpture, bridge building, and dimmer switches—to an extent that sometimes threatens to substitute for explanation. Nippert-Eng locates her project in an emerging field of "cognitive sociology," and she emphasizes norms and mentalities, with practice apparently following these cognitive constructs; however, I sometimes wished for more explicit discussion of the mechanisms that connect understandings and routines.

A sociological literature on work-family connections, referred to only glancingly here, has arisen from middle-class women's increased labor-force participation. Much writing in this area is quite pragmatic, taking pretty much for granted the categories "home" and "work" (though there are exceptions—studies of topics such as family day care, paid domestic labor, commuter couples, and wives' contributions to their husbands' careers—that would complement Nippert-Eng's analysis). This book should encourage us to look behind the powerfully commonsensical categories "home" and "work" in order to better understand their expression and consequences in particular lives. Like so much compelling sociology, it invites the reader to contemplate the significance of ordinary activity and provides a new way to see daily routines.

*Segmented Labor, Fractured Politics: Labor Politics in American Life.*  
By William Form. New York: Plenum Press, 1995. Pp. xviii+380. \$42.50.

Sam Rosenberg  
*Roosevelt University*

For at least a decade or a decade and a half, the labor movement has not been particularly politically relevant. Rather, it has been marginalized as a weak "special interest." Currently, steps are being taken to increase



the visibility of organized labor in the political process, to mobilize the membership, and to build support for labor's positions. Under the new leadership of John J. Sweeney, the AFL-CIO plans to devote \$35 million to elect a Democratic Congress and to reelect Bill Clinton in 1996.

Why has the labor movement faced declining political fortunes? What changes must unions and the AFL-CIO make so that organized labor can become a more effective voice for workers in the United States. With the perspective that divisions within the working class lie at the heart of labor's political problems, William Form provides answers to these questions.

The book is divided into three main parts. In part 1, Form relies on theories of economic and organizational segmentation to help explain changes in labor's class composition and changes in labor's political and organizational environment. Since the mid-1950s, union membership as a percentage of the labor force has been shrinking. Though diminishing in relative size, its changing membership has made the labor movement more representative of the country's labor force. Workers in the United States are split or segmented along many dimensions including gender, race, age, occupation, and earnings. Furthermore, the ex-AFL unions, ex-CIO unions, public sector unions, and service unions constitute four significant divisions of the labor movement.

A segmented working class and a divided union movement have made it difficult for organized labor to achieve policy consensus and concerted political action. Many union members do not vote the "labor line" in elections. Labor has not built a sturdy and effective political machine at the local level, a prerequisite for political success at the state and national levels, as well as locally.

Given the importance he places on local labor politics, in part 2, Form presents well-researched case studies of labor's political activities in Cleveland, Columbus, and Cincinnati, three Ohio cities with different economic, labor, and political traditions. They are based on interviews of local, regional, and national labor leaders and officials of the leading political parties in each city. Though conditions differed by city, major unions varied in their commitment to the local Committee on Political Education (COPE) and the Democratic Party and in their political aims. The ex-AFL unions were locked into a political model of job-oriented unionism. While their rhetoric was not the same as the ex-AFL unions, public sector unions also emphasized job-oriented issues. The ex-CIO unions were more likely to focus on social welfare matters important to workers as a whole as did the new service unions. A divided labor movement is tantamount to a politically weak labor movement.

In part 3, the author demonstrates that labor's political difficulties locally are mirrored nationally. Divisions within organized labor have weakened its influence within the Democratic Party. As labor's membership became more heterogeneous, it acted more like a special interest than a social movement. COPE's legislative agenda became more narrowly

labor oriented, rather than incorporating consumer, class, or general welfare concerns. Splits between the labor leadership and the rank and file resulted in many union members not voting for the COPE-endorsed candidate for president, virtually always a member of the Democratic Party, or not voting at all.

Labor's current political difficulties are not insurmountable. The author believes that the labor movement can become a stronger voice for the country's workers. To increase union membership, more resources must be devoted to organizing and the labor movement must become more concerned about the welfare of nonunion workers. Where unions exist, they must cooperate with management in raising labor productivity, thereby stemming the tide of disinvestment from unionized facilities. While a larger membership automatically signals greater political potential, greater numbers, by themselves, are not sufficient for increasing labor's political influence. In addition, more resources must be devoted to COPE, and COPE must become more effective in transmitting to union members the importance of labor's political goals.

This book clearly discusses the challenges facing the labor movement as it strives to become politically relevant once again. As such, it is very timely. However, the book reads slowly in places, particularly where Form is presenting the findings of the city case studies. The overall theoretical framework is at times lost in the details, and the programmatic suggestions do not speak very directly to the problems raised by segmentation within the labor movement.

Nevertheless, this book is an innovative attempt to apply segmentation theory to the current political difficulties facing organized labor. If the book's premise is correct, it will take more than dollars spent on presidential and congressional elections to revitalize the labor movement's presence in local, state, and national politics.

*Selling the Air: A Critique of the Policy of Commercial Broadcasting in the United States.* By Thomas Streeter. Chicago: University of Chicago Press, 1996. Pp. xvi+336. \$52.50 (cloth); \$16.95 (paper).

William Hoynes  
Vassar College

Thomas Streeter's *Selling the Air* provides an original and theoretically sophisticated analysis of commercial broadcasting in the United States. Streeter's primary concern is with the construction and maintenance of property relations within broadcasting, a remarkably complex topic given the political maneuvering and bureaucratic activity required to constitute ownership boundaries in the world of electronic media. Streeter makes a compelling case that broadcast property and the markets where buying and selling take place were (and are) created by government action. The organization of broadcasting, then, cannot be adequately understood by

labelling it simply private or free market. Instead, Streeter argues that commercial broadcasting is best explained within the framework of what he calls "corporate liberal" habits of thought. Commercial broadcasting, in the author's words, "embodies a faith in the broad liberal framework of property rights, the market, and minimal government, coupled to and qualified by a faith in expertise, administrative procedure, and a reified, paternalistic notion of the public good" (pp. 109–10).

Streeter's analysis of property, government, markets, and corporate liberalism effectively demonstrates that our system of commercial broadcasting is neither natural nor inevitable. This argument is both persuasive and nuanced and is well illustrated by careful historical analysis. Streeter's examination of the early struggles over radio—the marginalization of amateurs who had helped to develop radio and who represented an alternative organizational approach, the elevation of for-profit uses of radio over nonprofit uses, the institutionalization of one-way broadcasting over two-way communication, the invocation of the public interest as a rationale for commercial organization, and communications legislation that legitimized this centralized, commercial structure—is first-rate historical sociology. This demonstration of the ways that political and social choices created a specific broadcast industry and closed off alternatives makes *Selling the Air* required reading for those interested in the roots of our current communications industry.

When Streeter turns his attention to broadcast policy, he shows how the very decisions that shaped the broadcast industry—particularly those that organized media along commercial, for-profit lines—lie outside policy discourse. Broadcast policymakers, whom Streeter identifies as an interpretive community who abide by specific discursive rules, take the commercial organization of broadcasting for granted so deeply that a wide range of policy questions are ignored. The definition of what is "practical" tightly constricts policy discussion; ideas that question the taken-for-granted organization of the industry are dismissed or never articulated because they are defined as impractical. There is no conspiracy here, only the workings of a widely shared common sense. The consequences, nonetheless, are significant, as policy analysis works within and helps reproduce an institutional structure that appears to be natural and unalterable. Streeter's analysis of policy discourse effectively links the early history of radio with the contemporary media industry, showing the assumptions that have been more or less stable for the past 75 years. However, the analysis of the interpretive community of policymakers would have benefited from a more specific discussion. As it stands, the rules, premises, and rhetorical strategies operative in this interpretive community are not sufficiently located in explicit would-be policy questions.

The last part of the book examines the constitution of three key forms of broadcast property: stations, copyright, and audiences. The discussion of station licenses as property—after all, federal law explicitly states that licensees do not own the airwaves, even though licenses are bought and sold as commodities—is impressive and full of insight for ongoing de-

bates about the meaning of broadcast property in the impending shift to high definition television. The analysis of copyright shows how bureaucratic procedures are used to simulate statistically markets in situations where market exchange would otherwise be impossible, resulting in a kind of "postmodern property," with elaborate indicators standing in for actual markets. And the discussion of viewing as work, and why it is not widely seen as such, develops an intriguing feminist argument that defines television viewing as analogous to housework—nonmarket activities that are devalued.

The principal weakness of *Selling the Air* is that its insights are not fully developed. For example, the consequences of identifying property as postmodern are never really explored, and the feminist analysis of the audience is far from comprehensive. More generally, at times, the book reads like a series of related essays rather than a book-length argument. Perhaps this is due to the ambitious nature of the project; reconsidering the commercial organization of the media industry in a theoretically informed way is no small task. Nonetheless, by posing such provocative questions and grounding the analysis of media in the meaning of property, Streeter has made a real contribution, not only for scholars and concerned citizens, but, if they are so inclined, for policymakers who will benefit immeasurably from this learned book.

*Power, Ideology, and Control.* By John C. Oliga. New York: Plenum Press, 1996. Pp. xxi+321. \$59.60.

Richard Münch  
*University of Bamberg*

John C. Oliga's *Power, Ideology, and Control* contributes to an attempt being made by a group of scholars to link systems thinking and critical theory, and advance management sciences to a more critical stance. The book starts with an account of such attempts then elaborates its own approach to critical systems thinking with regard to the linkage of three aspects of social formation: power, ideology, and control. The guideline of critical theory is Jürgen Habermas's theory of communicative action.

The systematic analysis begins with metatheoretical concerns about social order, human freedom, and happiness. Oliga gives an account of three images of social order with regard to their social and intellectual roots and their ideology. The first type is individualism. It conflates utility and value, self-interest and rational behavior, market exchange and social efficiency, and it contributes to the reproduction of capitalist relations of production and domination. The second type is unitarism. It locates social order in value consensus and common interest and conceives of power as a neutral or positive force. In doing so, it overlooks conflicts of interests, domination, and the negative side of power. The third type is pluralism. It can be conceived in terms of a competitive pluralism where social order

is the outcome of bargaining processes between interest groups, or in terms of a corporatist pluralism where social order is the outcome of the collaboration between the state and major associations that bundle together a greater number of interests. Competitive pluralism underestimates the unequal access of interest groups to the bargaining process and their unequal power. Corporatist pluralism exaggerates the neutral position of the state and misses the fact that the state is often captured by the interests of the most powerful associations.

The next step is an analysis of different conceptions of power—subjectivist, objectivist, and relational—leading to the outline of a conception of power called “contingent relationalism.” According to this view, power analysis shows the location of power in the relations between social actors, it reveals the creative potential of oppressed groups, and it contributes to their empowerment. This power analysis is complemented by an analysis of ideology that distinguishes historical and naturalistic conceptions. Historical conceptions are provided by historical materialism, critical theory, and dialectical materialism; while naturalistic conceptions are provided by psychologism, *weltanschauungs* analysis, class analysis, structuralism, functionalism, and modes of production approaches. This analysis of ideology seeks to reveal ideological tendencies in positivist and structuralist hard systems approaches and in soft systems approaches also. The aim is liberating systems theory from ideology. The analysis of power and ideology shall contribute to the enlightenment and empowerment of the people.

The final part of the book is concerned with the question of how we can shape our own destinies. The analysis concentrates on extending systemic management theories of control and on elaborating a critical theory of managerial control. This task is completed with a systematic exposition of Habermas’s theory of communicative action. In Oliga’s eyes, Habermas overcomes the limits of the older strands of critical theory by his linguistic turn. The locus of critical reason is no longer the subjective consciousness but language with its potential for consensus formation between free and equal speakers. However, according to the author, Habermas’s critical emphasis is on enlightenment and should be complemented by empowerment processes against coercive forces of power and transformative action against dominative control. This critical approach is demonstrated on developing countries.

The introduction of critical theory into the management sciences certainly contributes to improving their reflective capacity. The way taken by Oliga to accomplish this task is adequate. The book primarily reads like a textbook with much emphasis on the systematic structuring of the material and on its schematic presentation, including a great number of figures. The style of writing corresponds to the style of the management sciences. For the sociologist, the text reads a bit too schematically. It is more concerned with the representation of ideas in short sections than with their elaboration in a discursive argumentation. This is why this plea for critical systems thinking is not very strong in critically reflecting

the feasibility of that approach itself. Systems theory is nearly exclusively understood in the sense of its representation in the management sciences whereas the approaches most important in sociology—those of Talcott Parsons and Niklas Luhmann—are not systematically discussed. With its concentration on the link between power, ideology, and control, the book has a tendency to underestimate the complexities of societal formations and thus to simplify critical analysis too much. Nevertheless the book is worth reading, particularly for those interested in a critical reflection on management sciences.

*Pedagogy, Symbolic Control, and Identity: Theory, Research, Critique.*  
By Basil Bernstein. London: Taylor and Francis, 1996. Pp. xiv+216.  
\$23.95 (paper).

Peter W. Cookson, Jr.  
*Columbia University*

For over 30 years, the work of Basil Bernstein has been an enigma and a challenge to sociologists, in general, and sociologists of education, in particular. We know that Bernstein is brilliant, we know that he is saying things about power and control that are strikingly original, and we know that his work relating everyday language to the larger social structure holds open the possibility of creating new theoretical approaches. But, if all of this is true, why is it so hard to embrace Bernstein's work, and why has so much of his work remained marginal to mainline sociology? Is it that his work lies outside the normal science of sociology? Is it his sometimes opaque use of language? Or is it that Bernstein's work is simply so idiosyncratic that it floats above the field like a colorful air balloon dancing among the clouds of Durkheimian abstraction?

In his most recent book, *Pedagogy, Symbolic Control, and Identity*, Bernstein provides us with another opportunity to explore the enigma and challenge of his thought. In this work he develops his theoretical explorations of classification (the means by which power relations are transformed into specialized discourses) and framing (the means by which principles of control are transformed into specialized regulations of interactional discursive practices). He also discusses the necessary and effective conditions of democracy, defends himself against his critics, and links his theoretical propositions to empirical research.

Given the multiple agendas of the book, it is difficult to summarize a theoretical or empirical bottom line that produces a single powerful reaction. Rather, the book reproduces almost perfectly Bernstein's thoughts with all their possibilities and problematics. The tantalizing original insight (i.e., "It is silence which carries the message of power" [p. 20]) is followed by an abstruse phrase (i.e., "Now, in this process of delocating a discourse, . . . that is, taking a discourse from its original site of effectiveness and moving it to a pedagogic site, a gap or rather a space is

“created” [p. 47]), which, in turn, is followed by an abstract set of theoretical constructions, complete with labels and diagrams that do not quite seem to relate to the text or to other theoretical work. Here, I think, is the key to the Bernstein mystery.

In a world where sociologists are too often trained to regress to the theoretical mean, Bernstein refuses to accept conventional language and approaches. He chooses, instead, to strike out on his own, creating new thought models, languages, and definitions of what constitutes evidence. Bernstein’s subject is fundamentally Bernstein, his puckish unconventionality, his insistence on courageous reasoning, and his proclivity for metatheory, which is at once highly structured yet simultaneously rootless. The world according to Bernstein can be understood structurally, but this approach to understanding requires the transmutation of ordinary language into a special code with its own syntax, grammar, pacing, and vocabulary.

Whatever the true contribution of Bernstein to sociological theory, one thing is clear. Unlike many theorists who choose to work in scholastic abstractions—Hegelian heaven as a friend refers to it—Bernstein is committed to a sociology of liberation (a word he would most definitely *not* choose). He is stung by the injustice of a social system that slices and dices human kind according to class, race, and gender. He does not accept the status quo and has attempted to break through bourgeois sociology by reframing Durkheim’s project, that is, Bernstein rejects the therapeutic psychological proclivities that are the secret vice of most sociological theory and embraces a radical and complete structuralism that relentlessly pushes us to the limits of conceptualization. Bernstein knows that class regulation goes deep into society and into the souls of children. Working-class children learn that they are school failures at an early age. That is the unspoken purpose of compulsory education in advanced capitalistic societies. But how is the process of class regulation disguised from those who are its victims *and* those who are its beneficiaries? Here Bernstein’s insights are illuminating, “Education preserves structural relations between social groups but changes structural relations between individuals. These changes in the structural relation between individuals are sufficient to create the impression of a general and probable movement. This enhances aspiration, motivation and commitment but failure, especially early school failure, can deaden these attributes” (p. 11).

Bernstein is a rare and brave thinker who has charted his own course with courage and consistency. He is an exemplar of a sociologist who is also an intellectual engaged in the reconstruction of society. In an academic milieu that tends to reward the conventional, it is perhaps entirely predictable that Bernstein’s work should be underutilized. The price of originality is often exile, a paradox that Bernstein himself might enjoy, if only it did not hurt so much.

*James S. Coleman*. Edited by Jon Clark. London: Falmer Press, 1996. Pp. vii+452. \$29.95 (paper).

Thomas Voss  
*Leipzig University*

This book is rightly announced as the "first book to debate critically James S. Coleman's work and contribution to sociology and the social sciences more generally." Since Coleman's contributions are singular not only for their depth and pathbreaking impact on numerous fields but also their breadth and comprehensiveness, this is an ambitious task. In fact, one could describe James Coleman as a scholar with at least three "components": First, there is Coleman 1, the rational choice theorist and author of *Foundations of Social Theory*. Coleman 2 is the model builder who pioneered mathematical sociology and authored the outstanding book *Introduction to Mathematical Sociology*. Properly understood, mathematical sociology's main task is the supply of methods for constructing social theories. As expressed in the *Introduction*, mathematics is indispensable to the achievement of micro-macro transitions. Many of Coleman's mathematical models are "synthetic theories," that is, theories that begin with assumptions about individual behavior and end with deductions on the system level. Coleman 2 also was a sophisticated statistical model builder who contributed to the multivariate analysis of qualitative data (including event history data). Third, there is Coleman 3 the famous sociologist of education and empirical researcher. This Coleman, authored *Adolescent Society* and was the principal investigator of the so-called Coleman report as well as of several other reports on the effects of school organization. All of these multiple Colemans had their distinguished careers and were regarded as *the* leading figures in their fields. Coleman himself (as quoted on p. 4) considered his *Foundations of Social Theory* as his magnum opus and *Introduction to Mathematical Sociology* as his other most important book. Unfortunately, half of the papers in the volume debate Coleman 3.

The book starts with an extended introduction by the editor on the complicated evolution of the volume. Coleman had an intensive exchange with the volume's editor on its contents, which is in part quoted in the introduction. The late Coleman's illness made it impossible to include a chapter with Coleman's replies to his critics. Only some shorter transcripts of drafts for a concluding chapter by Zdzislaw A. Coleman could be printed as an appendix to the book. Other appendixes include a piece on Coleman by Robert K. Merton, Coleman's teacher, and the speeches given at the memorial service by some of his friends, colleagues, and former students.

Space limits prevent the description of the 21 chapters in the volume (two articles are short contributions by Coleman himself). Only two articles consider the relations between Coleman's multiple researcher selves. An excellent chapter by James J. Heckman and Derek Neal debates the



relation between Coleman 3, the sociologist of education and empirical researcher, and Coleman 1, the rational choice theorist. Furthermore, Coleman 3 seems to be, at least *prima facie*, in conflict with Coleman 2, the quite sophisticated mathematical and statistical modeler. This is because Coleman 3's business was to tell simple causal stories with the help of quite simple statistical models. Remarkably, Heckman, being himself a virtuoso in econometrics, defends Coleman 3's approach to empirical research. Heckman summarizes his discussion with the words: "the main lessons from most of his empirical studies survive to this day" (p. 102). A stimulating article by Aage Sørensen attempts to blend Coleman 2 and Coleman 3 using a data set from one of Coleman's more recent education studies. Disappointingly, Sørensen carefully avoids inquiring into implications of an explicit rational choice approach for his problem.

Only one other chapter, written by David J. Bartholomew, addresses Coleman's mathematical sociology. Bartholomew shows how Markov process models of mobility can be used to generate measures of mobility in a way that is not *ad hoc*. Bartholomew suggests that this is consistent with the spirit of Coleman's celebrated applications of stochastic process models.

There are seven articles that mainly deal with Coleman 1, the rational choice theorist. In an interesting chapter, Richard Swedberg describes Coleman's rational choice perspective on economic sociology. Thomas J. Fararo analyzes how Coleman's ideas about the foundations of social theory are embedded into the various traditions of general theoretical sociology. Siegwart Lindenberg claims that Coleman's *Foundations* represents a particular version of rational choice sociology called "constitutionalism." Constitutionalism assumes, according to Lindenberg, *independent* rational individuals and therefore fails to analyze relations between people. Lindenberg's sketch of another version of rational choice, called "relationalism," is short and therefore somewhat shaky. Not every reader will be convinced that this kind of relationalism constitutes a "paradigm shift" in sociological rational choice theorizing. By the way, neither Lindenberg's nor any other chapter in this volume, comes up with the obvious idea, that some limitations of so-called constitutionalism may be overcome by using game theory instead of standard neoclassical or expected utility theory. Game theory deals with interacting individuals and is applied by an increasing number of economists, political scientists, and some sociologists to analyze relations between actors. A chapter written by Randall Collins gives a very general discussion of the future of rational choice sociology and its potential for the unification of sociology.

Among the many articles on Coleman 3, there is a remarkable contribution by Denise B. Kandel, who reviews and criticizes Coleman's major works on youth, adolescence, and schooling. There are also authors (Barbara Heyns, Karl L. Alexander, and Doris R. Entwistle) who do not address Coleman's work directly but present results of research that follows more or less closely Coleman's footsteps.

All in all, this carefully edited volume is a very valuable tribute to

Coleman's work. It also contains some personal impressions of James Coleman's personality. Unfortunately, there are not many contributions that profoundly debate Coleman's most important theoretical works on mathematical sociology and rational choice theory. Most articles are good but not all of these good articles contain a detailed discussion of Coleman's ideas. In this respect, some articles would have been more appropriate for a festschrift than a discussion volume.

*The Causes of Human Behavior: Implications for Theory and Method in the Social Sciences.* By Lawrence B. Mohr. Ann Arbor: University of Michigan Press, 1996. Pp. ix+183. \$37.50.

Daniel Little  
*Bucknell University*

Lawrence Mohr, a political scientist with empirical research interests in organizations and organizational behavior, offers a practitioner's analysis of some fundamental philosophical issues in the foundations of the social sciences. Two questions loom particularly large for Mohr: What is involved in providing a good causal explanation of social phenomena, and in what sense are intentional human actions "caused"? These are important questions for those of us interested in the philosophy and methodology of the social sciences. Mohr's treatment, however, adds less to our understanding of these issues than one might hope.

Causal explanation is the central topic. But in addition to an extended discussion of causation, the author provides an analysis of the causes of intentional behavior (do thoughts cause behavior?), an examination of some possible implications of the author's findings for research methodology, and a discussion of the principle whether there could be strong lawlike generalizations in the social sciences. This conclusion depends on the fact that social outcomes are the result both of causal processes and "encounters"—chance circumstances that do not fall under lawlike generalizations (pp. 131–32).

Consider first Mohr's analysis of causation. Mohr argues that current philosophical treatments of causation are unsatisfactory, particularly for the social sciences. He summarizes these theories under a "Humean regularity" view and a "necessary conditions" view. In place of these theories, he argues for a pair of conceptions of causation: physical causation and factual causation. Physical causation, he believes, is the fundamental notion; it involves "a relation between a force on an object and a motion of the same object" (p. 44). More generally, physical causation is mechanical causation, in which the physical characteristics of the causing factor produce or bring about the characteristics of the effect. Mohr regards this as the bedrock causal concept but observes that many causal ascriptions do not in fact refer to physical cause couples. Instead, many causal ascriptions refer to relations among statements or facts: the fact that the road

was slick caused the fact that the accident occurred. This is what Mohr refers to as "factual causation." Mohr defines factual causation in the following terms: X was the factual cause of Y if and only if X and Y both occurred and X occupied a necessary slot in the physical causal scenario pertinent to Y (p. 27). (Parenthetically, it does seem odd to call X the factual cause if it is merely one out of many necessary "slots" in the physical causal scenario.)

In my view, this analysis of causation fails to illuminate. There are very hard questions to be answered if we are to have a satisfactory analysis of social causation, but the retreat to physical causation seems not to be helpful in attempting to answer such questions. (Does it make sense to ask what was the physical cause of the fall of the Roman Empire?) There is a substantial literature within the philosophy of science on the issues of causation and causal explanation (e.g., work by Wesley Salmon, Nancy Cartwright, or Jon Elster). Within the context of the current state of debate, Mohr's analysis of physical and factual causation appears unhelpful.

Turn now to Mohr's treatment of the causation of behavior. He asks a simple question—Do thoughts (purposes, reasons, wishes, desires) cause behavior (physical movements of the body)? His answer is negative, because we sometimes decide to do X but fail to do so (the irregularity problem). "The connection between our thought categories and the appropriate behaviors does not seem to be necessary or lawlike" (p. 63). He argues that we should here take our cue from the analysis of physical causation that preceded and look for a set of physical circumstances that could be said to cause behavior. Thus Mohr believes that it is necessary to identify the physiological mechanism that underlies the "reason-behavior" nexus if we are to have adequate causal explanations of behavior at all (p. 58), and he offers the "affect-object" paradigm as the basis of further theorizing in this area (p. 70). This is highly speculative account of how desire and thought might be embodied within the central nervous system. Here we are in deep and troubled waters, for it seems fairly clear that cognitive science and neurophysiology are still very far from having convincing answers to questions such as What is the physiological basis of thought? Fortunately, most other observers have concluded that the sort of intertheoretic reduction that Mohr aspires to is in fact unnecessary. We can give satisfactory answers to questions such as How do falling interest rates cause rising levels of investment? without having a neurophysiology of investor decision making. More generally, it is not necessary to reduce causal assertions at one level of description to causal relations at a lower level of description.

In summary, Mohr deserves credit for focusing on a series of issues that are fundamental to our understanding of social research. Mohr's discussions of many of these issues ask some of the right questions, but his answers are often less than satisfying.

*Faculty at Work: Motivation, Expectation, Satisfaction.* By Robert T. Blackburn and Janet H. Lawrence. Baltimore: Johns Hopkins University Press, 1995. Pp. xvii+389.

Zelda F. Gamson  
*University of Massachusetts, Boston*

It is curious how little interest most academics show in their profession and the industry in which they work. Now more than ever, it is critical that faculty become more knowledgeable about these matters. As we go about our daily business, higher education is undergoing a massive shift. A whole generation of faculty who entered higher education during what turns out to be a rather brief golden age is retiring, and new faculty are entering a more depressed industry. Appropriations for student aid, institutional support, and research are not as forthcoming from the state and the federal governments, as higher education becomes only one of many worthy activities clamoring for attention. At the same time, higher education has become a central institution in U.S. society. As such, it draws even more attacks from legislators, governors, and the media than it did at the height of student protests.

*Faculty at Work* tells us all that we possibly need to know to respond to these attacks. It also offers a treasure trove of data for students of the sociology of work, the professions, and higher education. The book reports on results of the authors' survey of administrators and faculty members, supplemented by data from several other national surveys carried out by the National Center for Education Statistics, the National Research Council, and the Carnegie Foundation for the Advancement of Teaching.

The authors' own survey of faculty, carried out in late 1987 and early 1988, picks up a decade after a major national survey of faculty conducted by Howard Bowen and Jack Schuster in *American Professors: A National Resource Imperiled* (Oxford University Press, 1987). Blackburn and Lawrence drew a random sample of over 4,000 faculty in a cross section of liberal arts disciplines, selected to represent the national distribution of faculty in nine institutional types defined by the Carnegie classification—research universities I, research universities II, doctoral universities I, doctoral universities II, comprehensive universities and colleges I, comprehensive universities and colleges II, liberal arts colleges I, liberal arts colleges II, and community colleges (*A Classification of Institutions of Higher Education* [Carnegie Foundation for the Advancement of Teaching, 1987]). In addition, the book presents a more limited set of results from a survey of 500 academic-area administrators carried out in 1988 in 103 colleges and universities.

The book focuses primarily on understanding what motivates faculty to carry out research, teach, and engage in service. Blackburn and Lawrence present a causal model consisting of environmental, sociodemographic, situational, and individual variables to account for faculty

involvement in these different aspects of academic work. They test the model against their data and achieve quite impressive predictive power, especially for publication rates, where they explain 50% of the variance. Overall, the most important variables that explain faculty productivity are *self-knowledge* (self-judged competence and preferred amount of effort to engage in research, teaching, and service) and *social knowledge* (perceived institutional preferences for time allocation for research, teaching, and service).

Whether or not their causal model will stand up to close scrutiny with other samples, carried out in different times, Blackburn and Lawrence present a rich picture of the professoriate at the turn of the century. The 550,000 full-time faculty and nearly equal number of part-timers demonstrate a commitment to their work that is rarely visible to the public. Faculty work considerably more than a 40- or even a 50-hour work week. They take their teaching duties very seriously and spend many hours engaged in teaching. Research and publication are strong values among the professoriate, even outside the research universities where research is unambivalently encouraged and supported by low teaching loads and resources. Service, while less valued, also claims a substantial amount of the faculty's time.

There are, of course, enormous variations within the professoriate on these matters and others. Women and men differ consistently both in their placement in the hierarchies of higher education and in their motivations and commitments in the workplace. The disciplines are another source of differentiation. Institutional type is probably the most prominent basis for differences in the professoriate in terms of workload, resources, motivation, and, of course, prestige.

For all of these fault lines, perhaps the greatest divides faculty members and administrators. Blackburn and Lawrence demonstrate, again and again, that faculty and administrators inhabit sharply different worlds. On every question they report and for all types of institutions, administrators see faculty work life as better than the faculty does. What makes for a valued faculty member? Administrators, much more than faculty, say that valued faculty members deal effectively with students, believe in the academic work ethic, are understanding, open, candid, dedicated, and personable. Administrators see faculty as much more influential and trustworthy than faculty see themselves. Only on one question—that some units get more than a fair share of institutional resources than others—do faculty agree more than administrators.

Blackburn and Lawrence take great pains to point out some of the practical implications of their findings. Readers, especially faculty, would do well to ponder these implications and apply them to their own situations. The specialist reader will find lots of challenging data. It would have been useful, in a book that is close to a reference, if the index were more complete and the authors' methodological descriptions more systematic.

*Witnessing for Sociology: Sociologists in Court.* Edited by Pamela J. Jenkins and Steve Kroll-Smith. Westport, Conn.: Praeger, 1996. Pp. ix+272. \$65.00.

Joan McCord  
Temple University

*Witnessing for Sociology* is likely to have something of interest to a broad range of people. The first section contains the weakest chapters of the book. The authors attribute failures to the system, although on the evidence presented the experts themselves seem to have contributed to their lack of success. These three chapters portray a tension between law and sociology that need not be present. As the remaining sections of the book indicate, law relies on expectations, often rests upon community standards for interpretation, and turns to the notion of a "reasonable man" to inquire about what ought to have been known.

The second section includes three chapters linking individual instances to contextual perspectives. Craig J. Forsyth reports how he uses criminological theory to convince the jury to "spare the life of the murderer." Patricia G. Steinhoff, an expert in Japanese culture, demonstrates how nonverbal cues influence judgments about whether a crime has been committed. Although her chapter is about a particular culture, some of the points she makes are relevant to understanding behavior among subgroups more generally. Pamela J. Jenkins describes several perspectives on battering relationships and suggests that stereotypes built on these perspectives must be countered in particular cases. Describing her strategy for establishing a defense for a woman accused of murdering her intimate partner, she noted: "We do not attempt to prove that she suffers from battered woman's syndrome, only that she had reason to fear great bodily harm" (p. 101).

Taking a more sociological view of the battered woman syndrome, Lee H. Bowker argues that a sociological perspective is uniquely suitable for defending battered women against charges of incompetence. The battered woman syndrome, interpreted purely psychologically, implies an effect that lasts beyond her relationship with the batterer. This may have untoward consequences for child custody. In addition to introducing six "systems" of abuse—use of cultural symbols such as "bitch" and "whore," social isolation, economic deprivation, psychological abuse, physical abuse, and sexual abuse—Bowker gives advice about how to convince judges, attorneys, and jurors regarding relevant evidence.

Sociological perspectives get another boost from Barry Kinsey. His chapter criticizes widely accepted views about alcoholics and substance abusers. Kinsey's attacks on the disease model of intoxication and drug abuse invoke questions of volition that have wide implications for voluntary actions in general and criminal behavior in particular. Descriptions of the cases in which he has served as an expert show some of the ramifications of taking a social perspective on what are commonly considered to be an individual's problems.

In one of the most stimulating chapters, James T. Richardson criticizes the notion that brainwashing can occur in the absence of physical force. His discussion focuses on the *Molko/Leal* suit (1986) against the Unification Church, but in the process, he raises important questions about expert opinions, politically sensitive decisions by professional organizations, and potential damage to science should dissent be silenced through civil suits.

Another interesting chapter discusses foreseeability. Lydia Voigt and William E. Thornton, Jr., describe the types of information applicable to a judgment that a crime should have been prevented. In another, Philip Holley considers the role of sociologists in determining violation of community standards (established by *Miller v. California*, 1973, as necessary grounds for obscenity claims).

The book ends with two chapters reminding readers that judgments in the courts may not be rational in ways that are portrayed in the classroom. Hilary Silver, Steven Fischback, and Judith Kaye report a victory in the courts through settlement of a suit against the city to restore funds to shelter homeless people that was a hollow victory because the consent decree had not been honored. Kai Erikson reminds readers that honest choices may not reflect cost-benefit analyses.

*Witnessing for Sociology* provides a potpourri of information about the ways sociologists can actively be involved in court procedures. These range from statistical analyses for civil discrimination suits to testifying about criminals charged with capital crimes. Like many other agglomerations, this one mixes valuable information with useless stuff. Part of the problem, it seems to me, comes from the editors' view, mistaking J. Rawls on the point that justice involves treating everyone alike. Rawls (*A Theory of Justice* [Harvard University Press, 1971]) refers to a "veil of ignorance" in order to preserve both justice and difference and not to claim that all things are equally deserving. Some of the articles should have been enhanced by reference to the work of others, some could have used translations from professional jargon, and some required more careful editing.

The book is written in a confessional style that presumably makes for easy reading but sometimes leads to doubts regarding whether the authors' experiences are worth recording and whether the success or failure in specific cases had more to do with individual style than with the merits of cases reported. Pamela J. Jenkins and Steve Kroll-Smith are to be thanked, however, for bringing together a wealth of ideas about how sociologists can function professionally outside the academy.

*The Psychiatric Team and the Social Definition of Schizophrenia: An Anthropological Study of Person and Illness.* By Rob Barrett. New York: Cambridge University Press, 1996. Pp. xvi+237. \$69.95.

Sue E. Estroff  
*University of North Carolina, Chapel Hill*

In 1978, Erving Goffman was the discussant for a session on psychiatric ethnography at the American Anthropological Association meetings. He opened his remarks by declaring that, since he had written *Asylums*, "everything written in this area is essentially a footnote." Taken aback at the time by this none too humble assessment, I have since come almost to agree with Goffman. Rob Barrett's meticulous ethnography of an Australian psychiatric hospital in the late 1980s illustrates how very little has changed in psychiatric hospitals, in hospital ethnography, and in social constructivist views of schizophrenia since 1968.

Barrett's purpose, in this anthropology dissertation-cum-book, is to locate schizophrenia in the clinical setting in which it is "experienced, diagnosed, and treated" (p. 1). As a staff psychiatrist of six years at Ridgehaven Hospital, the pseudonym for the site, Barrett is arguably either uniquely situated to study a social organization that produces schizophrenia or especially disqualified by his role as one of the producers. The text does not settle this matter because there are some penetrating insights into the layered world of the hospital and its work that perhaps only a clinician could know alongside multiple missed reflections of the author's complicity in obliterating experience and personhood for many patients and an absence of unapologetic critique of what is happening in the work of the hospital. This is especially apparent in the opacity and nearly lifeless way in which patients are presented in the book and in the frank omission of staff conflicts from the analysis because the author is concerned "lest the antagonists be identified" (p. 91). Barrett's abbreviated discussion of his roles and some of the tensions are confined to methods—little of the intellectual and professional ferment that such a study should generate is evident in the book.

Barrett's analysis of the three primary professions at Ridgehaven—psychiatry, nursing, and social work—is clear and accessible. He argues that in spatial terms, psychiatry claims the inner person, nursing deploys itself to the surface of the person, and social work inhabits context and surroundings. In this way, turf wars are avoided and the person/patient can be possessed happily by three differing claimants. His narrative of these three groups of workers in the manufacture of schizophrenia is hauntingly similar to what we would find in almost any psychiatric unit and hospital in the United States. Some of the words used in the jokes differ, but the jokes themselves do not. The setting and its procedures, history, architecture, and staff are exhaustively chronicled in the book. Due either to writing style, unfortunate editing, or dissertation sequelae, these descriptions are punctuated repeatedly with footnotes that refer to



relevant conceptual and theoretical arguments or the occasional anthropological precedent. These seem extraneous and injected uneasily into the flow of the story.

Barrett's strongest and most engaging contribution is his analysis of the writing and talking of schizophrenia—the making of “the case.” This is fresh ground, and it is traversed with intellectual power and grace of prose. In his analysis of documenting the case, Barrett's ethnographic work pays off as we can compare the spoken word of the interview with the authored text of the medical record. The characters in this section are vivid and presented with richness. With the depth of insight only a clinician might muster, the author gives a nuanced tour of how the case record becomes a repository and reproducer of the schizophrenic identity of each patient. From speech to text, from text to treatment, from treatment to identity transformation (recovery or chronicity), the writing of schizophrenia proceeds in the hospital. The chart as an unauthorized biography that overtakes the autobiography of patients eludes the author, however, as he makes only brief mention of how the object of the narrative might respond to such a representation.

The author's observations about time and space and case time and space are helpful and insightful, as is his surmise that their admiration of suffering, work, and progress underlie the moral evaluations of patients by staff. The multiple dualities of the spoken record and the written record, the moral trajectory and the clinical course, the fragmented and whole dimensions of the person—all these polarities are persuasively conveyed.

This latest contribution to the ethnography and organizational analysis of psychiatric institutions is a rich source of detail about the workings of a modern public asylum. Barrett's analyses are likely to be most effective with clinical audiences or for students in introductory medical sociology classes. Barrett makes a fine point about the richness and spontaneity of spoken presentations of patients in contrast to the written record that portrays the author (clinician) as “rational” and with emotions only of empathy and care (p. 109). Similarly, this book may leave the critical social scientist searching for the spoken text that was somehow made too clean and too professionally coded by this book as a case record.

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# Why Is Classical Theory Classical?<sup>1</sup>

R. W. Connell

*University of Sydney*

The familiar canon embodies an untenable foundation story of great men theorizing European modernity. Sociology actually emerged from a broad cultural dynamic in which tensions of liberalism and empire were central. Global expansion and colonization gave sociology its main conceptual framework and much of its data, key problems, and methods. After early-20th-century crisis, a profoundly reconstructed American discipline emerged, centered on difference and disorder within the metropole. The retrospective creation of a "classical" canon solved certain cultural dilemmas for this enterprise and generated a discipline-defining pedagogy, at the price of narrowing sociology's intellectual scope and concealing much of its history.

## FOUNDATION STORIES

In any recent introductory sociology textbook, whether written in the United States (Calhoun, Light, and Keller 1994) or on the other side of the world (Waters and Crook 1993), there will almost certainly be, in the first few pages, a discussion of founding fathers, focused on Karl Marx, Émile Durkheim, and Max Weber. The introductory chapter will probably, though not certainly, also mention Auguste Comte, Herbert Spencer, and Georg Simmel. It will sometimes mention members of a second team: Ferdinand Tönnies, Friedrich Engels, Vilfredo Pareto, William Graham Sumner, Charles H. Cooley, and G. H. Mead.

In the view normally presented to undergraduates, these founding fathers created sociology in response to dramatic changes in European society: the industrial revolution, class conflict, secularization, alienation, and the modern state. "It was above all *a science of the new industrial society*"

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(Bottomore [1962] 1987, p. 7). This view is repeated in more sophisticated form in advanced training. American graduate programs in sociology normally have courses (often required courses) in which students are introduced to "classical theory" and make a close study of certain texts written by Marx, Durkheim, and Weber.

This curriculum, in turn, is backed by a disciplinary history in which sociologists account for their origins in virtually the same terms. For instance Swingewood's *Short History of Sociological Thought* (1991), a reliable and well-regarded British text, presents a three-part narrative: "Foundations" (leading up to Comte and Marx), "Classical Sociology" (centering on Durkheim, Weber, and Marx), and "Modern Sociology." Similarly, according to Münch (1994, 1:ix) in Germany, the "classical period . . . was the time when the founding fathers set the framework of sociological theory."

Sociologists take this account of their origins seriously. The classical texts are widely cited, and it is not uncommon for research papers, monographs, or treatises to identify themselves as having a Marxist, Durkheimian, or Weberian approach (e.g., Wallace 1994). A star-studded review of the field, *Social Theory Today*, began with a ringing declaration of "the centrality of the classics" (Alexander 1987). In the 1990s, commentary on classical texts remains a major genre of theory, even for those who think the process of interpretation should have an end (Turner 1993). To others, the genre is open-ended and can acquire new layers. One can go back in time and read Durkheim through the lens of Aristotle (Challenger 1994) or come forward and read Durkheim through the lens of post-post-structuralism (Lehmann 1993). Unfamiliar texts of Weber can be read (Drysdale 1996), or familiar ones reread (Kalberg 1996).

It is not just through direct commentary that the classical texts affect the discipline. As symbols of "what is most distinctively sociological" (Sugarman 1968, p. 84), they influence what kind of discussion counts as sociological theory, what theoretical language sociologists are to speak in, and what problems are most worth speaking about.

It is now well recognized (Seidman 1994) that the idea of classical theory embodies a "canon," in the sense used by literary theory (Guillory 1995): a privileged set of texts, whose interpretation and reinterpretation defines a field. Critique of the canon has begun, and a search is under way for improvements. The journal *Sociological Theory* has been running a series, Neglected Theorists. Undergraduate textbooks are beginning a kind of affirmative action, adding W. E. B. Du Bois (Macionis 1993) or Jane Addams (Thio 1992) to the familiar list of founders.

These responses are welcome as a modest broadening in ideas of the past. They do little to change the way of *using* the past that is embedded in the concept of "classical theory" and the pedagogy of classical texts.

Nor have they altered sociologists' general view of the structure of the discipline's history: a foundational moment arising from the internal transformation of European society; discipline-defining texts written by a small group of brilliant authors; a direct line of descent from them to us.

There are two troubling anomalies, however, about this story. First, though classical theory has great prestige in principle, much of sociological research ignores it in practice. The bulk of quantitative sociology, as well as most ethnographic and life-history research, proceeds without reference to canonical theory or the problems it defines. The gap has led some commentators (e.g., Chafetz 1993) to argue in vehement terms that the classical canon is now irrelevant and others to argue that it performs mainly a symbolic function, providing "rituals of solidarity" for the profession of sociology (Stinchcombe 1982).

Second, sociologists in the "classical period" themselves did not believe this origin story. When Franklin H. Giddings (1896), the first professor of sociology at Columbia University, published *The Principles of Sociology*, he named as the founding father—Adam Smith. Victor Branford (1904), expounding "the founders of sociology" to a meeting in London, treated as the central figure—Condorcet. Durkheim's and Weber's academic contemporaries did not see them as giants and often disregarded Marx.

Turn-of-the-century sociology had no list of classics in the modern sense. Writers expounding the new science would commonly refer to Comte as the inventor of the term, to Charles Darwin as the key figure in the theory of evolution, and then to any of a wide range of figures in the intellectual landscape of evolutionary speculation.

Witness the account of the discipline in the second edition of Lester F. Ward's *Dynamic Sociology*. At the time of the first edition in 1883, Ward observed, the term "sociology" had not been in popular use. But in the intervening decade a series of brilliant scientific contributions had established sociology as a popular concept. There were now research journals, university courses, and societies, and sociology "bids fair to become the leading science of the twentieth century, as biology has been that of the nineteenth" (Ward 1897, p. viii). Ward listed 37 notable contributors to the new science. The list included Durkheim and Tönnies but not Marx (nor Weber, who had published little that was recognizable as "sociology" at this date, when the discipline was already institutionalized and international).

The list of notables became a common feature in the textbooks of sociology that multiplied in the United States from the 1890s, Giddings's *Principles* being one of the first. (Ward had included Giddings in his list, and Giddings politely included Ward in his.) The famous "Green Bible" of the Chicago school, Robert E. Park and Ernest W. Burgess's ([1921] 1924,

p. 57) *Introduction to the Science of Sociology*, listed 23 “representative works in systematic sociology.” Simmel and Durkheim were among them but not Marx, Weber, or Pareto. Only one work by Weber was mentioned in this 1,000-page volume, only in the notes, and Weber was given less attention than Walker, Wallace, Wheeler, Wittenmyer, Woods, or Worms. Clarence M. Case’s *Outlines of Introductory Sociology* (1924) brushed aside Marx, mentioned Durkheim three times (disparagingly), completely ignored Weber, and included—in what was also a 1,000-page reader in sociology—no text from any of the three.

As late as the 1920s, then, there was no sense that certain texts were discipline-defining “classics” demanding special study. Rather, there was a sense of a broad, almost impersonal, advance of scientific knowledge with the notables being simply leading members of the pioneering crew. Sociologists accepted the view, articulated early in the history of the discipline by Charles Letourneau (1881, p. vi), that “the commencement of any science, however simple, is always a collective work. It requires the constant labour of many patient workmen.”

This suggests an encyclopedic, rather than a canonical, view of the new science by its practitioners. Encyclopedism was certainly the note struck by interwar academic commentators other than Talcott Parsons. Pitirim A. Sorokin, expounding European thought to North Americans, took a broad view of who should be included in *Contemporary Sociological Theories* (1928). Pareto, Durkheim, Marx, and Weber were there, along with Le Play, Bruhnes, Galton, Coste, Gumpłowicz, Petrajitzsky, and many more. Even more sweeping was the view from Howard Becker and Harry E. Barnes’s *Social Thought from Lore to Science* (1938). The members of the modern canon were certainly here—along with literally hundreds of other theorists and researchers, carefully abstracted and classified by nationality, date, and school.

It was only in the following generation that the idea of a classical period and the short list of classical authors and canonical texts took hold. The process is strikingly documented in Platt’s (1995) important study of the American reception of Durkheim’s *Rules of Sociological Method*. Platt shows, by a detailed examination of both textbook references and specialist studies, that Durkheim had only a modest presence in early American sociological thought. He was a known contemporary, but the *Rules* served mainly as a punching bag for arguments about the importance of the individual. The proportion of introductory texts that discussed Durkheim fell after his death to a low point in the 1930s. It then rose spectacularly, reaching nearly 90% in the 1960s. The stunning reversal of Durkheim’s reputation began in the late 1930s and consolidated in the 1950s. The *Rules* became a must-read classic, for the first time, half a century after it was published.

Detailed studies of this kind immediately confront us with the larger question, Why did a canon come into existence at all? Why does sociology now have the configuration of constructed memories known as "classical theory"? Psychoanalysis aside, sociology is the only one of the social sciences to take such an intense interest in the writings of a small group of putative founding fathers. Political scientists remember Niccolò Machiavelli and James Bryce, psychologists sometimes discuss Wilhelm Wundt and Sigmund Freud, educationists remember John Amos Comenius and often read John Dewey, some economists still read Adam Smith and David Ricardo, and historians honor—though they do not often read—Jules Michelet and Leopold von Ranke. None of these disciplines, however, shares sociology's identity-defining focus on a small group of classical theorists nor its pedagogy of canonical texts.

To understand why this is so and what the classical canon means for modern sociology, it is not sufficient to deconstruct the canon and show its artifactual character. Nor is it sufficient to trace the waxing and waning "influence" of particular theorists, though these are useful steps along the way. The question concerns the structure of sociological thought as a whole. Accordingly we must examine the history of sociology as a collective product—the shared concerns, assumptions, and practices making up the discipline at various times—and the shape given to that history by the changing social forces that constructed the discipline.

#### SOCIOLOGY, GLOBAL DIFFERENCE, AND EMPIRE

Sociology as a teaching discipline and a discourse in the public arena was constructed at a particular time and place: the final two decades of the 19th century and the first decade of the 20th century, in the great cities and university towns of France, the United States, Britain, Germany, and, a little later, Russia.

The conventional foundation story interprets these places as the locale of a process of modernization, or capitalist industrialization, internal to European and North American society. In this view—well illustrated by Nisbet's (1967) formula of "the two revolutions," industrial and democratic, and by Giddens's (1989) formula of industrialization and urbanism—sociology arose from the experience of modernity. It was an attempt to interpret the new society being brought into being. Even the revisionist historiography of Therborn (1976), Seidman (1983), Lepenies (1988), and Ross (1991) maintains this internalist focus.

The main difficulty with this view is that it does not square with the most relevant evidence—the texts that sociologists at the time were writing and reading. This evidence is unfamiliar because of the canon itself. Most sociologists today do not read early sociological texts *except* the ca-

nonical ones. However surprising it may now seem, most general treatises or textbooks of sociology, up to the First World War, did not have a great deal to say about the modernization or the contemporary structure of the society the authors lived in. Their main interest was elsewhere. Giddings's *Readings in Descriptive and Historical Sociology* (1906), typical in this respect, ranged from polyandry in Ceylon via matrilineal survivals among the Tartars to the mining camps of California. It was so little focused on modernity that it took as its reading on "sovereignty" a medieval rendering of the legend of King Arthur.

The evidence of college textbooks needs not correspond to the research focus of sociology, but on this topic too, we have abundant evidence, collected by no less a figure than Durkheim. Between 1898 and 1913, Durkheim and his hardworking collaborators produced 12 issues of *L'Année sociologique*, an extraordinarily detailed international survey of each year's publications in or relevant to sociology. In these issues, nearly 2,400 reviews were published.<sup>2</sup> Modern industrial society was certainly included: the journal published reviews on the American worker, the European middle class, technology in German industries, books by Beatrice and Sidney Webb, by Werner Sombart, by Charles Booth on London poverty, even a work by J. Ramsay MacDonald, later Labour prime minister of Britain. But works focused on the recent or contemporary societies of Europe and North America made up only a minority of the content of *L'Année sociologique*—about 28% of the reviews. Even fewer were focused on "the new industrial society," since the reviews on Europe included treatises on peasant folktales, witchcraft in Scotland, crime in Asturias, and the measurements of skulls.

The bulk of the research material of sociology, as Durkheim and his colleagues documented it, did not concern modernity. Twice as many of the reviews concerned ancient and medieval societies, colonial or remote societies, or global surveys of human history. Studies of holy war in ancient Israel, Malay magic, Buddhist India, technical points of Roman law, medieval vengeance, Aboriginal kinship in central Australia, and the legal systems of primitive societies were more characteristic of "sociology" as seen in *L'Année sociologique* than studies of new technology or bureaucracy. The pattern is exactly that of textbooks like Giddings's (1906).

The enormous spectrum of human history that the sociologists took as their domain was organized by a central idea: difference between the

<sup>2</sup> I have counted only reviews in large type, whatever their length, not the brief notices in small type in the early issues nor the listings of titles without reviews. The reviews concerning Western/Northern Europe and modern North America increase with time: they average 24% of all reviews in the first six issues, 28% in the next five issues, and 32% in the bumper issue in the year before the war.



civilization of the metropole and an Other whose main feature was its primitiveness. I will call this the idea of "global difference." Presented in many different forms, this contrast pervades the sociology of the late 19th and early 20th centuries. Together with the idea of "progress" from the primitive to the advanced (discussed below), it is both the key assumption of sociological research and the major object of sociological theory.

The idea of global difference was often conveyed by a discussion of "origins." In this genre of writing, sociologists would posit an original state of society (or some aspect of society, such as law, morality, or marriage), then speculate on the process of evolution that must have led forward from there. The bulk of the three volumes of Spencer's *Principles of Sociology* ([1874–77] 1893–96), was spent in telling such a story for every type of institution that Spencer could identify: domestic institutions, political institutions, ecclesiastical institutions, and so forth. Spencer proceeded as if the proof of social evolution was not complete without an evolutionary narrative, from origins to the contemporary form, for each and every case. (This was a much more important feature of his theorizing than the "society as organism" analogy for which he is now mostly remembered.)

The formula of development from a primitive origin to an advanced form was widespread in Victorian biological and social thought (Burrow 1966). Sociologists simply applied a logic that their audience would take for granted. The same architecture is found in works as well known as Durkheim's *Division of Labor in Society* ([1893] 1964) and as obscure as Fairbanks's *Introduction to Sociology* ([1896] 1901).

In none of these works was the idea of an "origin" taken as a concrete historical question. It could have been. Knowledge of early societies was growing dramatically in these decades. Troy, Mycenae, and Knossos were excavated, Flinders Petrie systematized the archaeology of Egypt, the first evidence of Sumerian culture was uncovered at Lagash and Nippur (Stiebing 1993). But sociology was not interested in where and when a particular originating event occurred. This insouciance was not confined to origins. Treatise after treatise offered surveys of social evolution or contrasts of primitive and advanced institutions, with only the slightest reference to when the major changes actually happened. Time functioned in sociological thought mainly as a sign of global difference. What was early could be taken to be Other.

Durkheim did not have to find a precise time in the past for "segmentary societies"; they existed in his own day. In a move characteristic of sociology at the time, Durkheim used the example of the Kabyle of Algeria as well as the ancient Hebrews and made no conceptual distinction between the cases. He knew about the Hebrews because the ancient texts were in his library. How did he know about Kabylia? Because the French had conquered Algeria earlier in the century, and at the time Durkheim wrote,

the French colonists were engaged in evicting the local population from the best land (Bennoune 1988). Given the recent history of violent conquest, peasant rebellion, and debate over colonization, no French intellectual could fail to know something about the Kabyle. For the curious, the social life of France's North African subjects was being documented in great detail by a series of private and official inquiries (Burke 1980).

This was far from being the only French adventure into the world of the primitive Other. In the dozen years before *Division of Labor* was published, the armies of the French republic had moved out from Algeria to conquer Tunisia, had fought a war in Indochina, conquered Annam and Tonkin (modern Vietnam), and seized control of Laos and Cambodia, and had established a protectorate over Madagascar. Under the Berlin Treaty of 1885, French trading posts in central and western Africa became the basis of a whole new empire. As Durkheim was writing and publishing the *Division of Labor* and *The Rules of Sociological Method*, French colonial armies were engaged in a spectacular series of campaigns against the Muslim regimes of inland north and west Africa, which produced vast conquests from the Atlantic almost to the Nile.

Impressive as this growth was, it was only part of a larger process. The British Empire, also a maritime empire with a preindustrial history, similarly gained a new dynamism and grew to vast size in the 19th century (Cain and Hopkins 1993). Imperial expansion went overland as well as overseas. The 13-colony United States became one of the most dynamic imperial powers throughout the 19th century, with about 80 years of overland conquest and settlement (the "westward expansion"), followed by a shorter period of overseas conquest. The tsarist overland conquests, begun in earlier centuries, were extended in the Far East and Central Asia. In the later part of the 19th century, they were consolidated by Russian settlement. Prussia's expansion as an imperial power began with conquest within Europe—in the process, setting up a relationship between dominant and conquered races in the East that became the subject of the young Weber's first sociological research (Weber [1894] 1989). German overseas colonies in Africa and the Pacific followed the formation of the Reich in 1871. By the time the global system of rival empires reached its violent crisis in the Great War of 1914–18, a world-historical change had been accomplished—the creation of a world-embracing social order.

In this light, the time and place where the discipline of sociology was created take a new significance. The locales were the urban and cultural centers of the major imperial powers at the high tide of modern imperialism. They were the "metropole," in the useful French term, to the larger colonial world. The intellectuals who created sociology were very much aware of this. One of the major tasks of sociological research—and of *L'Année sociologique* as a record of sociological knowledge—was to

gather up the information yielded by the colonizing powers' encounter with the colonized world. The idea of global difference was not invented by European and North American intellectuals. It was given to them by the process of economic and colonial expansion, inscribed in the social structure of the empires that the North Atlantic powers constructed.

The birth of sociology cannot be understood by internalist models, then, because it crucially involves the structure of *world society*. It can only be understood in geopolitical terms.

Since Kiernan's ([1969] 1986) remarkable survey, *The Lords of Human Kind*, historians have begun to grasp the immense impact that the global expansion of North Atlantic power had on popular culture (MacDonald 1994) and intellectual life (Asad 1973; Said 1993) in the metropole, as well as in the colonies. It would be astonishing if the new science of society had escaped the impact of the greatest social change in the world at the time. In fact, the relationship was intimate. Sociology was formed within the culture of imperialism and embodied a cultural response to the colonized world. This fact is crucial in understanding the content and method of sociology as well as the discipline's cultural significance.

## THE CONTENT AND METHOD OF SOCIOLOGY

### The Concept of Progress

As the civilized Todd (1918, p. vii) remarked (the first, and perhaps still the only, professor of sociology to introduce Japanese cherry-blossom paintings into a discussion of social theory): "From Comte onward sociologists have pretty generally agreed that the only justification for a Science of Society is its contributions to a workable theory of progress." At the inaugural meeting of the American Sociological Society, Ward's (1907) presidential address announced that sociology was a science that "has gone farther even than physics," since the basic law of social evolution had been discovered along with its explanation. As Comte had argued, sociology itself embodied intellectual progress, its place "definitely fixed" (Ward 1907) at the summit of the hierarchy of sciences.

John Stuart Mill ([1843] 1891, p. 596), the sharpest mind among all whose idea of social science was shaped by Comte, had cautioned against equating historical change with improvement. Few of the sociologists took heed. Spencer's first social theory, in *Social Statics* ([1850] 1954), made moral improvement the touchstone of analysis of "the social state." Both the teaching enterprise of sociology departments (as documented in textbooks such as Giddings's *Principles* [1896] or Fairbanks's *Introduction* [1901]), and the popular expositions of the new science (such as Spencer's *Study of Sociology* [(1873) 1887] or Benjamin Kidd's *Social Evolution* [1898]), centered on the moral, intellectual, and material improvement of

society. Discovering and expounding laws of progress was the core of what sociology *meant* in those two generations.

There was, however, a shift of meaning within the concept of progress. In Comte's writings, the idea of progress, while extremely generalized, mostly had to do with the ancient-medieval-modern sequence within Western culture that was the common property of European intellectuals, worked up in different forms by Hegel and Marx. (In some passages in Comte's *Système de politique positive* [(1851–54) 1875–77] Western Europe is clearly also the locale for the transition to sociolatriy, the religion of the final stage of human progress.) Sociological critiques of Comte in the next generation rejected the arbitrariness of his system and demanded an empirical base for the concept of progress.

This was the common ground between Spencer and Letourneau, and it is a fact of the greatest significance that both turned to the ethnographical dividend of empire as their main source of sociological data. Spencer's *Principles of Sociology* documented its evolutionary stories from the writings of European travelers, missionaries, settlers, and colonial officials, as well as historians. Spencer's reference list for "Political Institutions," for instance, ranged from the journals of the American explorers, Lewis and Clark, through the *Journal of the Asiatic Society of Bengal* and *Thirty-Three Years in Tasmania and Victoria*, to that riveting work, *A Phrenologist among the Todas*. Letourneau's *Sociology, Based upon Ethnography*, while setting the facts out in a finer grid, was very similar in its sources. For instance, discussing the condition of women, Letourneau (1881, pp. 182–83), in two pages, cites social facts from China, Cochin China (Vietnam), Burma, India, Afghanistan, Africa, ancient Rome, and Arabia—apart from the last two, a roll call of British and French colonial expansion in the 19th century.

By the time sociology was being institutionalized in the final decade of the 19th century, the central proof of progress, and therefore the main intellectual ground on which the discipline rested, was the contrast of metropole and colonial Other. Sociologists did not debate the reality of this contrast; they debated how it should be interpreted—whether through physical evolution from lower to higher human types or through an evolution of mind and social forms and whether competition or cooperation was the motor of progress. In this context, Durkheim's *Division of Labor* was no founding text. It was a late intervention in a long-running debate. Durkheim quarreled with Comte and Spencer about how to explain the evolution of society, and he drastically reformulated the relation between social change and morality. Yet he did so by offering new "laws" within the same problematic, not by establishing a new framework.

The concern with progress was not a "value" separable from the science; it was constitutive of sociological knowledge. The arguments of Ward,

Du Bois, L. T. Hobhouse, Durkheim, Spencer, and Comte himself are absurd if one does not presuppose the *reality* of progress. It was as an account of progress that "sociology" spread to colonized peoples and others beyond the metropole. Spencer's sociology, for instance, was being debated in India well before the turn of the century (Chatterjee 1986) and, in translation, became a significant influence on the intellectuals of Meiji Japan (Tominaga 1994) and the Chinese republican movement (Grieder 1981). Even Du Bois ([1904] 1978, p. 54) presented his research program on the American Negro community within the framework of progress, as a study of "the evolution of a vast group of men from simpler primitive conditions to higher more complex civilization."

### Race and Gender

The topics addressed by the new discipline also reveal its underlying connection with empire. Internalist accounts of classical sociology emphasize class, alienation, and industrialization and pay little attention to such topics as race, gender, and sexuality. "Race," for instance, was not one of the "unit-ideas of sociology" recognized by Nisbet (1967) and does not even appear in the index of Swingewood's *Short History* or Münch's *Sociological Theory*. But a social science based on the social relations of empire must deal with race, and a social science concerned with evolutionary progress and hierarchies of populations must deal with gender and sexuality.

It is, then, an important fact that race, gender, and sexuality *were* core issues in sociology. When Du Bois ([1950] 1978, p. 281) proposed in 1901 that the color line was "the problem of the twentieth century," he was saying nothing unusual for the time. The first "sociology" published in the United States, in the 1850s, was a defense of slave society in the South (Ross 1991, p. 32). After slavery was abolished, the wider colonial world remained as the object of sociological knowledge and was persistently interpreted in terms of race. Letourneau's (1881, pp. 5–6) "ethnography" meant a science of racial differences, and his *Sociology* opened with an enumeration of the human races: the black man, "whose brain is small"; the yellow man, whose brain "is better shaped"; and the white man, who has "ascended a few degrees higher in the organic hierarchy" and whose "brain is developed."

Spencer did not go so far but still displayed a genial contempt for the "primitive" races. Ward (1897) was confident that global race conflict reflected the superiority of the European races and that universal progress was dependent on their universal triumph. The extermination of other races was possible; at best, they could join the march of progress on the Europeans' terms. The Canadian author of *Sociology Applied to Practical*

*Politics* (Crozier 1911) was blunter. To him, sociology proved that race mixing was a "crime" that would lead to the degeneracy of the white races' culture. The solution was political: form a party of evolution and prevent miscegenation.

Here sociology reflected, in the most direct way, the social relations of imperialism. Racial hierarchies were the characteristic outcome of North Atlantic world power, the rule over indigenous peoples, and the creation of plantation economies and colonies of settlement. This is not to say that all sociologists were racists, though some certainly were. Others, Du Bois and Durkheim among them, suffered from racism. The point is, rather, that racial hierarchy on a world scale was a perception built into the concept of "progress" and a central part of what sociology was thought to be about: "It is evident that, despite the greater consideration now shown for the rights of the lower races, there can be no question as to the absolute ascendancy in the world today of the Western peoples and of Western civilisation" (Kidd 1898, p. 316).

Nor was there any question about the importance of gender and sexuality. Comte in his *Système* gave considerable prominence to the position of women, and his famous conflict with Mill included sharp differences over the subjection of women. When Spencer came to write the substantive part of *The Principles of Sociology*, the very first set of institutions he addressed was the "domestic." By this he meant what we now call "gender" issues: kinship, family, and the status of women. Letourneau treated marriage and the family before property, and at greater length. He dealt with sexuality ("the genesic need") near the start of *Sociology*, with an impressive absence of Victorian delicacy—menstruation, infanticide, prostitution, promiscuity, and sodomy were all on the agenda.

In the next generation, Ward devoted the longest chapter of *Pure Sociology* (1903) to developing a sociology of sex and gender, under the rubric of the "Phylogenetic Forces." The longest coherent argument in *Gemeinschaft und Gesellschaft* (Tönnies [1887] 1955, pp. 174–91) was devoted to gender difference. Much of Sumner's *Folkways* ([1906] 1934) was about gender arrangements and sexual customs, the more startling the better. William I. Thomas (1907) published a whole book about sex and gender, and his work on these topics was duly reviewed in *L'Année sociologique*.

Some of this interest can be explained on internalist lines. Ward was certainly influenced by American feminism. Spencer had begun as a partisan of feminism, though he rapidly retreated (Paxton 1991). *The Study of Sociology* included an essay arguing the physical and mental inferiority of women, who, because of their commitment of energy to childbearing, stopped evolving earlier than men.

The way gender and sexual issues were taken up in sociology, however, was very much affected by evolutionary concerns and the issues of empire.

In the imperial context, racial and sexual issues were not separate. The interplay between them was most powerful in the colonies, and its vehemence is hard to grasp for those unfamiliar with the history of empire. In the later 19th century, the expansion of the North Atlantic powers was accompanied by a growing fear of miscegenation, a hardening color line, contempt of the colonizers for the sexuality or masculinity of the colonized (Sinha 1995), and fears of racial swamping that gave rise to panics about the "yellow peril," pogroms against the Chinese in the United States and Australia, and racist immigration policies. Echoes are heard even in the most abstract texts from the metropole. Giddings (1896, p. xiii), expounding his theme of "consciousness of kind," remarked that "living creatures do not commonly mate with individuals of other than their own species," and his first example was "white men do not usually marry black women."

These concerns underlie the discipline's interest in eugenics. This pseudoscience of controlled evolution by selective breeding of humans was then regarded as a reputable and even important part of sociology. Francis Galton, the leading advocate of eugenics, was carefully evaluated by Durkheim in *The Division of Labor*. Galton's (1904) work was published in the *American Journal of Sociology*, noticed in *L'Année sociologique*, and, in due course, reprinted in Park and Burgess's (1924) famous textbook.

### Method: The Imperial Gaze and Grand Ethnography

The most striking feature of sociological method was its bold abstraction. Comte offered cultural "laws" of vast scope, and the inaugural meeting of the American Sociological Society, 60 years later, was still celebrating tremendous "laws" of social evolution. As far away as the colonial boom city of Melbourne, we find an evolutionary social scientist formulating broad generalizations about archaic and modern society (Hearn 1878).

Durkheim ([1895] 1964, p. 139) argued convincingly that this approach was the basis of the whole enterprise: "Comparative sociology is not a particular branch of sociology; it is sociology itself, insofar as it ceases to be purely descriptive and aspires to account for facts." The comparative method meant assembling examples of the particular social "species" under study and examining their variations. For all Durkheim's criticisms of Spencer, that was exactly what Spencer's *Principles of Sociology* had done.

Comparative method rested on a one-way flow of information, a capacity to examine a range of societies from the outside, and an ability to move freely from one society to another—features that all map the relation of colonial domination. Letourneau (1881, p. 15) expressed the sociological point of view in a striking image: "Let us imagine an observer placed

somewhere high up in air above our terrestrial equator, far enough from the globe on which we live to take in a whole hemisphere at one glance, and yet close enough to distinguish with the aid, if need be, of a magnifying-glass, the continents and the seas, the great ranges of mountains, the white frozen tops of the polar regions, etc. etc."

Sociology had limited interest in "ethnography" in the modern sense, the detailed description of a whole way of life. Witness the marginalization of Du Bois, whose *Philadelphia Negro* (1899) was perhaps the finest ethnography of the day. (The much greater attention given Chicago ethnographies from the 1920s is part of the argument below.) Rather, it was the structure of empire as a whole that provided the basis of sociological knowledge. Sociology's comparative method embodied the imperial gaze on the world.

This is seen most fully in surveys such as Spencer's *Descriptive Sociology* and the collective project of the *Année sociologique*. Perhaps the most striking was Hobhouse, Wheeler, and Ginsberg's *Material Culture and Social Institutions of the Simpler Peoples* (1915), an attempt to overcome the unsystematic use of data in theories of social evolution by providing a statistical base for comparative sociology. Hobhouse and his colleagues surveyed the whole world, collecting information on more than 500 societies. They classified societies by grade of economic development and tried to establish correlations of development with institutional patterns of law, government, family, war, and social hierarchy.

These surveys are virtually forgotten now, but the imperial gaze can also be found in more familiar texts. Sumner's *Folkways*, published in 1906, is his last major work and perhaps the most influential book written by an American in the "classical" period. It sought to "put together all that we have learned from anthropology and ethnography about primitive men and primitive society" (Sumner 1934, p. 2). The whole world and the whole of history was the field of Sumner's gaze. Take an entirely typical passage:

In Molembo a pestilence broke out soon after a Portuguese had died there. After that the natives took all possible measures not to allow any white man to die in their country. On the Nicobar islands some natives who had just begun to make pottery died. The art was given up and never again attempted. White men gave to one Bushman in a kraal a stick ornamented with buttons as a symbol of authority. The recipient died leaving the stick to his son. The son soon died. Then the Bushmen brought back the stick lest all should die. Until recently no building of incombustible materials could be built in any big town of the central province of Madagascar, on account of some ancient prejudice. A party of Eskimos met with no game. (Sumner 1934, p. 25)



Few cases delayed the author for more than two sentences. As this passage shows, for Sumner, the force of the argument did not lie in the depth of his ethnographic understanding. It was provided by the assemblage itself, the synoptic view of human affairs from a great height.

Durkheim's (1964, pp. 174–77) own use of the comparative method, for instance, in his analysis of transition from the horde to segmental societies, has been mentioned above. The imperial gaze can also be observed in Weber, in those late works where he was deliberately setting out to write sociology. In the manuscripts collected after his death and published as *Economy and Society*, Weber ([1922] 1978) had sometimes used the method of "ideal-types" with which the canon associates him. But more often he used the same procedure as Durkheim and Sumner, comparing actual social forms. The *Kategorienlehre* of part 1 set up the categories of theory in such a way as to require transhistorical content. In the studies of part 2, Weber swept across continents and civilizations in vast comparative analyses of economic forms, authority, religious groups, states, music, cities, and so forth. As Tenbruck ([1975] 1989) has suggested that the religious sociology provides whatever thematic unity there is in Weber's work, I will take an example from the chapter, "Religious Groups":

Normally, the god of a locality and his cultic association reach fullest development on the foundation of the city as a separate political association with corporate rights, independent of the court and the person of the ruler. Consequently, such a full development of the local god is not found in India, the Far East, or Iran, and occurred only in limited measure in northern Europe, in the form of the tribal god. On the other hand, outside the sphere of autonomous cities this development occurred in Egypt, as early as the stage of zoolatric religion, in the interest of apportioning districts. From the city-states, local deities spread to confederacies such as those of the Israelites, Aetollans, etc. (Weber 1978, pp. 414–15)

The obvious risk in such a sweeping use of comparative method is incoherence. The problem could be overcome with a variation of the comparative method whose dramatic quality produced some of the best-remembered "classical" texts. This approach might be called "grand ethnography," in contrast with the close-focus fieldwork of Franz Boas, Du Bois, or the French specialists on Algeria and Morocco.

Spencer pioneered it, with the broad contrast (in part 5 of *Principles of Sociology*) of "the militant type of society" oriented toward war, and "the industrial type of society" oriented toward peaceful production. But Spencer saw these two types of society existing at the same time—he feared, indeed, the tendency of empire to foster militarism in his own day. The more usual style of grand ethnography was built on the idea of global difference. It presented holistic accounts of the societies found at the origin

and the end of progress, on the understanding that "in all its leading characteristics—political, legal, religious, economic—archaic society presents a complete contrast to that in which we live" (Hearn 1878, p. 4).

The famous contrast of *gemeinschaft* and *gesellschaft* is grand ethnography in this sense, identifying polar states of society. Tönnies reached back to Henry Sumner Maine's account of Indian society in expounding *gemeinschaft*, came forward to expound the state as an association characteristic of *gesellschaft*. Durkheim's *Division of Labor* was a more rigorous grand ethnography, specifying the basis of the contrast in the division of labor and more systematically following out its ramifications at a given point in social evolution. It is not surprising that *L'Année sociologique* took a persistent interest in attempts to distinguish primitive from modern law, in German theories of *Naturvölker* and their distinction from *Kulturvölker*, and in attempts to formulate the nature of primitive religion.

Grand ethnography was the artistic climax of Comtean sociology, the literary form taken by the theory of progress as the rhetoric of the "struggle for existence" was bypassed. It replaced Spencer's shapeless vision of history with a well-defined succession of social types.

#### SOCIOLOGY IN THE POLITICAL CULTURE OF EMPIRE

The late-19th- and early-20th-century world had several groups of intellectuals grappling with the analysis of society. The mobilization of European and American workers had produced an intellectual ferment, leading to Mikhail Bakunin's analysis of religion and the state, Marx's formulation of the structure of the capitalist economy, Ferdinand Lassalle's strategy of mass organization, Fabian socialism, and Georges Sorel's revolutionary syndicalism. The mobilization of women around the franchise, married women's property rights, temperance, and women's wages and working conditions had produced another ferment. Feminist ideas were articulated by intellectuals from John Stuart Mill to Henrik Ibsen, Susan B. Anthony, the Pankhursts, Clara Zetkin, Olive Schreiner, and Marianne Weber. The claim that Harriet Martineau was the "first woman sociologist" (Hoecker-Drysdale 1992) is anachronistic, but the story of Martineau—novelist, political economist, translator of Comte, travel writer, and reformer—should alert us to the complexity of the milieu in which sociology arose.

Beyond the metropole, there were many intellectuals in these decades who looked at modernity from the standpoint of non-European cultures and at Europeans from the standpoint of the colonized. Changes in culture and social life were central issues to writers as diverse as Jamāl ad-Dīn al-Afghānī in the Islamic Middle East (Ghamari-Tabrizi 1994), Bankim-

chandra Chatterjee (1986) in Bengal, and revolutionary activists such as Sun Yat-sen ([1927] 1975) in China.

From this range of groups, a range of discourses about society emerged, of which "sociology" was only one. The anarchist Bakunin ([1873] 1973), developing critiques of Comte on one side and Marx on the other, recognized at an astonishingly early stage that a "science of society" might rationalize the interests of a particular group (an insight that launched a long debate about intellectuals and the "new class"). Following Bakunin's lead, we should consider the specific social location in which sociology developed and the cultural issues to which it was a response.

Sociology as a collective practice—that is to say, a set of institutions, a pedagogy, and a genre of writing—developed in a specific social location: among the men of the liberal bourgeoisie in the metropole. Those who wrote sociology were a mixture of engineers and doctors, academics, journalists, clerics, and a few who (like Weber after his breakdown) could live on their family capital.

This is not to say that the sociologists were, generally speaking, either rich or apologists for the rich. Ross (1991) points to the social distance between the academic makers of American sociology and the capitalist entrepreneurs of American industrialization. Weber was a fierce critic of the ruling class of the German Reich, and Durkheim was no friend of the French aristocracy. Nevertheless, they were beneficiaries of both class and gender hierarchies. Most lived modest bourgeois lives, supported by the domestic labor of women in patriarchal households. Their social interests were well captured by Comte's celebrated slogan, "Order and Progress."

Men of this sort began to discuss "the social science," as Mill called it, from the 1850s on, in a diffuse movement to apply scientific thinking to society and to promote moral improvement. The successful Association for the Promotion of Social Science was set up in London as early as 1857 (Burrow 1966), soon copied in Boston (and thus the remote ancestor of the American Sociological Association of today). The same movement produced a heavily moralized "social science" curriculum in American colleges from the 1860s (Bernard and Bernard [1943] 1965). Individual attempts to synthesize the facts of primitive life and social progress (such as Edward Tylor's *Primitive Culture* [1873]) counterpointed attempts to establish societies and institutes to formulate a science of man. The latter, in France, gave rise to the first chair to be named "sociology"—to which Letourneau was appointed in 1885 (Clark 1973).

In the 1890s "social science" curricula in American colleges, already fissuring, began to be replaced by more self-consciously scientific courses called "sociology." Their claim to scientificity was closely connected with the shift to comparative method and imperial gaze discussed above. Hence

the world-spanning content of the first generation of sociology textbooks. Named sociology departments were established, undergraduate courses multiplied, and a market for textbooks rapidly developed (Morgan 1983). This was the historical moment registered in Ward's discussion of the foundation, mentioned above. Europe was a little slower to set up departments of sociology but quicker with associations and journals.

By the outbreak of the Great War, sociological societies, journals of sociology, and university courses in sociology were established institutions in most countries of the metropole. International links were built up, for instance, through René Worms's Institut International de Sociologie, launched in 1893, through visits both ways across the North Atlantic and through the journals. These provided a practical basis for sociology to develop as an international (more strictly, intermetropolitan) cultural formation. Histories that emphasize the distinctness of national traditions (e.g., Levine 1995) underestimate the extent to which scholars of the period were oriented to an international academic milieu and conceived of "sociology" as a universal science. Even a minor figure such as Stuckenberg (1903), the author of a now-forgotten evolutionary sociology, traveled from Boston to Berlin, Paris, and London collecting his materials and became well informed about the state of sociology on both sides of the Atlantic.

Overlapping the academic initiatives, and at first more important, was a genre of popular sociological writing. A text like Kidd's *Social Evolution* could be a considerable best-seller. Within four years of its publication in 1894, this book had gone through 14 printings in England and had American, German, Swedish, French, Russian, and Italian editions.

Sociological thought first circulated as part of the uplifting and informative literature avidly consumed by the new educated reading public. The same public, in the English-speaking countries, read novelists like Charles Dickens and George Eliot, cultural critics like John Ruskin and Matthew Arnold, and scientists like Thomas H. Huxley. Sociology circulated through the same channels as these writers. Thus, Spencer's *The Study of Sociology* ([1873] 1887) was first published in installments in magazines—the *Contemporary Review* in Britain and *Popular Science Monthly* in America. It was then issued in book form in a new popular education collection called the International Scientific Series. Spencer's *Principles of Sociology*, an integral part of the vast survey of human knowledge that he called the "synthetic philosophy," was first issued in installments to subscribers, the first volume coming out in 10 parts over three years, as it was being written.

The relationship between writers and readers was far from what sociological writing later became—the issuing of self-contained, impersonal re-

search reports to an abstract, professional audience. Lepenies (1988) has suggested that European sociology was culturally positioned "between literature and science," but that exaggerates the contrast. Science too at this time was ethically and politically charged. Darwin, for instance, long hesitated to publish his work on evolution because he knew its religious and political consequences (Desmond and Moore 1992). Sociologists were expected as scientists to provide moral and political teaching. Their teaching especially addressed the dilemma that was inescapable for men of the liberal bourgeoisie: the tension between material privilege and reforming principle.

Nineteenth-century liberalism was, of course, a complex movement. It was not necessarily egalitarian and was often at odds with radical and democratic movements. But in liberal struggles against the ancien regime, commitments were forged that Hobhouse rousingly summarized in his *Liberalism* (1911): civil liberty and the rule of law, fiscal liberty, personal liberty, social liberty, economic liberty, domestic liberty, local, racial, and national liberty, international liberty, political liberty, and popular sovereignty. These remained culturally powerful beliefs in the public addressed by sociology.

These commitments were challenged by the class and gender inequalities of the metropole. The former is already recognized in discussions of the history of sociology (e.g., Therborn 1976); the latter is increasingly recognized in feminist revisions of that history (e.g., Deegan 1988; Lehmann 1994). What is still to be recognized is that liberal commitments were even more severely challenged by empire. Concepts of liberty, rights, and independence were plainly, repeatedly, and brutally violated by what the North Atlantic states were doing all over the world to the colonized.

The language I use is not overdramatized. Imperial expansion through this period was continuous and violent. It aroused resistance from the colonized, from the Little Big Horn to Zululand. It aroused protest from intellectuals in the colonies and the metropole. Contemporaries like the Anglican Bishop Colenso in South Africa (Guy 1983), the Australian historian G. W. Rusden, and the American journalist Park (whose indictment of Belgian rule in the Congo [Lyman 1992] is a striking prelude to his later career in Chicago sociology) denounced the murderous treatment of indigenous peoples. As Rusden (1908, 2:161) said,

It is not to be supposed that all colonists were accomplices in, or even knew of, the atrocities which were rife; nor were all offenders of like character. Some destroyed every native they met. Some with compunction (and in self-defence, as they persuaded themselves) kept guard on their property, and took life but rarely. At some stations in the interior there were but few ser-

vants, convicts or freedmen, and they were without moral restraint or support from a master. In all districts the first capture of a black woman, or the first shot fired at a black man, led to retaliation by the blacks, and thence to indiscriminate slaughter.

Hobhouse (1904, p. 56), consistent with his progressive liberalism, was a vehement critic of the ideology of imperialism, which he denounced as an amalgam of "the dream of conquest, the vanity of racial domination, and the greed of commercial gain."

It is a familiar historical point that empire created political difficulties for liberalism: witness Gladstone's conscience-stricken wrestling with Egyptian and Irish issues. Empire also created cultural and intellectual difficulties (Semmel 1993). Sociology, the science of progress that claimed the world as its province and that used so extensively the data of empire, was positioned squarely in this contradiction. And it offered a resolution.

Sociology displaced imperial power over the colonized into an abstract space of difference. The comparative method and grand ethnography deleted the actual practice of colonialism from the world of empire. We do not find passages like the one just quoted from Rusden in Durkheim's account of segmentary societies, nor is the violent pressure of the Western powers on China throughout the 19th century part of Weber's comparative sociology of Chinese and Western culture.

The relation between the imperial powers and the conquered was most directly addressed by the Darwinian wing of Comtean sociology (Spencer, Sumner, Ward, Hobhouse, Kidd, and fringe figures like John B. Crozier). They addressed it by constructing a fiction of "social evolution" that naturalized global difference. It is no wonder that Spencer became immensely influential in the colonies of settlement, where the idea of the evolutionary superiority of the settlers and their institutions replaced missionary religion as the main justification of empire.

This was despite the fact that Spencer was personally opposed to imperial conquest. In Spencer's (1887, p. 212) writing, we do find sharp denunciation of "the diabolical cruelties committed by the invading Europeans" in America, the South Seas, and elsewhere. Like Gladstone—with whom he discussed the question—Spencer saw forcible conquest as a sign of militarism. But he had no such objection to peaceable settlement and economic competition. In the same passages, it is clear he regarded the colonized as "inferior races," likely to lose out in evolutionary competition (Spencer 1887, p. 212; Duncan 1908, p. 224). Even Hobhouse (1911, p. 43), in full flight expounding the principles of liberalism, blurred them in the case of empire by wondering if the black races were capable of self-rule.

In other writers, there was no distance at all between naturalizing progress and justifying empire. The climax of Kidd's *Social Evolution*, quoted

above on the subject of race, was a justification for rule over the tropical regions of the world—now languishing under the maladministration of “the black and coloured races”—by the more progressive peoples of European extraction. Kidd’s reconciliation of imperial rule, with his belief that natural selection inherently tended toward more religious and ethical conduct, epitomizes the ideological work done by sociology. After reviewing the advantages brought by the British to India, Kidd (1898, pp. 324–25) judiciously considered the case of northern Africa and concluded, “The work undertaken by France in Algeria and Tunis, although it has differed in many important respects from that performed by Great Britain in India, and although it has been undoubtedly more directly inspired by the thought of immediate benefit to French interests, has been on the whole, it must be frankly confessed, work done in the cause of civilisation in general.”

The resolution that sociology offered to the dilemmas of liberalism claimed the status of science. Mill and Comte had insisted programmatically that sociology must promulgate “laws.” This task was accepted by both academic and popular writers in sociology. Legitimacy for “laws of progress” was provided by the prestige of geology and evolutionary biology. Accordingly, treatises on sociology often expounded organic evolution and might even start with the evolution of the stars and the solar system (e.g., Ward 1897).

This conception of “laws of progress” enabled sociology to conflate the problems of empire with the problems of the metropole. The “social science” of the 1860s and 1870s embraced the social tensions of the metropole as ethical and practical problems. Investigations such as Booth’s (1902) *Life and Labour of the People in London* were more likely to be called “statistics” than “sociology.” Nevertheless questions of poverty, class struggle, and social amelioration—“the social question” in the terminology of the day—certainly came on the agenda of the sociological societies and journals in the 1890s and 1900s. In cities such as London, Chicago, and Paris, there was significant contact and overlap between academic sociologists, Fabian socialists, feminists, progressive liberals, religious and ethical reformers, and social workers (Besnard 1983; Deegan 1988).

What “the social science” contributed to “the social question” was an interpretation of the metropole’s problems in the light of an overarching theory of progress. The best-known example now is Durkheim’s conception of anomie as a pathological consequence of the division of labor. A more characteristic example then was the discussion of socialism found in many treatises of sociology. The universal approach of the sociologists was to evaluate the goals of the workers’ movement in terms of their own model of evolutionary progress—whether the conclusion was cautious endorsement of ethical socialism (as by Hobhouse, Durkheim, and Albion

W. Small) or robust rejection, as by Spencer or this one from Sumner (1963, p. 97): "The sound student of sociology can hold out to mankind, as individuals or as a race, only one hope of better and happier living. That hope lies in an enhancement of the industrial virtues and of the moral forces which thence arise. . . . The socialist or philanthropist who nourishes them [i.e., fools who insist on living in slums, etc.] in their situation and saves them from the distress of it is only cultivating the distress which he pretends to cure." The force of this argument lies in Sumner's widely shared conviction that the general laws governing social progress, established by evolutionary sociology, operate with rigor in the metropole as elsewhere.

## THE CRISIS OF SOCIOLOGY

### War and Progress

In *Pure Sociology: A Treatise on the Origin and Spontaneous Development of Society*, Ward (1903, pp. 450–51) offered as proof of progress the fact that atrocities were scarcely possible in modern society. Just 13 years later, the British army lost 60,000 young men killed or wounded on a single day at the opening of the Battle of the Somme.

The Great War marked a crisis of the old imperialism and triggered major shifts in global power. The European empires began to fissure, with the independence of Ireland and the dismemberment of the Hapsburg Empire. The French, British, and American systems continued to expand by taking over former Turkish and German territories and increasing their economic penetration of China and Latin America. The United States emerged as the leading industrial power. The 1940s saw a second crisis of imperialism, with the Japanese offensive against Western power in Asia, the independence of India from the British and Indonesia from the Dutch, and the Vietnamese war of liberation against the French. Soviet power reconstituted the old tsarist empire but supported the breakup of other empires and stood as a challenge to global capitalism. By mid 20th century, the United States had become the leading international investor, the dominant military power, and the center of mass communications and an emerging world commercial culture.

These changes decisively altered the conditions of existence for sociology as a collective enterprise—beginning with the Great War itself, which precipitated a disciplinary crisis. The war tore apart the intellectual community of sociologists that had been developing around the North Atlantic. Some, like Hobhouse, were horrified by the fighting; others became belligerent. Durkheim and Giddings became vehement anti-German propagandists; Small at Chicago broke with his German contacts, clashing with Simmel in particular (Bannister 1987). Weber went into the army,



as did a number of the younger members of the *Année sociologique* group, some of whom were killed. After serving for a while with the rank of captain, Weber threw himself into political projects to overcome the internal weaknesses of the German state. The young German Sociological Society liquidated itself when war broke out and donated its money to a fund for German war propaganda in neutral countries (Liebersohn 1988)—ironically, exactly the work that Durkheim undertook on the other side.

For all that, the more important impact was at the level of ideas. The course of social evolution had generally been understood as the growth of reason and civilized conduct: "Objectively viewed, progress is an increasing intercourse, a multiplication of relationships, an advance in material well-being, a growth of population, and an evolution of rational conduct. It is a final display in the grand metamorphosis of universal evolution" (Giddings 1896, p. 359). When brutal and protracted war in Europe made this an untenable view of civilization—as the experience of the frontier came home to the metropole—more than just a hypothesis had to be revised. The foundation of sociology's worldview was ruptured. It was no longer possible to take "progress" unproblematically as the reality to be studied, the object of knowledge. "Cheap optimism," as Hobhouse (1915) realized during the war, was now forbidden by history. Comtean language was too well entrenched to be immediately abandoned, but in books written after the war, evocations of progress tail off quite soon. The Park and Burgess "Green Bible," first issued in 1921, was on the cusp of this change. It ended with a chapter defending the concept of progress, but the chapter was a defense against acknowledged criticism, and none too confident at that.

The war might not have had such an impact if the concept of progress were not already under challenge. A broader cultural crisis was developing in early-20th-century Europe, marked by sharp breaks in the visual arts, physics, music, and psychology. What Berger (1974) has called "the moment of cubism" was also the moment of Albert Einstein, Freud, and Arnold Schoenberg. When Pareto in the *Treatise of General Sociology* ([1916] 1935) denounced "sentimental ethics" as a basis for sociology and demanded a harsher realism, he was distancing himself from what he already saw as the pseudoscience of Comte and Spencer and from his own earlier liberalism.

That the framework of sociology created in the 19th century had reached its limits was indicated, finally, by institutional stagnation. British sociology entered a fallow period after the war, with Hobhouse retreating into metaphysics, while the intellectual excitement of the new ethnography was channeled off into a separate discipline, "anthropology," under the leadership of Bronislaw Malinowski and A. R. Radcliffe-Brown. Durkheim's close-knit group dispersed. Though some of its members con-

tinued to have distinguished individual careers, French sociology as a collective enterprise languished (Karady 1983).

Out in the colonies, an attempt to institutionalize Comtean sociology in the Australian university system (Anderson 1912) came to nothing. The first chair of sociology in that country was not filled until 1959. In South Africa, imperial sociology made its little-known final bow. As the racial hierarchy created by conquest hardened, it was a sociologist, Geoffrey Cronjé (1947), who produced the intellectual blueprint for the apartheid regime.

### Alternatives

The decade around 1920 may thus be understood as the historical moment when the project of Comte, Spencer, Letourneau, and their successors had irretrievably broken down. It is clear that there was more than one possible replacement.

One possibility grew out of the cultural crisis developing in the central European intelligentsia. A measure of the depth of this crisis was Oswald Spengler's *Decline of the West* ([1918–22] 1932), which was enormously influential in the 1920s. Like Pareto, Spengler rejected the concept of social evolution; unlike Pareto, he was also acutely conscious of the imperial gaze. Spengler offered a scathing criticism of the Eurocentrism of European intellectuals and their view of human history. He saw European imperial expansion as the “murder” of other cultures, such as the Aztec/Maya culture in Central America, and thus treated contemporary imperialism not as the triumph of light but as a sign of the Last Days, evidence of decline. The break with the framework of progress could hardly be more complete.

The project of a critical sociology of culture was taken up by Sorokin, leading eventually to the immense, cloudy generalities of *Social and Cultural Dynamics* (1937–41). In Weimar Germany, from different starting points, Max Scheler and Karl Mannheim converged on a “sociology of knowledge,” the object of fierce controversy for a short period. Mannheim ([1935] 1940) in exile, like the scholars of the Frankfurt school, began to develop a synthesis of Freudian psychoanalysis with structural sociology to explain the catastrophe of fascism. A synthesis of phenomenology with sociology was proposed by Alfred Schutz ([1932] 1972) in Austria; a social theory grounded in idealist philosophy was proposed by Giovanni Gentile in Italy (Bellamy 1987).

The revolutionary movements of metropole and colony also gave rise to possible sociologies. In Russia, the Bolshevik leader Nikolai Bukharin ([1925] 1965) produced “a system of sociology” as ambitious as anything in the capitalist world at that time. Much better remembered are the social

theories of the Italian revolutionary Antonio Gramsci, who developed in the 1920s and 1930s not only the famous analyses of intellectuals and hegemony, but also theories of socioeconomic change that have influenced current discussions of postmodernity (Harvey 1990). Du Bois (1968), having left academic sociology for civil rights activism, connected race issues in the metropole with movements in the colonial world and, increasingly, with the structure of global capitalism.

None of these initiatives, however, produced an institutionalized replacement for the old sociology. The principal reason was not their intellectual calibre—this work includes the most sophisticated social theory in the first half of the 20th century. It was, rather, the balance of global power and the growth of totalitarianism in Europe out of the same cultural and political crisis that had crippled sociology. Bukharin was purged and then killed under Stalin. Gramsci was arrested under Mussolini and died after years in prison. Sorokin fled the Bolsheviks; Mannheim, Schutz, and the Frankfurt school fled the fascists. Gentile became a minister under Mussolini but was killed during the overthrow of the Fascist regime, and his influence died with it. Colonial dominance in Africa remained unshaken, and Du Bois, returning to academia in the 1930s, found no audience for his internationalist views.

### The Epistemological Break: A Practical Sociology of the Metropole

There was only one place where academic sociology flourished between 1920 and 1950: in the new metropole, the United States. But here the discipline was transformed. The United States was the arena of a change so fundamental that it can be regarded, to use Louis Althusser's expression, as an epistemological break (Althusser and Balibar 1970).<sup>4</sup> In the academic space created by the late-19th-century sociologists, a new enterprise arose. It bore the same name but had a different object of knowledge, a new set of methods and applications, a revised definition of the discipline, a new audience, and—eventually—a new view of its history.

The new object of knowledge was society and, especially, social difference and social disorder *within the metropole*. Nineteenth-century sociology had some concern with these issues, as already noted, but had intellectually subordinated them to the conceptions of global difference and evolutionary progress. In the 1920s and 1930s, the internal problems of the society of the metropole became the intellectual center of sociology.

<sup>4</sup> I emphasize that this was an *epistemological* break, not an abrupt institutional change; the transition took a couple of decades. Comtean texts could still be produced after the Great War (e.g., Case 1924). But the sociology centered on concepts of progress was visibly in retreat by then and had almost vanished by the 1930s.

Familiar markers of this shift are the prominence of the Chicago school's urban research and the growth of specializations within sociology—many of them defined by a social problem or an administrative apparatus of the society of the metropole. The custom of organizing American Sociological Society meetings in specialized subdivisions along these lines began in 1921.

In terms of method, where the old sociology had focused on difference between the metropole and the primitive, the new sociology focused on difference *within* the metropole. This can be seen clearly in the increasingly influential statistical techniques, from early measures of correlation through attitude scaling in the interwar decades to the formalization of "latent structure analysis" by Lazarsfeld at midcentury. The new quantitative procedures had as their logical basis the idea of difference within a population. Their main applications were to surveys and official statistics collected within metropole countries, principally the United States (Easthope 1974).

It is familiar that the 1920s and 1930s saw a flowering of empirical research on the social life of American towns, cities, and suburbs. There was great inventiveness in method, from the statistics of difference through urban ethnography to the first fusion of psychoanalysis with field sociology (Dollard 1937). Sociological empiricism was not new—as shown above, Letourneau, Spencer, and Sumner had put great energy into assembling facts—but the new empiricism had immeasurably superior techniques and new intentions. The Chicago school did not just do urban ethnography. It set up a comprehensive system of surveillance for America's second largest, and most turbulent, city (Smith and White 1929). In this, as in many other sociological projects, the intention was defined by an increasingly popular term, "social control."

The rapid development of sociology in this direction was made possible by corporate and government funding, begun before World War I but greatly accelerated in the 1920s. Ross (1991, p. 402) gives the impressive total of \$41 million of Rockefeller money going to American social science and social work between 1922 and 1929. (Twenty years earlier, Du Bois had been unable to raise \$5,000 a year for his Atlanta research.) A high point was reached when the national government set up the Committee on Recent Social Trends, with William Fielding Ogburn, a president of the American Sociological Society and its leading advocate of empiricism, as director of research.

As research expertise rose, however, the conception of the discipline narrowed. R. M. MacIver (1937, p. vii), a successor to Giddings at Columbia, opened his textbook *Society* by acknowledging a crisis in the definition of the subject: "Abundant controversy has arisen over the question whether there is at all a subject deserving to be named sociology, whether

if there is, it is a science, whether in that event it is a generic or a specific science, and so forth." The only secure institutional base available for sociology at this time, the American university, provided an organizational definition that contradicted the Comtean vision of "the social science." Sociology could survive in the American university not as a metascience but as one department among a range of social science departments, distinguished from history, political science, economics, and psychology only by its special focus of interest. The intellectual difficulty, for a discipline whose earlier claims had been so vast, was to define what that special focus was. "Social relations" (MacIver 1937), "groups," forms of association, and human relations (Hiller 1933), "the social process" (Reuter and Hart 1933)—no formulation was very convincing, and none became generally accepted.

Bannister's (1987) absorbing history of sociological scientism in the United States in this period shows that the empiricist triumph failed to produce an intellectual program for sociology. The empiricists themselves fell apart in bitter faction fighting, which racked the American Sociological Society in the 1930s. Sorokin (1928, p. 757), an acid critic of the empiricists, remarked at the end of his survey of *Contemporary Sociological Theories* that "the whole field reminds one of a half-wild national forest rather than a carefully planned garden." The technocratic vision articulated by some empiricists (e.g., Smith and White 1929) lost credibility as funding dried up in the 1930s. The new sociology started life as the old sociology died, with a severe deficit of legitimacy.

## MAKING THE CANON

In this conceptual vacuum—as Hinkle (1994, p. 339) aptly describes the situation after the collapse of evolutionism—the formation of the classical canon began. The process lasted a generation. It was the work of many hands, and the canon makers were far from harmonious—two of the most prominent being Parsons and C. Wright Mills. Over this period, nevertheless, a series of projects and debates converged on a new understanding of sociology's history. Intellectually, the process of canon making has two sides: the creation of a canonical point of view and the selection of particular founding fathers. Interwoven with both is a practical enterprise, the dissemination and pedagogy of "classical" texts.

Though the point is difficult to document, I would suggest that a condition for these changes was a change in sociology's audience. The late-Victorian liberal reading public was no more. However, the enormous wealth being accumulated in the rising world power, the United States, made possible, for the first time in history, a mass higher education system. Here sociology expanded tremendously as a teaching enterprise in the

three decades after World War II (Turner and Turner 1990). A mass audience of students required a teacher training program. The expanding graduate sociology programs, which did this job, became central to sociology's professional self-definition. It was in this milieu, and at this juncture, that the pedagogy of classical texts developed.

### The Canonical View

The emerging vision of sociology's history identified a foundational moment, "that brilliant age in which the foundations of the discipline were laid" (Bottomore and Nisbet 1978, p. x). It was characterized by the texts of a small group of key figures: "There are four men, however, whom everyone in sociology, regardless of his special emphasis, bias, or bent, will probably accept as the central figures in the development of modern sociology" (Inkeles 1964, p. 3).

Few sociologists will miss the echo of Parsons's *Structure of Social Action* (1937). Parsons was not the only, nor even the first, American theorist to address the intellectual disintegration of sociology (Turner and Turner 1990, pp. 71–73), but there is no denying the genius of his solution. Parsons purged the discipline's history, acknowledging the collapse of the Comt-ean agenda ("Spencer is dead"; 1937, p. 3). He took the *empirical* problem of postcrisis sociology, difference and disorder in the metropole, and made it the *theoretical* center of sociology (the Hobbesian "problem of order"; 1937, p. 89). He even made this the basis of the discipline's definition.<sup>4</sup> Parsons solved the problem of where sociology fitted by inverting the academic division of labor: the sociological conception of action became the basis for legislating the places of the other "sciences of action."

Parsons was no historian and did not claim to be writing the history of sociology. His project was to construct the logical bases of the science. But his reconstruction of the "emergence" of the action model in the theoretical logic of Alfred Marshall, Pareto, Weber, and Durkheim was understandably read as an origin narrative, and this story, as Camic (1989) has shown in an important article, became a means of legislating for the discipline. Parsons himself gave a strong lead toward this reading by his vigorous championing of Weber as a key figure for sociology. Parsons not only issued two volumes of translation but published extended commentary, and Weber was still the undisputed star of *Theories of Society*, the book of readings that Parsons and his collaborators issued in 1961.

*The Structure of Social Action* was not an overnight success; it became

<sup>4</sup> In Parsons's (1937, p. 768) matchless prose, sociology is "the science which attempts to develop an analytical theory of social action systems in so far as these systems can be understood in terms of the property of common-value integration."

more important as Parsons's influence in American sociology grew over the following 20 years. The canonical view had to be established against other accounts of sociology. A residual version of the interwar encyclopedic view can still be found in some introductory textbooks after World War II (e.g., Murray 1947) and in such a substantial review as Nicholas S. Timasheff's *Sociological Theory* ([1955] 1967). There also survived from the 1920s an American-centered empiricism, which saw early sociological theory as a bad dream from which the discipline had been awakened by the kiss of science (e.g., Lundberg, Schrag, and Larsen 1958).

Parsons's vision, however, acquired powerful and unexpected allies. Sociology as social criticism could also yield a canonical view. In his very widely read *Sociological Imagination*, Mills (1959) constructed a composite image of "the classic social analyst" (his examples include Marx, Durkheim, and Weber, as well as Spencer, Mannheim, Thorstein Veblen, and others), which he held up as a model of how sociology ought to be done. "Classical sociology" to Mills was a style of work, not a period—though there was a definite sense that it was more practiced in the past and that the present was a period of decline. Mills (1960) soon turned the concept into a textbook, defining "the classic tradition" in a rather freewheeling way but including readings from all major figures in the emerging canon.

The canonical view was also reinforced by theorists wishing to establish a problem or a field as a central issue. Robert K. Merton's ([1949] 1957) account of anomie helped establish Durkheim as classic. Lewis A. Coser (1956) performed a like office for Simmel as founder of a theory of conflict. Not only functionalism but also the critique of functionalism fed into the canonical reading of history. An opposition between "conflict theory" and "consensus theory," symbolized by the classics Simmel and Marx versus Durkheim, became a textbook cliché (e.g., Broom, Selznick, and Darroch 1981).

The translation of the main texts incorporated into the canon was accomplished by 1950 (see below). Of course not all texts that are translated get recognized as classics—Leopold von Wiese (1932), for instance, was translated but is now forgotten in Anglophone sociology. The mark of canonization is the development of a genre of commentary and exposition. Levine (1995, p. 63) aptly remarks of the 1960s and 1970s that "fresh translations, editions, and secondary analyses of classic authors became one of the faster-growing industries within sociology." Indeed the rush had begun earlier. Long commentaries on Weber appeared with the translations in the 1940s (Gerth and Mills 1946), and Parsons made an exposition of Weber the centerpiece of the first edition of his *Essays in Sociological Theory* (1949). Reinhard Bendix's very widely read *Max Weber: An Intellectual Portrait* was issued in 1960. Coser's *Functions of Social Conflict* (1956) was in large part a commentary on Simmel. Elaborate symposia

on Simmel and Durkheim, edited by Wolff (Simmel's translator), were published as books in 1959 and 1960, respectively. Platt (1995) traces a literature of commentary on Durkheim beginning in the late 1930s and picking up steam after midcentury. In 1958–59, centenary celebrations of Durkheim and Simmel appeared in both the *American Journal of Sociology* and the *American Sociological Review*.

As this genre developed, American interest both stimulated and was reinforced by European writing. The process is well illustrated by a British text, John Rex's *Key Problems of Sociological Theory* (1961). Rex's main points of departure were Durkheim and Weber as read in America. Rex had studied Parsons, and though he took more interest in class struggle—and therefore Marx—than the functionalists did, the new construction of canonical theory was the basis of his reasoning. American interest even helped to create a Weber revival in German sociology "after a period of inattention to its classical past," as Lüschen (1994, p. 11) delicately puts it. The German sociologists got round to a celebration of Weber at their national conference in 1964. This year also saw the revision and wider circulation, in paperback, of Winckelmann's great edition of *Wirtschaft und Gesellschaft* (Weber 1964). The sophisticated historical scholarship that has followed (Tribe 1989) has steadily undermined the "Weber" of Parsons, Mills, and Bendix, with little effect so far on sociological theory.

By the early 1960s, a quarter century after Parsons's *Structure of Social Action*, the canonical view was firmly established in American sociology. It was not the only view of the discipline's history available, but it was by now hegemonic. When Alvin W. Gouldner delivered his famous "anti-Minotaur" address to the Society for the Study of Social Problems in 1961, he could simply assume that all his audience regarded Weber as a central figure in the discipline. Gouldner (1973) could thus make points about sociology in general by criticizing a canonical text.

Translators and commentators saw themselves as providing a vital resource for contemporary sociology. On the face of it, the resource was intellectual, a set of ideas and methods. But something more was involved. Here I would call attention to the *language* of canon making: the imagery of the "golden age in sociology" (Nisbet 1967), when "giants" (Smelser 1967) walked the earth. The hagiographical language continued in evocations of "that brilliant age" (Bottomore and Nisbet 1978), in a book series called *Masters of Social Theory*, and in the textbook evocations of "pioneers" (McNall 1971) and "Founding Fathers" (Inkeles 1964)—terms with strong emotional overtones in the United States especially.

Such language indicates that emotional labor is being done (Hochschild 1983). The construction of the canon provided not only an intellectual but also a symbolic solution to the internal disintegration and cultural



marginalization that had overtaken sociology before the midcentury. Inheritors of a golden age, bearers of the insights of great thinkers, sociologists had weight in the world—in their own eyes and, increasingly, in the eyes of students.

### The Choice of Fathers

Like Parsons, Inkeles in the textbook quoted above had a list of four great men—but not the same four. Inkeles listed Comte, Spencer, Durkheim, and Weber. It is an important fact (confirming the symbolic and emotional work done by the canon) that a canonical view of sociology could be shared by people who disagreed about which particular fathers had done the founding.

Parsons's attitude, that is to say, was more influential than his precise arguments. Some of his choices were accepted. Merton reinforced the interest in Durkheim; Gerth and Mills reinforced the interest in Weber. Each could be read as creator of a systematic conception of sociology, a key need of the academic discipline at the time. More, each as founder could do ideological work for sociology in the Cold War context: Durkheim as theorist of social solidarity, Weber as refuter of Marx. Platt (1995) rightly observes the complexity of influences behind the choice of particular founding fathers: broad historical circumstance, particular academic entrepreneurs or departments, affinity with current trends in the profession. These factors seem to have worked for Weber and Durkheim but against Parsons's other nominee, Pareto. Though Pareto was even more eligible as a systematist, his irony and pessimism were perhaps too obtrusive for his texts to work as "foundations" for the revived discipline. Pareto survives as a name among the founding fathers, but his writings are not routinely studied in the pedagogy of classical texts.

The complexities are greatest, and the changes most dramatic, in the case of Marx. To Parsons in *The Structure of Social Action*, he was part of the background, essentially a minor utilitarian, and in *Theories of Society* he received less attention than Mead, Pareto, or Freud. To Merton (1957) in *Social Theory and Social Structure*, Marx was no more important. Some American textbooks of sociology in the 1940s and 1950s got along without any attention to Marx at all. When Marx did get attention, students might well wonder why; Bierstedt (1963, pp. 544–45) for instance presented him as an author of "vast oversimplification," "excessive, dogmatic, and radical."

But Marx was, of course, the founding father of modern social science from the point of view of orthodox socialism. However debauched the Stalinist version had become, Marxism remained an intellectual force in

global culture. It was the point of departure in the sociology of class even for non-Marxists like Ralf Dahrendorf (1957) and Stanislaw Ossowski (1963). In the United States, Marx was a significant presence to the neo-conservative theorists of the Cold War era. Thus to Daniel Bell (1960), Marx was a major theorist of alienation as well as the ancestor of Soviet communism. A progressive American sociologist could find in Marxism an important resource; in best canon-making style, Mills issued a collection of Marxist texts, with commentary, in 1962.

However Marx did not become a full-fledged member of the sociological canon until the dramatic expansion of sociology in the 1960s and the radicalization of university students. Sociology's new audience made its needs known. The "radical sociology" proposed by the student movement centered on Marx and Marxists (Horowitz 1971). From the other side, studying Marx provided a way for academic sociologists to talk to their new audience about social struggle and transformation. In 1965, the American Sociological Association annual meeting included a plenary session tellingly named "A Re-Evaluation of Karl Marx." Marx assumed a more prominent place in accounts of the history of sociological theory (Bottomore and Nisbet 1978) and appeared with increasing frequency in textbooks for undergraduates. The account of the formation of sociology was revised, in new editions of established textbooks, to give more prominence to Marx (cf. Bottomore 1962, 1987). By the late 1970s, he figured in some introductory texts as "the first great radical sociologist" (Sherman and Wood 1979, p. 7).

The canonization of Marx required a change in content for sociology but not a change in the conception of theory. The established pedagogy of classical texts was readily adapted. Marx's writings had long been available in translation because of their importance to socialism. They could now be positioned in opposition to a "consensus" position, or an "elitist" position, or a "micro" position, in retrospective constructions of sociological traditions (Nisbet 1967; Collins 1994). New editions and selections of Marx's writings appeared, and a sociological literature of commentary multiplied.

The grouping of Marx, Durkheim, and Weber was, thus, a late development in the construction of the canon. Durkheim and Weber were the survivors of the canon-making enterprise of Parsons's generation, Marx was grafted on in the next generation, and others fell by the wayside. The trio appears in the role of the founding fathers in elementary textbooks in the 1970s (McGee 1977). In theoretical sociology, a considerable effort of secondary interpretation tried to make sense of the Marx-Durkheim-Weber grouping, in the work of Giddens (1971), Alexander (1982-83), Seidman (1983), and others.

## Dissemination

In Victorian scholarship it had been assumed that an English-speaking academic could read the major European languages, plus two dead ones. But for the study of a set of texts to become a teaching routine in American universities, translation was essential. Accordingly, the process of translation is an important index of the formation of a canon.

Translation of sociological books became a systematic process in the 1930s, aided by the emigration of European scholars to North America. The young Parsons, after visiting Heidelberg and negotiating with Marianne Weber, translated Max Weber's *Protestant Ethic and the Spirit of Capitalism*; the product appeared in 1930. Von Wiese's *Systematic Sociology* (trans. by H. P. Bieleke) appeared in 1932. English versions of Durkheim's *Division of Labor in Society* (trans. by G. Simpson) appeared in 1933, *The Rules of Sociological Method* (trans. by S. A. Solovay and J. H. Mueller) in 1938. Pareto's *Trattato di sociologia generale*, curiously renamed *The Mind and Society*, was published in translation in 1935, Tönnies's *Gemeinschaft und Gesellschaft*—renamed *Fundamental Concepts of Sociology*—in 1940. In 1946 Gerth and Mills issued their influential selection from the second part of Weber's *Wirtschaft und Gesellschaft*, and in 1947 Parsons issued his translation of the first part. Passages from Simmel had been available in translation from the 1920s, but a larger selection was translated by K. H. Wolff in 1950.

The translations were received in the United States with less than universal rapture (Hinkle 1994). Yet they provided the essential basis for the pedagogy of "classics" to become established in American graduate education. As this pedagogy became systematized, books of readings in sociological theory, with extracts from the classical texts, appeared. Coser and Rosenberg (1957) edited one of the first, noting that with the rediscovery of Weber, Durkheim, and Simmel, American sociology had produced a "renaissance of sociological theory." Books of readings made it possible to teach an abbreviated version of the canon in undergraduate courses: Dennis H. Wrong and Harry L. Gracey (1972) claimed to have pioneered this at the introductory level. "Classical theory" courses appeared in undergraduate as well as graduate lists. Thus a canonical point of view became available from start to finish of higher education in sociology. What was available, in due course, became required: thus the Graduate Record Examination in sociology demands a familiarity with canonical authors (ETS 1987). Thus, through the pedagogy of classics, the canon became part of the constitution of sociology as a force in American higher education.

The effect was not confined to the United States. In the postwar de-

cares, a liberal intelligentsia was reconstituted internationally, based in growing higher education systems and the new culture industries. This was done under American hegemony, in the context of Cold War politics and U.S. investment abroad. American foundations once again were active players in the process (Berman 1983). Academic sociology followed the general pattern.

In most countries that could afford to have sociology at all, the discipline was created or remade in the 1950s and 1960s on the basis of research techniques, research problems, and theoretical languages, not to mention textbooks and instructors, imported from the United States (e.g., Japan, Tominaga [1994]; Australia, Baldock and Lally [1974]; Scandinavia, Al-lardt [1994]). Maus's (1962, p. 165) well-known *Short History of Sociology*, written in the mid-1950s, acknowledges "the dominating position American sociology occupies in the world today"—a comment the more revealing because written from a German point of view.

With the reconstructed discipline came its reconstructed foundation story and the developing canon of classical texts. Sociology students from Nigeria to New Zealand found themselves studying the canonical texts as the basic definition of "theory." Sociologists in the most remote parts of the world began to define themselves in the categories of the canon. A survey of Australian and New Zealand sociologists in the early 1970s reported that, of those who identified themselves as members of a school, "three named symbolic interactionism, two functionalism, three Marxist, three Weberian sociology, and another three emphasised the ideas of Durkheim. . . . It is interesting that only four respondents mentioned modern sociologists as having influenced their work" (Baldock and Lally 1974, pp. 279–80).

Thus world sociology arrived at the situation described in the opening paragraphs of this article. In a sense, the canon on a world scale did what it had already done in the United States: providing a symbolic focus, a shared language, and some kind of identity, for academics and students in sociology. Though many local variations have developed, the same accent of canon making can be heard in evocations of "Max Weber, the Living Classic" (Käsler [1994] in Germany), the "classical mainstream program" (Boudon [1994] in France), and "one of sociology's few true classics" (Kalberg [1996] in the United States).

## REFLECTIONS

In his article on "the centrality of the classics," Alexander (1987) observed that classical theory performs an integrative function for the discipline. This is undoubtedly true but not very helpful unless we also know why the need for integration arose.

I have argued that the classical canon in sociology was created, mainly in the United States, as part of an effort at reconstruction after the collapse of the first European-American project of sociology; that the idea of "classical theory" replaced earlier and very different accounts of the making of sociology; and that this whole course of events can only be understood in the framework of global history, especially the history of imperialism. If this is broadly correct, it has implications for contemporary thinking about theory, beyond the calls for a more inclusive canon.

The circumstances in which the canon was created produced a striking disjunction in academic sociology between "theory" and "research." The creation of the canon followed an extremely creative period of empirical research. The canon provides broad symbolic legitimation for the discipline, but none of the elected fathers actually motivates the empirical activities of post-1920 sociology at all well. Despite the designation of "methodological classics," the main line of modern research methods does not run through Marx, Durkheim, Weber, Pareto, or Simmel.

The teaching of the canon in American graduate education did, nevertheless, consolidate the ideology of professionalism in sociology that the empiricists of the 1920s struggled to establish. As Stinchcombe (1982) observed, reference to the classics has become a badge of membership in a professional community. But that membership comes complete with the patterns of hegemony inscribed in the canon. It thus becomes important to consider not only which *writers* are included and excluded, but also which *problems*.

This is particularly important in relation to the formative issue of empire. The making of the canon deleted the discourse of imperialism from sociology. Those Comtean notables whose texts had most explicitly concerned the primitive, the concept of progress, racial hierarchies, and gender and population issues failed to be canonized. ("Spencer is dead.") Those texts of canonized authors that most clearly bore the mark of empire, such as *L'Année sociologique*, Durkheim's ([1912] 1995) *The Elementary Forms of the Religious Life*, or Weber's inaugural lecture "The National State and Economic Policy" ([1895] 1989), were the least likely to be used in the pedagogy of "classics."

This had the desirable effect of deleting open racism from the discipline's theoretical core. It had the undesirable effect of excusing most sociologists from thinking about global society at all. Ironically the major attempt to reverse this, "world-systems theory," has been institutionally defined as a new specialization.

Gender, sexuality, and race relations, which were core issues for evolutionary sociology, were pushed to the margins in the process of canon formation. Nisbet's (1967) list of the "unit-ideas of sociology" (community, authority, status, the sacred, alienation) was a travesty of history but not

a bad map of the narrowed territory left after the canon making. Recent critiques of the patriarchal subtext of classical theory (Sydie 1987; Seidler 1994), which analyze 19th-century texts as if the foundation story were true, miss this larger effect. The continuing influence of the canon in defining what counts is one of the main reasons why gender and race, though now institutionally important for American sociology (as shown by affirmative action programs or the sections of the ASA), have still not reestablished themselves as central concerns of sociological theory.

A definition of subject matter can have this effect only if it carries authority; the *style* of theory making associated with the canon is also important. We might recall that "canon" in English originally meant a rule or edict of the Church. "Classical theory" is a package that not only exaggerates the importance of a few great men but in the same gesture excludes or discredits the noncanonical. The sociologists of the late 19th century, to do them justice, were not like this. They had a sense of adventure, a skepticism about authority, and a breadth of interest, which we could still do with.

It follows that no repair job on the canon will meet contemporary intellectual needs; a revised pseudohistory of founding fathers (e.g., Turner 1993) does not help. But throwing away the discipline's history (Chafetz 1993) is no answer either. This would leave us with the consequences of history and no grasp of their causes.

What we need instead of "classical theory" is better history—sociological history—and an inclusive way of doing theory. Sociology can be introduced to students not as a story of "great men" but as a practice shaped by the social relations that made it possible. The full range of intellectuals who produced "theories of society" can be recovered for this history, including the feminists, anarchists, and colonials who were erased from the canonical story. The exclusions constructing the discipline can become part of the discipline's self-knowledge.

The argument of this article suggests that a vital step—though perhaps the most difficult—is to abandon the internalist history of sociology and recover the global context that Victorian sociology, in its own way, addressed. This means including in the process of theory formation the intellectuals of the colonized world as well as the metropole—not, however, by revising the story of "great men" to include Ibn Khaldun among the classics. Rather, it is a matter of studying the rich analysis of *the world in which sociology was constructed* that came from outside the metropole, ranging from Islamic and Chinese debates about modernity to Indian and African critiques of empire. This is not utopian; it has been done in the past. While Durkheim and his colleagues built the imperial gaze into their sociology, other French social scientists engaged intellectuals of the Is-

lamic world in dialogue about modernity, colonialism, and culture (Burke 1980). The history of the discipline is richer than we normally assume.

For their part, Marx, Durkheim, and Weber will still be present in this history. They will be present in realistic contexts and proportions, not as shadowy giants at the limit of vision.

## APPENDIX

### Sources for Views of "Origins" and Incorporation of the Canon into Sociology Teaching

This article's argument about accounts of origins and the crystallization of a classical canon in sociology teaching is based on (a) the programmatic statements and debates about classics and their teaching cited in the main bibliography; (b) an examination of current course descriptions in 30 U.S. graduate programs, reproduced in Career Guidance Foundation (1995); (c) a study of accounts of the history of sociology in English-language textbooks of introductory sociology and sociological theory. A list of the texts examined for this purpose follows. The period covered is 1896–1996, with a concentration on the last 50 years, tracing the arrival of the canon in textbook sociology. As anyone using such material will know, the boundaries of what is a "textbook" are blurred, and it is sometimes difficult to tell when a new edition should count as a separate book. Where more than one edition was consulted, I have listed the earliest.

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# A Sociological Guilt Trip: Comment on Connell<sup>1</sup>

Randall Collins

*University of California, Riverside*

The main arguments of "Why Is Classical Theory Classical?" are as follows. (1) Sociology arose out of the concerns and observations of European colonial empires, which led to the concepts of progress, evolution, and the primitive/modern contrast. (2) For all the evils of this beginning, early sociology had at least a central concern for gender and race, later forgotten. (3) These concerns broke down because of World War I, with the shift of sociology's center to the United States and to concerns for inequality and disorder in urban society. (4) Because this empirical work failed to legitimate itself, a classical canon was adopted.

The polemical power of the article stems from the first two points. Sociology's history is doubly tainted because its true origin is nothing more than the imperial gaze, the self-glorification of European "progress" at the expense of subjugating the nonwhite parts of the world and because the later establishment of a canon is intellectual dishonesty, preventing us from confessing how imperialist we really are.

Points 3 and 4, if presented by themselves, would make little splash. They are supported by observations that are familiar to most historians of sociology and only take on a lurid coloring from the imperialism-of-the-founding-generation polemic. What I will argue is that the first two points are drastically overstated, scarcely more than another one-sided revisionist move of the kind Connell claims to be repudiating. This leaves the last two points dangling. Rhetorical pathos aside, Connell never really answers his main question, Why is classical theory classical? Is it because it is imperialist? Or because it is a mask for imperialism? In fact he has no real explanation of canonicity, just a denunciation of it.

In point 1, the author wants to dismiss accounts that sociology originated in concerns for internal problems of European social change, such as industrialization, the rural/urban shift, and religious secularization. But it is easy to document just how concerned pre-1914 sociologists were with these issues. The earliest generations of the crystalizing field of sociological

<sup>1</sup>Direct correspondence to Randall Collins, Department of Sociology, University of California, Riverside, California 92521-0419.

argument, Saint-Simon, Comte, de Bonald, de Maistre, and, on the empirical side, Le Play, were centrally concerned with contemporary matters like the French Revolution and what would come after it, the abolition of the aristocracy and the rise of a new class in commercial-industrial society, the threatened abolition of religious faith and the need for a substitute, and the family changes that were the topics of Le Play's research. Connell cites a few scattered examples, such as Durkheim's reference to the Kabyle tribal people of the French Algerian empire, but one can prove anything by selective quotations. I would submit a straightforward factual point: the founding generations of French sociologists wrote much more heavily about the transformation their own society was undergoing than about the contrast between European metropole and colonized Other.

The history of the idea of progress, and of the modern/nonmodern contrast, of course goes much further back. There has been a great deal written about how these ideas originated in Christian eschatology, in the struggles from the 1300s onward between the canons of medieval Scholastics against the Latin classics touted by Renaissance humanists, and again in the famous battle during the 1600s between the "ancients and the moderns" in literature and science. The beginning of the European world system of overseas conquest and economic penetration, beginning in the late 1400s, brought an increasing awareness of non-European societies; I would suggest that European intellectuals for the most part incorporated this material into the framework of ideas already developed. There is of course some merit to Connell's point; writers following Condorcet developed the idea of progress into a theory of universal history by incorporating tribal societies into the European modernists' notion of sequence. The more general problem is how much influence we should attribute to materials from the overseas empires; Connell polemically would like to have all of it.

Connell puts a better face on contrary evidence by the rhetorical move of conflating sociologists' comparisons to historically earlier societies (ancient Greece, Israel, Rome, medieval Europe) with comparisons to subjugated colonial peoples. An alternative sociology of knowledge would explain the rise of these historical concerns by institutional changes within the European intellectual community. The rise of professional historiography in the 19th-century universities was a key part of the background of sociology, and the first analytical breakthroughs came largely from increasing sophistication about what ancient societies were really like. Earlier scholars had portrayed the Greeks and Romans as heroic alabaster statues; it was recent studies of class conflict in these ancient civilizations that enabled the young Marx to write a dissertation on ancient Epicureans and to formulate the theory presented in *The Communist Manifesto*. Durkheim's main teacher was Fustel de Coulanges, who pioneered in un-

covering the ancient property system anchored on the religious rituals of dominant households; it is from Fustel that Durkheim got his most striking ideas about ritual as the mechanism of social solidarity. Weber's most important teachers included Mommsen, the historian of Roman class warfare, as well as the leading experts on legal and economic history of Mediterranean antiquity and the European middle ages. Sociology made its first important analytical steps beyond simplistic concepts of historical progress because a second and third generation of intellectuals were trained in this accumulating knowledge of Europe's history. Marx could now point out processes of economic domination and conflict, Durkheim, processes of moral and symbolic solidarity, and Weber, the multisided mixtures of material, organizational, and ideological conditions of action. These theories were analytically sophisticated because they recognized that such processes occur, in varying combinations, within all human societies. The simplicities of earlier philosophies of progress were overthrown by a paradigm revolution made possible by theorizing the work of several generations of professional historians; and that in turn, rested upon the organizational transformation of the 19th-century university.

Connell ignores the analytical contents of theories, because his polemic concentrates on showing sociology at its worst. For example, Durkheim's *Elementary Forms of the Religious Life* is not an evolutionist tract about the superiority of modern colonialists over backward natives. It is an explicit analysis of tribal rituals as the archetype of moral integration and symbolic concept-formation within all societies and treats Australian aborigines as exemplars of what is most deeply human. Of course, one may also find genuine Eurocentric biases in sociological writings; there are some of them here and there in Durkheim, Weber, and Marx, but it is much easier to document them in sociologists who were much less influential in the profession. Charles Letourneau's 1881 ethnography of racial differences, like Henry Hughes's sociology in the 1850s defending the slave system of the American South, make shocking examples; ironically, these are more important figures for contemporary guilt seekers like Connell than they were for the historical development of sociological theories and research.

Connell treats the substantive contents of sociology only so far as to claim that the comparative method is simply the imperial gaze. The trouble with this assertion is that sociologists were hardly the only, or the first, intellectuals to use this method. Comparisons were already central in research sciences such as chemistry. The comparative method was analytically regarded by many sociologists as the equivalent of laboratory experiment, a point explicitly made by both John Stuart Mill and Durkheim. A different kind of comparative method was inspired by neo-Kantian philosophy, exploring the formal properties of the several realms

of phenomenal experience; this philosophical program, anchored in the internal politics of German universities in the late 19th century, was the direct stimulus to the typologizing activities of Tönnies, Weber, and Simmel. These comparative approaches could of course be applied by sociologists to the abundance of new materials that came in with the growth of the colonial empires, but once again it was the extension of intellectual patterns already developed in nonimperialist contexts.

Ironically, Connell's picture of the development of sociology suffers from what might be called an Anglocentric gaze. Britain was the main imperial power until 1945 and thus was most oriented toward colonial peoples. But England was not central in establishing the discipline of sociology nor indeed in laying down its main ideas. I think Connell is misled here because he based this study on a collection of 120 sociology textbooks, 1896–1996, all but two of which were published in the United States, Britain, or the former British Empire. Evolutionist ideas were prominent in American sociology until the 1920s, but this was because of American provincialism and intellectual dependence upon Britain. The United States only began to become an independent player in the world intellectual scene at the turn of the 20th century, when the old religious colleges were reformed into research universities along lines pioneered in Germany 100 years earlier. The leading American intellectuals, including the sociologists who launched a more sophisticated period of theorizing and research, were academics like Park and Mead (and also W. E. B. DuBois) who had sojourned in Germany. American sociology was evolutionist (and therefore in some measure influenced by imperialism) during the time when American education remained under British influence; the break from evolutionism coincides with the importation of non-British ideas, initially from Germany.

The thesis of the imperialist origins of sociology works best for Britain, where there is at least some correlation in time. But it is hard to make a case that sociology originated in England, either as an organized discipline or as a set of intellectual innovations. Sociology was much more of a French development (taking off by the time of Saint-Simon's generation), joined by a network of German thinkers crystalizing around the time of Tönnies. French and German intellectuals did not generally follow the lead of English ideas; even in biology, Darwinian evolution was not accepted in France. For France and Germany, the correlation of sociology with imperialism is poor. Germany had very little overseas empire to be concerned with. The political concerns that did affect 19th-century intellectuals were mainly connected with the unification of the German-speaking states; this is one reason that German scholars focused on the history of language and the uniqueness of national cultures, and the most influential schools repudiated British-style evolutionism and biological reductionism.

France became an important imperial power largely after 1880, so there is some correlation in time with the Durkheim generation. But French imperialism fails to account for the earlier generations (Saint-Simon, Comte, Le Play) in which sociology was founded. As a general theme, the claim that imperialism leads to sociology is vastly overstated. The failure of sociology to develop in the earlier great imperial powers (Spain, Portugal, Holland) shows that other factors must be involved.

In point 2, Connell claims that "race, gender, and sexuality were core issues in sociology" (p. 1521) during this golden age of out-front imperialists. If Connell wants to humble us still further with the argument that, if they were chauvinists, at least they did not hide from the issues the way the rest of us have, then this will have to be more than impressionistically documented. The fact that Tönnies and Durkheim occasionally applied their analyses to gender does not make it their central theme. Again there is conflation: the concern of the 19th-century comparativists with sexuality was not the kind of concern for the imposition of gender patterns spurred by late-20th-century feminism.

Why writers like Sumner were so concerned with sexual exotica can be explained sociologically, but the important conditions were something other than imperialist voyeurism. The late 19th century was the period when the church/state conflict was being fought out on intellectuals' home base. Durkheim participated in the struggle to declericalize the French school system; this was why the issue of religious versus secular morality and solidarity was so central for him. In Germany, the *Kulturkampf* after the 1871 unification of the Protestant north with the Catholic south set the stage for the religious concerns of Nietzsche and Weber. In the United States, one of the most famous leaders of the battle to secularize the colleges was Sumner, who quarreled openly with the president of Yale, the most conservative proponent of the old religious education. Sumner's desire to relativize the moral restraints of puritanical Christianity motivated him to publicize the wide variety of non-Christian ethics and sexual moralities and to ransack the anthropological literature on tribal societies for materials.

It is probably true that sociological attention to gender and sexuality went through a trough in the first half of the 20th century, until the revival of interest by the feminist movement in the 1970s. Connell's explanation of this is weak; to say that interest in gender rose and fell because of the rise and fall of imperialism does not ring true. In the case of sociological concern for race, very likely he is wrong about the trend. Sociologists have been strongly interested in race, from at least the late 19th century continuously through the present. Connell seems to project the concepts of today's political alliances back onto the past; if there is a coalition of people of color with women and gays today, he would like to claim that their

issues always rose and fell together in the past. It is trendy political sloganizing, but it probably is not true.

For point 3, the claim that evolutionism broke down because of World War I is ad hoc. The logic of the previous argument is that imperialism produced the idea of evolutionary progress. But imperialism did not break down in World War I, and in fact the European empires expanded thereafter; the height of empire was the 1920s and 1930s. The intellectual shift during this time tells against the imperialism argument.

Connell has no real explanation of why evolutionism broke down but in effect simply notes that it did. Again this is just the Anglo-American story, since it is convenient for Connell's story line to ignore the sociology of France and Germany in the 1920s and 1930s. Here he seems to be taken in by his own canon seeking, as if, after Weber and Durkheim are dead, there is no reason to pay attention to those countries. In fact, there was a network of French researchers and theorists stretching directly from Durkheim's nephew Mauss through Lévi-Strauss and Aron to Bourdieu. French sociology has never been intellectually dependent upon the United States, but it simplifies matters for Connell to assume that it became so. For Germany, the devastation of the Nazis makes the question more complex, but here too it is simplistic to assume that the story line is the downfall of evolutionism and the adoption of an American canon.

Ironically, although Connell has repudiated the idea that sociology originated in the problems of urban industrial society, he tacitly embraces the notion that American sociology in the 1920s focused on exactly these questions, although he tries to hide the point by renaming them "social difference and social disorder" (p. 1535). It would be more accurate to admit that these have been perennial themes.

Finally, in point 4, Connell documents that a canon was established in mid-20th-century America, but the explanation is vague. He asserts that the upsurge of empirical research ended in "a severe deficit of legitimacy" (p. 1537), but there is no evidence for this. One could argue the contrary, that empirical sociology was highly regarded for its insights into contemporary social problems, that research was booming in the 1940s and 1950s, and the academic discipline was expanding. The creation of the canon was carried out by rival movements of theorists led by Parsons, Merton, and C. Wright Mills, who bolstered their positions by translating European texts and making personal alliances with European scholars fleeing the Nazis. But the formation of these theoretical camps never undermined empirical research on urban social problems, race and ethnic divisions, and similar issues that have been the main activity of most American sociologists throughout the 20th century.

At this point Connell's argument runs out of ideological steam. He does not appear to have much of an idea what a theory of canonicity would

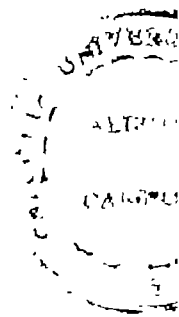
be. He seems to think it is surprising that the "classic" thinkers focused upon after 1950 were nowhere near as dominant among their contemporaries. In fact this is quite typical of many intellectual fields, such as of philosophy; the number of thinkers in the center of intellectual attention is always much greater in their own lifetimes, and the winnowing down to a few great reputations does not usually occur until about the third generation later. Hume, Kant, and Hegel were surrounded by many names famous among their contemporaries and became canonical objects of study only after a period of eclipse at their death, followed by a later revival. The fact that canons have appeared in many different disciplines, and at many historical periods, undercuts the notion that there is something anomalous about English-speaking sociologists creating a canon in the mid 20th century.

If we are really interested in the sociology of canonicity, rather than in polemicizing about it, these historical patterns open up an opportunity for serious work in the sociology of knowledge. Contributions have been made by Charles Camic, George Ritzer, Norbert Wiley, and others on the rise and fall of sociological paradigms and reputations in American sociology. What we need is to broaden out to the formation of canons in other disciplines and in other parts of the world. Even restricting ourselves to sociology, it is not at all clear that France or Germany have had the same sociological canons as the United States, let alone in all historical periods. Certainly one cannot establish this from Connell's list of sociology textbooks, only two out of 120 of which were originally published in France and none in Germany. The French intellectual world has a different institutional structure and hence a different intellectual history. For a refreshing change of viewpoint, I recommend Johan Heilbron's *The Rise of Social Theory* (Oxford: Polity Press, 1995). Its French-centered sociology of the creation of disciplinary discourse is a useful antidote to the Anglocentrism that most of us English-speakers, even Connell, assume all too naturally. Perhaps some day there will be a genuinely cosmopolitan account.



# Blood and Debt: War and Taxation in Nineteenth-Century Latin America<sup>1</sup>

Miguel Angel Centeno  
*Princeton University*



Using data from 11 Latin American countries, this article challenges the universality of the positive relationship between war and state making. Availability of external resources, state organizational capacity, and alliances with social actors are shown to help determine the political response to armed conflict. Overall, the article emphasizes the importance of causal sequence in determining the effect of war. War did not make states in Latin America because it occurred under very different historical circumstances than during the European "military revolution." Without the prior establishment of political authority and without a link between such an organization and social actors, war will not contribute to institutional development.

## INTRODUCTION

The notion that war supports the institutional development of the state is widely accepted in political sociology (Andreski 1954; Finer 1975; Tilly 1975, 1990; Downing 1990; Porter 1994). This is not a recent discovery but reflects the importance assigned to war by Weber (1950) and Hintze (1975). Wars help build the institutional basis of the modern state by requiring a degree of organization and efficiency that only new political structures could provide. Charles Tilly has best summarized this process with his statement that "war made the state, and the state made war" (Tilly 1975, p. 42). Using data from 11 Latin American countries, this article elaborates the causal links between war and state building while challenging the universality of a positive correlation between the two. It

<sup>1</sup> I would like to thank Jeremy Adelman, Paul DiMaggio, Frank Dobbin, Susel Gonzalez, Paul Gootenberg, Deborah Kaple, Jay Kinsbruner, James McPherson, John Markoff, Michael Mann, Charles Perrow, Sean Rourke, Patricio Silva, Arthur Stinchcombe, Barbara Tenenbaum, Charles Tilly, John Womack, Viviana Zelizer, the staff of Firestone Library, and several *AJS* referees for their assistance. The project was partly funded through a Summer Fellowship from the NEH and generous support from Princeton University. Direct correspondence to Miguel Angel Centeno, Department of Sociology, Princeton University, Princeton, New Jersey 08544.

finds that the availability of external resources, the extent of prior institutional development, and the nature of class alliances supporting the state all play critical roles in defining the political influence of war.<sup>1</sup>

Latin America offers an excellent opportunity to test the viability of standard accounts of the relationship between wars and states. Between 1810 and 1830, 16 independent nations arose and began the development of the administrative, ideological, and fiscal bases of modern states.<sup>2</sup> This period of state formation offers an invaluable and as yet largely unexplored arena for testing theories of state making.<sup>3</sup> The study of these often neglected cases may allow for a better appreciation of the exceptional circumstances that led to the creation of that combination of coercion and capital that is the modern state. Not only do these cases expand the relevant sample, but they also allow us to determine if the specific postcolonial conditions under which these states initially developed helped determine their further evolution. In this way at least, the Latin American experience may be much more relevant for contemporary analysis than Western Europe in the early modern era.

### European Paradigm

The key to the relationship between war and state making in Western Europe is what *Finer* (1975) calls the "extraction-coercion" cycle. We begin with the obvious fact that wars require capital: By the 16th century, war became so expensive as to require the mobilization of an entire country. Professional armies clearly outperformed any rivals, but these required "ample and continuous amounts of money" (*Howard* 1976, p. 37). These changes causally linked military and political development. On the one hand, states penetrated their societies in increasingly complex forms in

<sup>1</sup> This article is part of a larger project analyzing the origins and consequences of war (and the lack thereof) in Latin America.

<sup>2</sup> My cases include all the countries in four of the five geopolitical zones in Latin America: La Plata Basin (Argentina, Uruguay, Paraguay, Brazil), Pacific Littoral (Chile, Peru, Bolivia), Northern Andes (Ecuador, Colombia, Venezuela), and Mexico. I have not included Central America as the addition of these cases would make the management of the narrative impossibly complex. Moreover, in the absence of even remotely reliable data on these countries during the relevant time period, their inclusion would bring little benefit.

<sup>3</sup> The time period I am studying may be described as Latin America's "short 19th century" beginning in 1810 and ending with the 1880s. The validity of this "middle period" between the colonial and contemporary eras remains the subject of debate (*Safford* 1992). For my purposes, it is extremely useful as it contains the vast majority of wars experienced on the continent and all but one of its major international conflicts.

order to obtain resources. The organizational innovations that occurred during wartime did not disappear with peace but often left an infrastructural residue that Ardant (1975) calls the "physiology" of the state. On the other hand, the new form of the post-Westphalian state was particularly well suited to the organizational task of managing this penetration and channeling the resources thus obtained into "productive" violence directed at some external enemy. Thus, wars both built and were an expression of political power.

### A Gauge of Strength

How can we compare relative rates of political development and measure the effect of war on these? Taxation is a reasonable measure of such capacity or institutional development. Taxes both represent and augment the strength of the state as measured by the capacity to enforce centralized rule on a territory and its population (Peacock and Wiseman 1961; Organski and Kugler 1980). Taxes partly determine the very size of states' institutions and shape relationships between these and society; they help mold the eventual form of the state (Tilly 1975; Ardant 1975; Schumpeter 1954; Gallo 1991; von Stein [1885] 1958; Levi 1988). War is widely perceived as increasing the capacity of a state to tax its population (Peacock and Wiseman 1961; Mann 1986, 1988, 1993; Tilly 1990; Rasler and Thompson 1989; Campbell 1993). Wars generate greater needs for resources while also providing temporary declines in the state's social constraints. Wars also provide an organizational focus around which the state's organizational capacity may improve. Finally, armies raised for war might also serve as a means with which to collect resources.

The evidence for the positive link between war and the rise of taxes in early modern Europe is exhaustive (Ames and Rapp 1977; Mathias and O'Brien 1976; O'Brien 1988; Stone 1994; Brewer 1989; Aftalion 1990). The pattern is also obvious in the United States (Bensel 1990; Skowronek 1982; David and Legler 1966; Hooks and McLauchlan 1992). In all these cases, we see not only that state revenue increases after war but that the structure of taxation also changes. For example, wars led both the British state in the 18th century and the American in the 19th and 20th centuries to both increase the amount of revenue (which never returned to prewar levels) and to increase the relative importance of domestic and direct taxes (see figs. 1–2). Military conflicts allow (and force) the state to depend less on the administratively simple, but inelastic, custom taxes and to rely on the more politically challenging, but potentially more lucrative, domestic sources of revenue. The greater bureaucratic complexity required is at the heart of the institutional legacy of war.

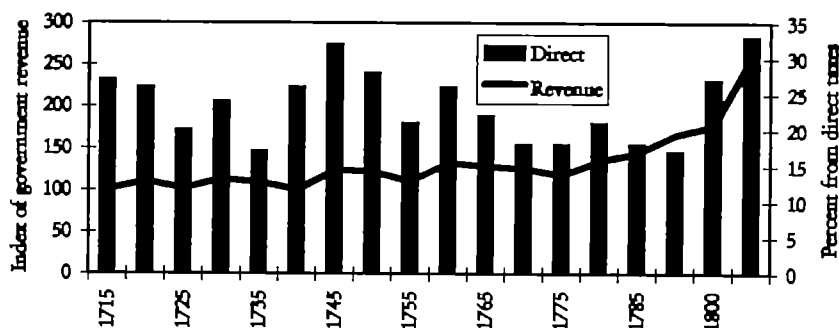


FIG. 1.—U.K. government income (data from Mathias and O'Brien [1976])

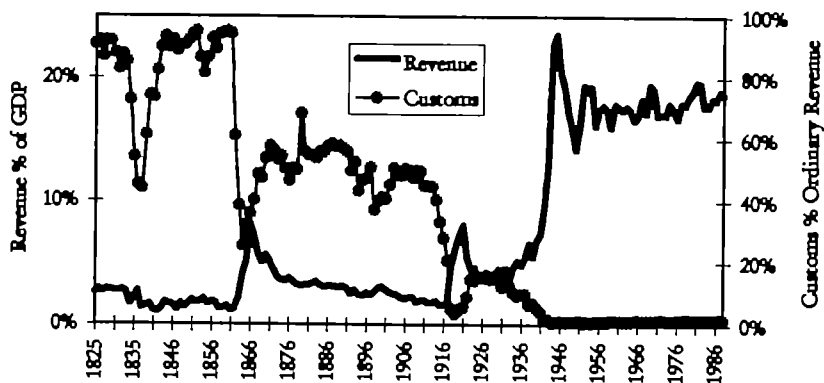


FIG. 2.—U.S. government income (data from Mitchell [1983])

### Challenging the Paradigm

How automatic is the relationship between war and increased state strength? Appreciation of historical specificity and structural conditions is vital for the production of truly generalizable models of state development. Only some wars built states, only some states were built by wars. The European experience indicates that warfare in and of itself does not necessarily lead to state making. Several centuries of prior warfare had not produced states in Europe before the 16th century. Rather, as Tilly (1990) has emphasized, particular circumstances found in parts of Europe between 1600 and 1800 promoted conflict-led state development. What happened to change the institutional contribution made by war?

To an extent, this transformation remains a historical black box. We possess myriad references to the rise of the modern state and countless monographic descriptions of the specific historical sequences. Yet, political sociology has generally failed to produce a coherent model of how violence was transformed into order. Following the methodological logic of the counterfactual, the failure of Latin American wars to generate similar state-building forces as seen in Europe after the 17th century can serve to improve our understanding of the relationship between conflict and institutional development. The findings from this exercise could then be used to explain both geographic and temporal variation outside of Latin America.

The Latin American cases suggest that there are three critical prerequisites for institutional development aided by war. First, the relevant states must be forced to turn inward in order to meet the financial challenges of war. Second, adequate administrative mechanisms must be in place to manage the explosion in both revenues and expenditures. Third, the central state must have already established sovereignty over its territory and must be supported by enough local actors as to make domestic extraction profitable.

These three factors may also help explain why only those European wars following the military revolution contributed to the development of modern political structures. While to an extent these changes were technologically determined by the greater expenses involved in post-17th-century warfare, the important break had more to do with the social and economic contexts in which the wars took place than with what happened on the battlefield (Kaiser 1990; Tallett 1992; Wallerstein 1974–80).

For the “extraction-coercion cycle” to begin, the relevant states must not have alternative sources of financing while the domestic economy must be capable of sustaining the new fiscal and bureaucratic growth. Conflict-induced extraction will only occur if easier options are not available. Even then, the relevant societies might not be able to produce enough surplus to make the effort productive. Thus, for example, the availability of Latin American silver and the willingness of bankers to risk massive sums freed the Spanish Hapsburgs from imposing greater fiscal control over their provinces as a means to pay for their wars. Conversely, the relative scarcity of such external supports drove the expansion of the early English state.

Sociological and comparative accounts of the relationship between war and state building have also not sufficiently emphasized historical order in their analysis. There is a causal ambiguity in Tilly’s famous aphorism: Which came first, states or wars? The Latin American cases suggest that wars in and of themselves do not make anything. Rather, they merely

provide a *potential* stimulus for state growth.<sup>5</sup> Wars can only make states if they are preceded by at least a modicum of political organization. Without institutional cohesion, wars will make for chaos and defeat. Wars provide an opportunity for those political organizations that are able to capitalize on it; they cannot create them. The consolidation of central authority and the creation of a modicum of a bureaucracy appear to have preceded the state-making stage of war in England, France, and Prussia. The venality of the Spanish bureaucracy and the financial leakage of tax farming in a variety of other countries represented critical obstacles to state development.

Finally, it is important to appreciate fully the social resistance that may be offered to state penetration. The combination of coercion and capital symbolized by the military draft and direct taxation (the defining characteristics of a war-made modern state as per Tilly [1993, p. 32]) does not come about simply because a bureaucratic apparatus is in place and wishes it so. The capacity of a state to extract resources will be closely linked to the willingness of the population to accept these burdens. Reluctance on the part of an economic oligarchy to part with its cash may make expansion of taxes simply not worth the effort. Thus, state capacity is not an absolute phenomenon but a relational one. It is not merely a question of strength but also of the potential of the relevant societies to resist (or welcome) intrusion. Wars only make states when there already exists some form of union between a politically or militarily dominant institution and a social class that sees it as the best means with which to defend and reproduce its privilege. That is (following Perry Anderson [1979]), there has to be a prior agreement that the state will be responsible for collecting and disposing of social surplus. European cases demonstrate that the fragmentation of sovereignty, be it through the persistence of local autonomies (Spain), powerful but divided aristocracies (Poland), or direct external control (the Balkans), can and does prevent the solidification of states even when surrounded by conflict. In contrast, more successful war-making states established a coalition between central authority and potential aristocratic challengers either through alliance (England) or through coercion (France, Prussia).

#### WARS AND STATES IN LATIN AMERICA

What was the Latin American experience with war and taxation? Despite its almost unprecedented peace during the 20th century, Latin America's

<sup>5</sup> Tallett (1992; citing I. A. A. Thompson's work on Spain) calls war less a stimulant than a test of state strength.

first 70 years were quite bloody. As may be seen in table 1, for most of the 19th century, at least one-third of the relevant countries were at war, securing their independence, fighting neighbors, or establishing centralized control.<sup>6</sup> The expenditure patterns of the Latin American states for which we have a modicum of information also look a great deal like those of the European counterparts during the critical period of state building (see table 2).<sup>7</sup> These were countries *apparently* devoted to war. Expenditures were concentrated on the military and paying the debt derived from war.<sup>8</sup> (The overall decrease in military expenditures after the 1870s does reflect a general decline in interstate violence from the late 19th century on). The exceptions to the pattern tend to prove the rule. The decline in Paraguayan expenditures after 1840, for example, actually reflects a change in the manner of accounting adopted in order to disguise an increase in the attention paid by the state to military development.<sup>9</sup> The low Uruguayan numbers come almost 50 years after formal independence during which the state did little but fight internal and external enemies.

War did have some of the expected results in Latin America. As in Europe, it often led to the destruction of the losing side. In at least three cases (Peru in the 1880s, Mexico in the 1850s, and Paraguay after 1870), war led to the practical elimination of the state as an entity. Among winners (Argentina and Brazil in the 1870s), war led to an increase in the size of government. Wars also provided the expected economic stimulus (McLynn 1984). Military procurement and the slightly higher war tariffs encouraged domestic industrial development in Brazil during the War of the Triple Alliance (Bethell 1987a, p. 768). The need to supply the Chilean expeditionary forces in Peru during the War of the Pacific increased the demand for domestic production of basic products such as textiles and foodstuffs. More factories were founded in Chile between 1880 and 1889 than had existed prior to the war (Loveman 1979, p. 169; Zeitlin 1984, pp. 77–78). Recruitment and increased demand also lowered unemployment (Cariola and Sunkel 1991, p. 42).

<sup>6</sup> I define war as "substantial armed conflict between the organized military forces of political units" (Levy 1983, p. 51) and use Singer and Small's (1982) threshold of 1,000 deaths. I have included both civil and interstate conflicts in my analysis.

<sup>7</sup> I have avoided the use of formal statistical methods for two reasons. First, given the vagaries in the data, formal cross-national comparisons would be deceptive. Second, qualifying any individual year as peaceful or at war would be an extremely subjective process and involve distinctions beyond the purview of this article.

<sup>8</sup> Conflict did not necessarily mean large organized armed forces. Colombia had a very violent 19th century, yet its army was down to 800 men in 1854 and 511 in 1858. The army was in fact almost nothing more than a palace guard well into the 20th century (McGreevey 1971, p. 87). In the 1830s, the entire Venezuelan army consisted of 1,000 men (Bethell 1987a, p. 520).

<sup>9</sup> Thomas Whigham, private communication with the author, July 1996.

TABLE 1  
MAJOR WARS IN 19TH-CENTURY LATIN AMERICA

War	Countries	Year Started
Independence .....	Argentina	1810
Independence .....	Bolivia	1810
Independence .....	Chile	1810
Independence .....	Bolivia, Colombia, Ecuador, Venezuela	1810
Independence .....	Mexico	1810
Independence .....	Paraguay	1810
Independence .....	Peru	1810
Provincial autonomy struggle .....	Argentina	1819
La Plata .....	Argentina, Brazil, Uruguay	1825
Independence .....	Central American republics	1821
Buenos Aires troubles and civil war .....	Argentina	1828
Civil war .....	Chile	1829
Mexico wars and revolts .....	Mexico	1829
Revolution of the Farrapos .....	Brazil	1831
Buenos Aires .....	Argentina	1833
Conflict over Texas .....	Mexico	1835
Argentina-Uruguay war of La Plata .....	Argentina, Brazil, Uruguay	1836
Chile-Bolivia War .....	Bolivia, Chile, Peru	1836
Guerra Grande .....	Argentina, Brazil, Uruguay	1838
Civil war .....	Colombia	1839
La Paz .....	Peru	1841
Mexican-American War .....	Mexico	1846
Civil war .....	Chile	1851
La Plata war .....	Argentina, Brazil, Uruguay	1851
Civil war .....	Peru	1853
Civil war .....	Colombia	1854
Civil war .....	Mexico	1858
La Plata .....	Argentina	1859
War of the Cauca .....	Colombia	1859
Franco-Mexican War .....	Mexico	1862
Civil war .....	Ecuador	1863
Northwest province instability .....	Argentina	1863
Triple Alliance .....	Argentina, Brazil, Paraguay, Uruguay	1864
Peruvian-Spanish War .....	Bolivia, Chile, Peru	1865
Ten-Years War .....	Cuba	1868
Civil war .....	Venezuela	1868
Mitre Revolt/Pampas .....	Argentina	1874
Civil war .....	Colombia	1876
War of the Pacific .....	Bolivia, Chile, Peru	1879
Patagonia campaign .....	Argentina	1879
Civil war .....	Argentina	1880
Civil war .....	Colombia	1884



TABLE 1 (*Continued*)

War	Countries	Year Started
Central American .....	Guatemala, El Salvador	1885
Civil war .....	Chile	1891
Rio Grande do Sul .....	Brazil	1892
Civil war .....	Peru	1894
Independence .....	Cuba	1895
Civil war/Bahia revolt .....	Brazil	1896
Secession of Panama .....	Colombia	1898
Civil war .....	Colombia	1899

SOURCES—Singer and Small (1982), Kaye, Grant, and Emond (1985); Bethell (1987a)

At least as measured by taxation, however, the Latin American states did not penetrate or extract from their societies in the expected manner. Comparisons of relative extractive capacities in the 19th century are difficult given the lack of comparable national economic data and the questionable use of official exchange figures. The two countries for which we have the most reliable information that can be translated into an international currency are Chile and Brazil. During the entire period in question, neither Latin American state could extract even half of the revenue per capita available to the British state, arguably the least rapacious European power at the time, despite the fact that these countries experienced considerable conflict (Flora 1987; Mitchell 1983; Dirección de Contabilidad 1914; Buescu 1984; see fig. 3). Moreover, both of the Latin American countries were much more dependent on customs revenues than the United Kingdom during this time period. While such taxes accounted for roughly one-third of British revenues and were marginal for France, in Brazil and Chile, they represented at least two-thirds and often more (Dirección General de Contabilidad 1914; Buescu 1984; Flora 1987; see fig. 4).<sup>10</sup> Thus, the upward trend in tax receipts of the Latin American coun-

<sup>10</sup> The relative fiscal "weakness" of the Latin American state measured both in terms of money produced and the means used continues to the present day. In 1970 (prior to the debt boom-and-bust cycle), e.g., the Latin American average of government revenue as percentage of GDP was 13.6; for the Organization for Economic Cooperation and Development (OECD) it was 23.1%. While Latin American countries depended on customs for 22.3% of revenue, the OECD figure was 5.6%. See Kling (1959) for a discussion of the implication of customs dependency. Kiser, Drass, and Brustein (1995) imply that customs may serve to protect the autonomy of a ruler since these make the central state less dependent on social sources of revenue. I wish to emphasize the other side of the same equation. The presence of social extraction implies that the state *can* demand social resources.

TABLE 2  
COMBINED MILITARY AND FINANCIAL EXPENDITURES AS PERCENTAGE OF BUDGET

	Argentina	Brazil	Chile	Ecuador	Mexico	Paraguay	Peru	Uruguay	Venezuela
1820						81			
1821									
1822	92				96	80			
1823	93					85			
1824	85				81				
1825					95	84			
1826	98				66		84		
1827					91				
1828		90			92				
1829	90	88			94	89			
1830	85	85		74	93				
1831	100	82			92	86	70		83
1832	74	62		57	91	87			79
1833	78	88	57		81	85			84
1834	78	86	58		70	76			80
1835		86	60		90	85			83
1836		85	56		80				41
1837		86	51		93	92			65
1838		87	48			94			73
1839		88	47	58	86	89			68
1840	98	84	41		88	89			53
1841	96	86	34		85				64
1842	100	83	83		95				72
1843	100	82	83		94				57
1844	98	81	73		93	43			73
1845	98	79	72		98				69
1846	93	78	70	77	93	81	51		63
1847	94	78	72				58		66
1848	95	79	71		89		66		84
1849	93	77	71		90		74		72
1850	95	78	70		40		75		76
1851		81	77		55		76		83
1852		77	73	74	86		82		83
1853		76	66	60	85	56	83		82
1854		74	60	62	73		85		35
1855		72	53	61	84		86		32
1856		74	55	62		27	87		
1857		74	57	60			88		
1858		70	44			40	84		83
1859		70	68				79		
1860		68	67		57	32	77		74
1861		71	66	80	75		74		
1862		72	64	62			80		
1863		72	67	69			73		
1864	80	73	70	62			72		
1865	88	84	71	57			71		94

TABLE 2 (*Continued*)

	Argentina	Brazil	Chile	Ecuador	Mexico	Paraguay	Peru	Uruguay	Venezuela
1866 .....	73	83	79	66			69		79
1867 .....	72	87	77	58	85		60		
1868 .....	64	86	67		87		47		
1869 .....	66	84	70	54	65		57		
1870 .....	71	72	64	38	55		57		
1871 .....	63	69	57		55		57		93
1872 ....	88	69	58	51	51		58		
1873 .....	82	67	57		51		53		60
1874 .....	80	67	52		49		57		82
1875 .....	54	65	54	61	70		74		67
1876 .....	51	62	60		76		78		
1877 .....	61	52	67		63		57		
1878 .....	58	43	64	28	69		61		77
1879 .....	54	57	78		82		53		
1880 .....	57	62	68						69
1881 .....	49	62	76						
1882 .....	56	61	78						
1883 .....		58	74						45
1884 .....		57	68	43				35	29
1885 .....		61	64	25				35	46
1886 .....		59	72	41				32	
1887 .....			68	50				31	
1888 ...			44	47				32	
1889 ...			41	35				31	
1890 .....			37	40					

NOTE.—For Paraguay, the figures are military expenditures alone. Apparent drop after 1840 reflects changes in government accounting that appear to disguise continued military expenditure.

SOURCES.—Cortés Conde (1969), Burgin (1946), Halperin-Donghi (1982), Buescu (1984), Dirección de Contabilidad (1914), Rodríguez (1985), Aguilar (1940), Arbulu (1983), Reber, n.d., República de Uruguay, various years, Camillo Batalla (1969).

tries during the late 19th century reflects increased connection to the global economy, not a stronger state.

Even if the state grew (and this was not universally true), it did not develop the fiscal musculature associated with the warring state.<sup>11</sup> The stimulus of war did not produce the dramatic increase in the institutional complexity of extraction associated with the theoretical model. Despite

<sup>11</sup> No matter whether at war or peace, Colombia had one of the lowest levels of government expenditure per capita in Latin America (Tovar 1988, p. 115). In 1871, Colombia gathered one-half of Mexico's revenues and one-fifth of those of Chile. During the same period, one local authority estimated that the government received only 2% of the national product (Deas 1982, pp. 289, 310, 326). See also Ocampo (1987).

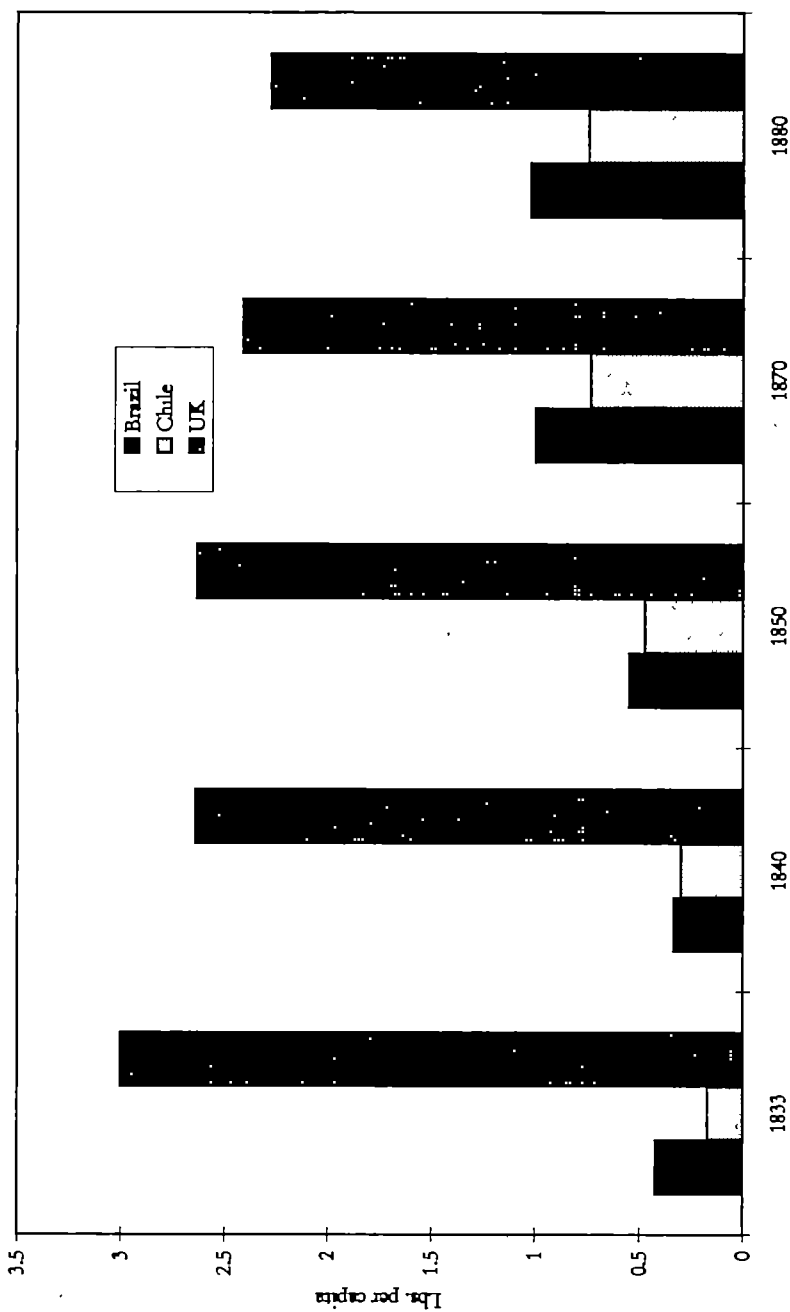
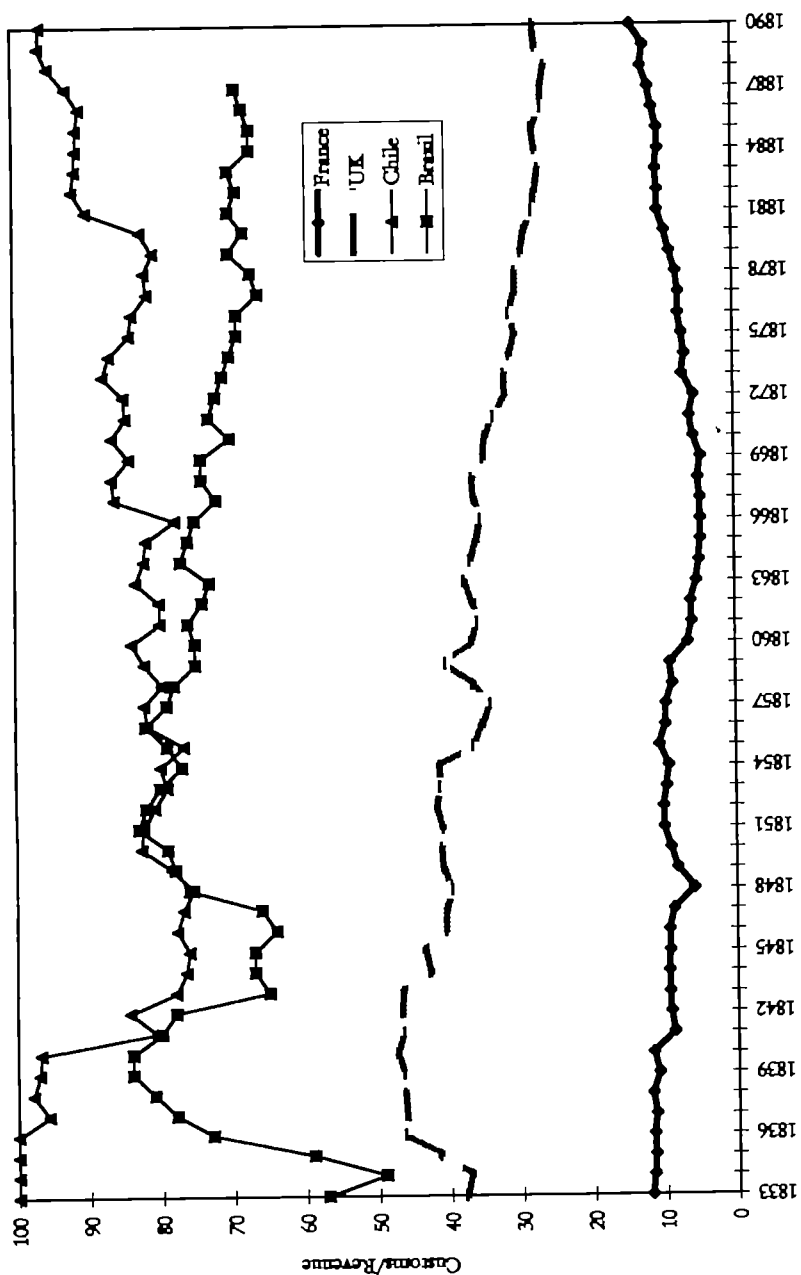


FIG. 3.—Fiscal capacity in the 19th century (data from Dirección de Contabilidad [1914], Buescu [1974], and Flora [1987])



rises in expenditures, the revenues lagged far behind. As in the European cases, war produced immediate deficits, but with one prominent exception, the Latin American states did not respond to these with increased extractions, at least not in the form of domestic taxes. Customs and royalties from the export of primary goods remained the mainstay of most Latin American states (see table 3).

The internal and external wars experienced by the Brazilian Empire did not elicit the kind of fiscal intrusion seen in Western Europe or even the United States.<sup>12</sup> Overall, taxes on wealth and production contributed less than 4% of ordinary revenue even during the war years (Buescu 1974, pp. 89–91; Cavalcanti 1989, p. 326). In Chile, domestic taxes played an even smaller role. The most significant was a colonial tithe used to support the Church that represented less than 3% of all revenues by the 1860s. Early Argentinean governments did attempt to impose a capital tax (*contribución directa*) with rates of 1%–8%, but this never functioned and was not obeyed (Alemann 1989, p. 61). Land rents never accounted for more than 3% of total receipts (Rock 1987, p. 99). In no case do we see any relationship between war (either internal or external) and the expansion of the fiscal penetration of the state.

Three important partial exceptions to the general trend serve to clarify the pattern. Mexico's relatively low dependence on customs receipts reflects continual state dependence on domestic loans and the printing press. The central government was generally incapable of imposing domestic taxes prior to the Porfiriato. On the other hand, Bolivia did maintain the oppressive colonial Indian tribute and relied on these sources for a large part of its revenues through the mid 19th century. Two aspects of this tax bear notice. First, it was not sensitive to the stimulus provided by external conflict but rather reflected internal caste divisions. Second, such a tax did not rely on central government infrastructural development but rather on the retention of special social privileges and localized fiefdoms.

The third exception is the most interesting. Because of its location and the almost continual threats to its existence as well as the ideological proclivities of the dictator José Francia, Paraguay could not rely on external financing for much of its early history. That is, unlike other Latin American countries, it could not count on either customs or loans to balance its books but had to live on its own resources (Pastore 1994b, p. 539). The state autofinanced itself through sales to soldiers and the populace as well as through confiscation of property. The structure of Paraguayan finances

<sup>12</sup> During the War of the Triple Alliance, a tax was instituted on buildings with a 3% surcharge on their value, but this excluded the most valuable rural properties. There was also an attempt to establish a tax on industry and the professions, but this was never enforced and the rates remained minimal.

TABLE 3

## CUSTOMS AS PERCENTAGE OF ORDINARY INCOME

	Argentina	Bolivia	Brazil	Chile	Colombia	Ecuador	Mexico	Paraguay	Peru	Uruguay	Venezuela
1820								18			
1821											
1822	84						54	2			
1823								9			
1824	80						41				
1825							55				
1826							46				
1827		13					42		33		
1828			50				45	0			
1829	85		32				34	0			
1830	87		29			51	45				39
1831	83	15	29					1	52		63
1832	82	11	33		30	43	37	2			55
1833	87	12	57	51			42	0			52
1834	76		49	56		51	49	0			49
1835		9	59	56			20	1			52
1836		9	73	57	33	57	21				61
1837		10	78	57		57	25	0			50
1838			81	57		57	25	1			59
1839		7	84	57		57	19	3			65
1840	70		84	62	32		35	6			74
1841	93	7	80	59			25				88
1842	92	7	78	60			17				73
1843	90	6	66	55			22				62
1844	90	6	67	51			23	9			68
1845	89	9	67	53							67

TABLE 3 (Continued)

	Argentina	Bolivia	Brazil	Chile	Colombia	Ecuador	Mexico	Paraguay	Peru	Uruguay	Venezuela
1846	69		64	56	21	54		5	35		69
1847	84	7	65	53		60			40		57
1848	90	7	76	49		62	25		44		33
1849	93	9	77	51			35		37		45
1850			79	57	47			4	53		49
1851	93		84	57					57		57
1852		7	82	57		60			74		72
1853		11	80	43		59	45	6	82		66
1854		9	77	60		59	50		86		77
1855		10	79	56	52	59	43		91		74
1856		9	81	50		61		11	91		74
1857			79	58		61		22	95		74
1858			78	51		65		18	94		53
1859			76	41					86		63
1860			75	56		70		25	94		56
1861			76	49		59			95		62
1862		10	74	43		52			86		60
1863		15	73	47	32	50			56		57
1864	81		78	36		50			62		74
1865	92		76	23	21	50			74		75
1866	100	5	75	18		64			87		80
1867	86	6	72	28	40	62	53		70		89
1868	95	4	74	47		77	53		72		



1869	97	5	74	45	73	48	47	99
1870	94		70	34	73	53	75	
1871	100	12	73	45	64	45	96	
1872	83		72	53	45	45	62	76
1873	83	35	71	33	52	48	76	85
1874	80		70	50	59	47	94	79
1875	72		69	38	69	63	72	80
1876	89		70	40	69	57	73	74
1877	96		66	36	62	44	85	57
1878	82		67	37	64	36	90	
1879	72		69	25	49	36	85	75
1880	79	42	68	34	61	72		71
1881	89	31	70	58	61			70
1882	78	26	69	70	68	75		65
1883			70	65	63	70	53	72
1884			67	67	68	68	57	70
1885			67	62	57	57	50	63
1886			68	39	62	62	57	69
1887				44	74	74	60	73
1888				71	68	68	59	70
1889				66	61	61	61	74
1890				65	80	80	55	76

NOTE.—Ecuador figures are for indirect taxes largely consisting of customs, a category that includes royalty payments for minerals.  
 SOMALIA.—Cortés Conde (1989), Burgin (1946), Halperin-Dougli (1982), Grieshaber (1977), Buescu (1984), Dirección de Contabilidad (1914), DANE (1972-73), Ocampo and Montenegro (1984), Rodríguez (1985), Carnagiani (1983), Artuhn (1983), Reber, n.d., República de Uruguay, various years, Veloz (1984), Caray Angulo (1988).

appeared to change immediately after Francia's death as the importance of customs increased to nearly half of state revenues. Unlike most Latin American countries, however, Paraguay ran a consistent trade surplus during this period (Rivarola Paoli 1982, p. 104). The state remained largely in charge of external trade (mostly of yerba maté) either through its own administration or through the patronage network of the dictators Carlos Antonio López and his son Francisco Solano López (Whigham 1991, pp. 69–70). Direct sales continued to play an important role as well. What is most significant is that Paraguay depended on foreign trade (directly or indirectly) for only 40% of its government revenue (Pastore 1994a, p. 304). Paraguay also did not rely on debt, that other great source of international financing; when the War of the Triple Alliance began, it did not have any external debt (Rivarola Paoli 1982, p. 119.) Given the chaos found in Paraguay during the War of the Triple Alliance, there are no records of how López financed it. It would appear, however, that he paid for the war through the complete mobilization of the country (Williams 1979, pp. 217–21; Pastore 1994a, pp. 306, 318). How much of this was voluntary patriotism and how much it reflected the reach of the state is impossible to tell. The relevant point, however, remains the same: Unlike other Latin American countries, Paraguay autofinanced its war.

Even in these cases, however, wars did not produce the expected institutional transformation in state civil relations or the structure of the political apparatus. The fiscal infrastructure was established independent of interstate conflict and remained relatively constant in peace and war. In general, Latin American states were not able to escape the "cycle of levies, bankruptcies, and mutinies" (Kaiser 1990, p. 35) that characterized warring states prior to the revenue revolution of the late 17th century; they were not "built" by war.

#### EXPLAINING THE LATIN AMERICAN PATTERN

How do we explain the Latin American experience and the deviations within the continent? First, the Latin American states had alternative sources of finance that allowed them to escape the coerced extraction of resources from the domestic economy. More important, the wars came at the wrong time. The Latin American states were not structurally, politically, or ideologically ready to exploit the opportunities presented by war. The birth of the Latin American state, despite being announced by the sound of guns, did not produce the expected form of political apparatus. Liberation from Spain produced a much weaker institution (at least as measured by fiscal structure) and one much more dependent on the international economy. The independence wars also wrecked the economy and indebted the countries, making the rise of the structural equivalent of a

national bourgeoisie much more difficult. Thus Latin America was deprived of both a political and a social anchor on which to base institutional development. Finally, the Latin American state was never able to impose an internal unity required for the extraction process, even in the face of military threats. As strange as it might appear given the oppression so endemic to the continent, the Latin American state may have suffered from an incomplete process of internal domination. In the European cases, representatives of the monarchy, the landed oligarchy, or the newly developing bourgeoisie were either willing to bear part of the burden in order to protect themselves, or were able to impose that obligation on recalcitrant social sectors. In Latin America, control of the state remained in contention.

The following section discusses the availability of financial resources. The second segment analyzes the organizational and political capacity of the state to exploit these. The final part discusses the importance of domestic class alliances.

### Artificial Wealth

It is not clear how much even the most voracious state could have extracted from such extremely poor societies. The independence wars left little base on which to build a state and destroyed much of the economy on which it might have relied. The Mexican economy experienced a significant decline after independence.<sup>13</sup> During the first 40 years of the 19th century, Bolivia saw the decapitalization of its mining industry (In 1840 there were 10,000 abandoned mines) and the depopulation of its cities. Venezuela was "left a wasteland" by a war that was "cruel, destructive, and total" with armies regularly destroying the property of enemies and paying their soldiers with plunder (Lynch 1973, pp. 202–18; Halperín-Donghi 1973, p. 8). The Peruvian economy was crippled by the need to support loyal armies throughout the continent (Lynch 1973, p. 162). The situation was made even worse by the collapse of the continental "customs union" maintained by Spanish mercantilism (Bulmer-Thomas 1994, p. 29). Material goods were not the only things destroyed by the wars of independence. An entire system of economic, juridical, and social relationships was also their victim (Bethell 1987a, p. 307). For most, the wars brought misery, hardship, and penury.

<sup>13</sup> Per capita income declined from 35–40 pesos to 25–30 pesos between 1810 and 1820 and did not attain colonial levels until the Porfiriato of the last quarter century (Bethell 1987b, p. 91). One indication of the fall is Mexico's changing position relative to the U.S. economy. By John Coatsworth's estimates (1978, pp. 82–83), Mexico had 44% of the U.S. per capita income in 1800, but only 13% by 1910, with most of the comparative decline coming prior to 1845.

This did not change during most of the relevant time period. The Colombian economy, for example, was small and undeveloped, while difficult transport constrained the growth of taxable market exchange (Bushnell 1993, p. 76). In general, squeezing the rich did not yield very much because even this social group had relatively small amounts of capital available (Deas 1982, p. 314). In the late 1830s, Ecuador's exports totaled the equivalent of £200,000 (Bethell 1987a, p. 511). Even a completely rapacious state would have produced per capita extractions much lower than in Europe. Those states that did impose a direct tax on their populations could expect little. Even with an onerous Indian tribute, for example, Bolivia's government revenues per capita in the 1840s (following a war against Chile) were one-fortieth of those of Britain (Dalence [1851] 1975, p. 316).

This poverty made it extremely difficult to use excise taxes as a form of income. Aside from relatively few commodities, large parts of the population did not consume very much that could be easily taxed. In any case, the hatred of the colonial *alcabala* would have made it practically impossible for the newly independent governments to have imposed such a tax. Equally important, there were few wage laborers whose income could be measured and taxed in any systemic manner. Even the landed oligarchy, while rich in land, in most cases did not possess large amounts of directly extractable resources.<sup>14</sup> In several cases, the Church did have considerable wealth, but even successful appropriations (as in Mexico) produced disappointing results.

How then did the Latin American states pay for their wars? A common experience (and one not dissimilar to that of the European cases) was to print money. During its war with Brazil in the 1820s, Argentina resorted to currency emissions, which produced a monetary cataclysm: the price of an ounce of gold on the Buenos Aires stock exchange went from 17 pesos in January 1826 to 112 pesos in December 1830 (Burgin 1946, p. 69). During the same war, the amount of Brazilian currency available doubled, and subsequently the reis lost half of its value (Cardoso 1992, p. 105; Barman 1988, p. 140; Nogueira 1988, p. 313). Even more dramatic was the endless printing of money in Brazil during the War of the Triple Alliance. In 1864, there were 29 million milreis in circulation; by 1870, there were 151 million (Castro Carreira [1889] 1980, p. 743). Between 1859 and 1901, the Uruguayan state issued 342 million pesos of which 124 million were still outstanding. To give an idea of the degree of printing insanity, the customs house of Montevideo was producing an average of 10 million pesos per year (Acevedo 1903, 1:457). Thus, the vast majority of

<sup>14</sup> Nevertheless, it is possible to build a considerable state apparatus on the basis of a land tax. See Bird (1977).

Latin American governments resorted to a form of inflation tax in order to pay at least partially for their wars.

This profligate reliance on an inflation tax may represent a possible institutional and administrative legacy of war. Such a tax would require that the central government be able to establish a monopoly on the issue of currency. Thus war would provide an incentive to expand centralized authority in order to be able to tax through a printing press. It may be worthwhile analyzing the link between war and monetary development in Europe. The Latin American experience also would deserve further analysis. This form of taxation also serves as an indication of the relative power of social groups. Inflation taxes would favor rural versus urban populations and exporters versus importers. They were generally extremely regressive (Rock 1987, p. 107; Oszlak 1982, p. 183; Halperín-Donghi 1982, p. 161; Ferrer 1967, p. 61).

States also borrowed from both domestic and international sources. In Mexico, the government increasingly relied on the *agiotistas*. These would provide funds during fiscal emergencies (an almost everyday occurrence as ministers would often find literally empty tills upon assuming office). In exchange for the considerable risk, domestic lenders were often given very favorable terms, with rates in the range of 300%–500% (Bazant 1968, p. 44). Since borrowing was constant and loans were continuously rolled over, the debt ballooned to 102 million pesos by 1840, 120 million in the 1850s, and 165 million by 1867 (López Cámara 1967, pp. 171–72; López Gallo 1967, p. 98).<sup>15</sup> The Argentine government also borrowed: by 1840, the debt stood at 36 million pesos while the total income for that year was 1,710,491 pesos (Halperín-Donghi 1982, p. 213). The response to later wars was no different. Between 1865 and 1876, Argentina acquired almost £19 million of debt (Pomer [1968] 1987, p. 238). By 1885, the figure was £26 million (Randall 1977, 2:215). Currency conversions make comparisons difficult, but this represented at least four times the revenue of the state during these years. The *total* public debt by 1888 was over £60 million and indebtedness per capita tripled (Oszlak 1982, p. 217; Dirección General de Contabilidad 1914, sec. 7). The War of the Triple Alliance brought similar results in Brazil. Real net debt increased from £4.5 million in 1863 to £9.3 million in 1871. Debt as a percentage of exports increased from 58% to 82% during the same decade (Buescu 1974, pp. 119, 126; Nogueira 1988, pp. 378–80; Castro Carreira 1980, p. 429). By the time an independent Uruguay appeared in 1830, the government had already accumulated a debt of 2 million pesos (Reyes Abadie and Romero, n.d., p. 337). The debt continued to spiral. By 1853 (when the first attempt was made to organize

<sup>15</sup> These are considerable sums considering that the metal peso was officially set at practically dollar parity.

and systematically manage public finances), the debt was 40 million pesos. By 1854, the estimate was 60 million pesos and by 1858, it was 106 million pesos. To give a scale for these numbers, the estimate for the annual budget of these years is 2 million pesos (Reyes Abadie and Romero, n.d., p. 339).<sup>16</sup>

In and of itself, however, relying on debt and the printing press does not explain why the Latin American countries did not impose domestic taxes *after* the wars. Many European countries initially used debt to pay for wars and later taxed in order to meet their obligations. What distinguishes Latin America is that the fiscal reckoning never came. Moreover, government debt did not encourage the creation of a *stable* domestic financial market, a critical contribution of war in the cases of Britain and the Netherlands. Rather, government paper fueled unproductive cycles of speculation and ruin. Because of the risk involved, interest rates remained usurious, further hampering domestic development and increasing external dependence.

The availability of external resources freed the state from having to exploit the domestic economy. The relationship between the state and the global economy had three legs: foreign debt, the sale of commodities, and customs.

Much of the debt discussed above was to foreign banks (Rippy 1959; Marichal 1989). From the beginning, postindependence governments sought to supplement their inadequate domestic sources with foreign loans.<sup>17</sup> Unlike the United States, for example, the new countries lacked allies and external aid, which meant that they had to pay hard cash for all the supplies that reached them. By 1820, the Gran Colombian government had already accumulated European debts of £500,000. Chile similarly contracted in 1822 for £1 million in order to buy a navy. Peru also had to obtain loans in order to pay back wages and a bonus to the victors of Ayacucho as well as some loans to the Gran Colombian government. Mexico also borrowed £2,500,000 in 1824, 65% of which went to direct military expenses. Argentina also borrowed in London beginning in 1824. Most of these proceeds went to the creation of a domestic financial system which was largely destroyed by the Argentine-Brazilian War of 1826–28

<sup>16</sup> Percentage of GDP or of export income would obviously be much better measures of the relative size of these debts. Numbers on the size of the national economy even well into the 20th century are notoriously unreliable. Export revenue and debts are often expressed in different currencies (e.g., paper vs. gold, or in different international currencies) making a more precise measure practically impossible.

<sup>17</sup> These included some intracontinental debts. These usually represented payments for armies or military materials during the independence struggles. By the late 1820s, Bolivia owed Peru 725,000 pesos, Peru owed 6 million pesos to Colombia and 3 million pesos to Chile (Seckinger 1984, p. 51).

(Marichal, pp. 27–36). The first Brazilian loan of 1824 was used to pay for Portuguese loans from Britain. More money followed in 1825 and 1829. By 1830, Brazil had already borrowed £4.8 million or approximately four times its annual revenues (Cardoso 1992, p. 105). While Latin America was out of the international financial markets for nearly 40 years following this early boom, it made a significant comeback after 1860. For example, during the War of the Triple Alliance, Brazil borrowed £5 million in 1865. In 1867, the state needed another loan from Rothschild for 71 million milreis (Nogueira 1988, pp. 378–80; Castro Carreira 1980, p. 429).

If they could not borrow on international markets (as was the case from roughly 1830 to 1870), Latin American states could sell access to a commodity. Guano allowed Peru to become what Shane Hunt (1973) has called a “rentier state.” The availability of guano revenues retarded the development of the state by allowing it to exist without the remotest contact with the society on which it rested and without having to institute a more efficient administrative machine. Guano did allow the removal of the regressive *contribución* (in 1855), but it also permitted the state to avoid modernizing its fiscal structure while borrowing large amounts of money. A contemporary British observer (Markham 1883, p. 37; my emphasis) noted that “a wise government would have treated this source of revenues as temporary and extraordinary. The Peruvians looked upon it as if it was permanent, *abolishing other taxes*, and recklessly increasing expenditure.” Much like the guano bonanza in the Peruvian case, the conquest of nitrate territories allowed the Chilean state to expand without having to “penetrate” its society and confront the rampant inequality (Loveman 1979, p. 169; Sater 1986, p. 227). By 1900, nitrate and iodine were accounting for 50% of Chilean revenues and 14% of GDP (Mamakakis 1977, pp. 19–21; Sater 1986, p. 275).

Custom taxes also represented an ideal solution to fiscal problems given the organizational ease with which they could be collected. A few soldiers in the main ports could provide considerable income. More important, given their indirect nature, these taxes were the least likely to provoke popular protest (Marichal 1989, p. 17). A revenue tariff was a characteristic feature of a society dominated by landed proprietors who diverted taxation away from property toward the consumer (Lynch 1973, p. 150). A reliance on customs also reflected the sectoral distribution of the continent's economies. For some countries (e.g., Peru), a large share of the national product was concentrated on the export of a commodity. Thus, government taxed that part of the economy that was most visible. Others having a less developed export market would target imports as well as exports. The distribution between these two often reflected the relative influence of importers versus exporters. In the case of Brazil, for example, the heaviest taxes were on manufactured imports. This strategy, in turn,

may have made this form of taxation even more regressive depending on the distribution of goods within the typical import basket.

Argentina is an extreme example of this pattern. From the 1820s, the various incarnations of the Argentine state depended on customs duties for the vast majority of its income. The dictator Rosas continued the policy of allowing customs revenues to replace the more politically costly excise or land taxes (Halperin-Donghi 1982, pp. 242–43). The fragility of such dependence was demonstrated by the blockades of 1827, 1839, and 1846 by European navies, which produced fiscal crises. Despite the massive changes in the Argentine economy during the last quarter of the century (e.g., the fivefold increase in exports from 1862 to 1914), the fiscal system remained largely dependent on import taxes (Oszlak 1982, p. 173). Even by Latin American standards, this dependence is striking as customs often accounted for over 90% of ordinary revenues. Trade taxes were seen as the only way of maintaining some semblance of peace between the various politically relevant factions (Oszlak 1982, p. 186). All knew that this fiscal system was inadequate, but they were also aware that it was the only way of maintaining the social status quo (Gorostegui de Torres 1972, pp. 120–21). Tariffs were particularly attractive to the rural elite. While saving them from any sacrifice, they could also help finance the expansion of the frontier from which they benefited disproportionately (Oszlak 1982, p. 189). Such a fiscal system also required few administrative resources.

The particular links between Latin America and the global markets had important domestic repercussions. First, they often linked the fiscal health of the state to the world economy and the price of a single commodity. In the well-known pattern, declines in trade or demand for a good could halve government receipts in a single year. Long-term planning and investment were impossible. Depending on the state apparatus as a political patron was also extremely risky. Precisely because the new governments were so fiscally strained and could not impose domestic taxes, they also could not risk losing foreign trade from which they garnered such a huge part of their income. The fiscal use of trade thus contradicted any possibility of protectionist economic policy.<sup>18</sup>

<sup>18</sup> This, combined with the already significant ideological bent toward *laissez faire*, devastated what little domestic industry existed and worsened the already considerable tensions between urban centers and the interior. For example, the dependence on the guano receipts (either through loans or through customs) allowed the Peruvian government to follow an open market policy since there was no fiscal requirement for tariffs. This “scorched earth free trade” further devastated what was left of a domestic producer class (Gootenberg 1989, p. 134). In Peru, unlike in Europe, economic liberalism was not used to help the victory of an industrial bourgeoisie over a rural oligarchy but was used by foreign capital to establish its dominance (Yepes del Castillo 1971, p. 38). The low tariffs enforced by the British on Brazil until 1844 had the expected devastating impact on domestic industry. A similar decimation of the colonial artesa-



Simultaneously, the availability of foreign capital also prevented the government from challenging elite groups and forging them into a national alliance. In fact, rather than war leading to greater central control, the absence of such sovereignty may have led to conflict. The War of the Pacific may be the best example of the consequences of the external orientation of these states and the lack of domestic domination. It was "at heart a bald struggle over exports among jealous Chile, Bolivia, and Peru" (Gootenberg 1993, p. 182). "All three countries were hard up, and run by oligarchies which disliked paying taxes and looked to revenue from these fertilizers [nitrate] as a substitute" (Kiernan 1955, p. 14). Each country was competing with the others for those resources that would allow it to continue its "rentier" status and not challenge the domestic status quo (Bonilla 1986, p. 179). War came because the states were too weak to fight their respective elites. For example, because the elites of the altiplano were too powerful to tax, the Bolivian state saw the littoral and the nascent nitrate industry as the best source of fiscal support (Mörner 1985, pp. 140–43). This brought it into conflict with Chile. But, precisely because it did not have adequate support from its home base, Bolivia could not hope to win (Klein 1982, pp. 144–46).

### Timing Is Everything

Latin America's wars also came at the "wrong" historical moment when the relevant states could not use war as an avenue for expansion. The obstacles facing these states were both ideological and organizational.

First, it may be that wars can only make states within an ideological framework of enlightened despotism (Kaiser 1990, p. 206). If so, independent Latin America provided an inauspicious setting. The postindependence period was not ideologically predisposed to state growth. In part this was a response to the expansion of the Spanish colonial state during the 18th century.<sup>19</sup> This met significant resistance; after the 1770s, rebellion against colonial authority became increasingly commonplace. Thus it was particularly difficult for postindependence governments to impose new tax measures as these were associated with the absolutism that had just been defeated. Old taxes were abolished before new ones could be instituted (Burkholder and Johnson 1990, p. 330).

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nal and "manufacturing" class occurred in most other countries. Thus, Latin America delayed developing a national bourgeoisie around which a modern state apparatus could evolve.

<sup>19</sup> Similar to the experience of absolutism in Western Europe, this generated a remarkable increase in resources available to the colonial governments. Mexican revenues, e.g., increased fourfold during the 18th century (Bethell 1987b, p. 10).

The dominance of liberal economic thought throughout the continent also went against the idea of a powerful and intrusive state (Collier 1969; Hale 1968; Burns 1980). The acceptance of classic liberalism set the ideological stage for the challenges to follow (Peloso and Tenenbaum 1996; Love and Jacobsen 1988). Perhaps most important, none of the successful independence rebellions involved radical social reforms and most represented leading economic sectors. Those movements that did call for changes in the distribution of wealth (e.g., Hidalgo and Morelos in Mexico) may have actually contributed to the conservative bias of postindependence government by raising the specter of race war.

Obviously, Latin America was not alone in this attitude toward taxation and government penetration, and certainly the United States had a very different institutional development despite similar ideological constraints. But, Latin America also faced obstacles not found further north. Even if the fiscal spirit had been willing, it would have been difficult for the body to follow. Taxes do not collect themselves, but require a considerable administrative apparatus. Following the distinction made by Michael Mann (1986), we can say that even if the Latin American states were despotic, they were infrastructurally ineffectual. Even if orders flowed from the palace, they were rarely obeyed. The states facing war simply did not have the administrative capacity to respond with increased extraction. There was not enough "there" there to follow the extraction-coercion cycle (Lofstrom 1973). For example, the administrative backwardness of the Ecuadorian bureaucracy was such that double-entry bookkeeping was not successfully imposed even after the 1850s and 1860s (Rodríguez 1985). In 1851 and 1852, the Brazilian government attempted a census. There was considerable opposition to this measure from almost all social sectors who saw it as an effort to establish a list for either new taxes or conscription. Because of this opposition, the idea of a census was abandoned (Barman 1988, p. 236). Despite the obvious benefits of a land tax, the sheer task of a cadastral survey would have also been beyond the capacity of the Brazilian state.<sup>20</sup>

A major reason for this institutional dwarfism was the level of destruction that preceded the appearance of the independent states as discussed above. Accompanying this economic collapse, the independence wars left a legacy of widely dispersed military capacity. The most important impediment to the rise of a more extractive state was that (with some exceptions) no political institution was able to impose its dominance over the society

<sup>20</sup> For an interesting contrast with the Japanese case, see Bird (1977). The key comparative issue here would be that the military revolution in Europe was also accompanied by a bureaucratic revolution that dramatically increased the administrative capacity of the state.

during much of the 19th century. In the case of Mexico, the central government was not even able to enforce its monopoly over the means of violence or institute its *de facto* rule until the Porfirian dictatorship of the last quarter of the 19th century. Attempts to raise revenue internally merely worsened regional violence. The most obvious evidence of this came with the Texas revolt of 1836. Other attempts to raise revenue produced similar regional revolts well in the 1840s. Similar centrifugal forces condemned Argentina to almost permanent civil war until the 1880s. During the War of the Triple Alliance, for example, Argentina faced 85 rebellions, 27 mutinies, and 43 military protests (Pomer 1987, p. 246). The Uruguayan state might be the most extreme example of political fragility. The very concept of a state seems a misnomer for an entity lacking budgets, an accepted currency, or the capacity to maintain order even in its own capital.

Divisions also persisted during much of this period over notions of citizenship, sovereignty, and the relation between state and society. Supporters of opposing visions (generally labeled under the rubrics of liberal/conservative or federalist/centralist) "prolonged the military phase of the independence movements and virtually guaranteed that chaos would be an inescapable legacy of newly formed states" (Knight 1992, p. 18). Much as in 15th-century Italy, the prevalence of factions and their myopic preoccupation with local battles retarded the creation of a political union capable of acting on the international stage and expanding its domestic authority (Morse 1954, p. 79).

Because these divisions played themselves out militarily, armies, for all intents and purposes, *were* the state, and they certainly consumed the largest part of its resources (Halperin-Donghi 1973, p. 74-75). But, unlike the European case, the military did not serve a single master. First, it was willing to be bought by whichever actor promised the best reward for its services. Equally important, because of the low level of technical sophistication, it could not impose a monopoly on the means of violence. Throughout the continent, provinces and local caudillos raised and maintained militias that protected their interests. Militias served to defend property, not governments (Halperin-Donghi 1973, p. 9). The militarization of Latin America during this period represented the worst of all possible worlds: armies fought without being able to dominate, and they coerced without extracting. While draining large amounts of money, the military did not provide a means with which to pay for itself.

Yet, what of the well-known capacity of war to overwhelm social divisions and to provide an ideological center on which more solid foundations can be built (Porter 1994)? Perhaps we can speak of Latin America having the "wrong" kinds of wars? Thus, only interstate wars would serve to build states and Latin America had too many civil conflicts. But this distinction certainly would not hold true for the European or the U.S. cases.

Moreover, there is no discernible difference in the fiscal response to any particular category of war in Latin America.<sup>21</sup> It might be more accurate to say that the "right kind" of war came too soon. The Spanish conquest of the 16th century had already subjugated the most powerful enemy to the criollos, the Indians. Given the important role of the "frontier" in the development of the European state (Bartlett 1993) and the United States, as well as the experience of Argentina, Chile, and Mexico, the presence of an easily defined ethnic enemy *outside the borders* of a state could also have played an important role in Latin American political development.<sup>22</sup>

### The Hollow State

The appearance of the Latin American state was also not accompanied by the rise of a hegemonic class willing and able to ride it to social and political dominance. The wars of independence were the product of the collapse of the legitimacy of the Spanish crown. They did not result from *internal* changes in the colonial societies. The independence wars disrupted the old order, but they did not establish an alternative system of domination. When the colonial apparatus disappeared, no social group had an interest in replacing it with one equally strong. What the criollos wanted was as little interference as possible in their immediate profit making. The availability of international moneys allowed elites an exit, thereby inhibiting the development of a class loyalty to the state.

In an almost complete reversal of the European pattern, the appearance of the modern state strengthened the political power of the land-owning class. It was a *Fronde* in reverse. Those who did possess resources were

<sup>21</sup> I am indebted to discussions with Paul Gootenberg and John Markoff on this point.

<sup>22</sup> On the northern Mexican frontier, e.g., the fight against the Indians did generate a social consensus regarding the need to develop an extensive military force and the subsequent need for the capacity to pay for it (Cerrutti 1983, pp. 29–30). The state of Nuevo Leon established relationships with its merchant and landowning classes that appear much more along the lines of the European model than what we see in the central government. The final Argentinean war against the Indians (also known as the "Conquest of the Desert") in 1879–80 perhaps made a more significant economic contribution than the War of the Triple Alliance as it freed the frontier of the often expensive Indian raids while allowing the vast expansion of agribusiness and the promotion of greater immigration. The war also helped to solidify the legitimacy of the state and made President Julio Roca's political career (Puigbo 1964, pp. 125–27; Gasio and San Román 1977, pp. 115–25; Alemann 1989, p. 101). The conquest of the desert was partly financed by land sales. These further helped establish the power of the landowning elite (Rock 1987, p. 154; Ferns 1973, p. 70). The expansion of territory (combined with developments in the international economy) helped consolidate the oligarchic control, which was to allow the Argentinean state some measure of coherence during the next five decades. (I owe much of this point to conversations with Michael Jiménez, Stephen Aron, and Jeremy Adelman)

completely successful in protecting their wealth. The only exception came, perhaps, in the immediate period of the independence wars. When San Martín finally arrived in Peru in 1821, for example, he gave a great deal of Spanish-held property to those who fought in his army (Lynch 1973, p. 181). It is important to note, however, that the criollo landowning class was largely spared these sacrifices as they were borne by the Spanish *peninsulares* and, in some cases, the Church. When the criollo elite was asked to pay for its independence, it almost always refused, a pattern that was to be repeated for the next 100 years (if not more). Given that the criollos were unwilling to pay even for the elimination of the old masters, we should not be surprised that they would remain unwilling to pay for a new one.

The response to subsequent wars was similar. While the Chilean armies were marching on Lima in 1880, the Peruvian finance minister suggested a small tax on capital so as to pay the troops in the field. These measures were defeated (Ugarte 1926, pp. 165–68). The government also asked for an internal loan of 10 million soles. This request generated 1 million soles, largely from the “popular classes,” as the rich did not want to risk their money (Bonilla 1978, p. 99).<sup>23</sup> During the same war, the Chilean legislature was repeatedly unable to impose a wealth or an income tax (Sater 1986, pp. 131–54). When Mexican Finance Minister Lorenzo Zavala attempted to impose a direct tax in order to finance a defense against the possible Spanish invasion of 1829, he was defeated and his government overthrown by an elite sponsored coup (Tenenbaum 1986, pp. 34–35). Similar efforts during the so-called Pastry War with France produced identical results. Even as the U.S. army marched toward Mexico City in 1847, the government frantically negotiated with the Church and domestic lenders for funds (Tenenbaum 1986, p. 79). In Brazil, the Chamber of Deputies consistently refused to give any funds to Pedro I to fight in Uruguay (Harling 1958, p. 35, n.17).

Taxes and the avoidance thereof made it very clear where the line marking off the dominant from dominated was to be drawn (Gomes 1986, pp. 93–94). In Brazil, the *fazenderio* was systematically avoided as an object of taxation. Discussions regarding land or income taxes had no result; the landowning elite was considered fiscally untouchable (Buescu 1974, p. 142; Leff 1982). Bolivia's Antonio Sucre attempted to impose a direct tax on wealth in the 1820s. Within a year, this tax had been abolished. Resistance was both economically and racially based. Not only did

<sup>23</sup> The British ambassador (quoted in Bonilla 1978, p. 98) noted with surprise that “Peru appears struck with paralysis; the people themselves seem as indifferent to the future as the governing classes, who are thinking more of their personal ambition than the welfare of their country.”

the rich resist the new imposition, but whites resented being placed under a *contribución* on the same level as Indians (Lofstrom 1970, pp. 282–86; Paz 1927, p. 52). Argentinean attempts to expand the tax base faltered due to the successful opposition of powerful social interests already well represented in the legislature (Halperín-Donghi 1982, p. 155). The *contribución directa* was a farce as the legislature would not allow the creation of an independent system of assessment (Friedman 1984, p. 185). In the 1830s, a ranch with 19,000 head of cattle paid a total of 540 depreciated pesos (Burgin 1946, p. 189).

While the avoidance of taxes is perhaps one of the few truly universal traits, the absolute regressivity of the Latin American cases compares unfavorably with the European cases. Even in the 18th century, for example, the British propertied class was paying a fair share of taxes (Stone 1994). In Latin America, what little was paid appears to have come out of the hides of those on the bottom. Caste taxes are perhaps the most obvious example. While there are differences depending on the import basket and the specific rates, the general view is that customs taxes were also extremely regressive.<sup>24</sup>

An important factor here is that the relevant elites did not see the wars as threatening their social positions and thus did not have the incentive to permit greater political penetration. That is, the relevant elite did not appear to care which state ruled them as long as it was not markedly stronger than its predecessor. No state was alien to their immediate interests.<sup>25</sup> A transfer of political allegiance did not imply a change in property. Certainly in most cases, the concern appears to have been with protecting themselves from internal enemies, either ideological, or more commonly, class and racial enemies. The maintenance of such internal control did not require an expansive and expensive state. In this way, as in perhaps many others, the Latin American elites were much closer to their Italian and Polish counterparts than to the English gentry or the Dutch bourgeoisie. In both the latter cases, fear of external threats, be it the Spaniards or "Popery," drove the elites to support high levels of taxation.

Interestingly, losing wars appeared to have created the base not for a more powerful state but at least for a closer union between political goals and the interests of the dominant elite.<sup>26</sup> Following the defeat by Chile,

<sup>24</sup> Further research needs to be done on the composition of imports during the 19th century to determine the class distribution of payment of customs taxes.

<sup>25</sup> I owe this point to Michael Mann.

<sup>26</sup> The consequences of war could be disastrous. From 1870 to 1894, Peru went from having 18 millionaires to none, from 11,587 classified as rich to 1,725, from 22,148 classified as well off to 2,000. Yet, despite this looming disaster, the Peruvian elite seemed more concerned with resistance by peasants than invasion by Chileans (Mallón 1983, chap. 2).

Bolivian elites appear to have been more open to paying for a state that could protect them as well as build the infrastructure needed for the exploitation of natural resources. Mexico's defeat in 1848 and the subsequent Treaty of Guadeloupe did produce a split among the *agiotistas* regarding the need for stronger government. Some members of the elite began to realize the need for a better integrated national economy and the need for a government to nurture it (Tenenbaum 1986, pp. 83–85, 116–17). For the first time, the state was perceived as something other than a massive feeding trough. The Liberals who took power in 1855 did have the support of some of the wealthy who had begun to realize the potential benefits of a stronger state, and they looked to the considerable wealth of the Church for funds. What is the most interesting aspect of the ecclesiastical reforms is that, in its battle with the Church, the government enjoyed the support of a faction of the *agiotistas* who sought a securer basis for their loans. Thus, for the first time, the government had social allies supporting its encroachment on a part of civil society (Tenenbaum 1986, pp. 162–66). In this way, at least, wars did make the foundational first steps toward a state.

### Blood and Debt

What were the effects of the wars of 19th-century Latin America on the fiscal capacity of the state? Instead of a state built on "blood and iron," they constructed a constantly bankrupt beggar made of blood and debt. The easy availability of external financing allowed the state the luxury of not coming into conflict with those social sectors who possessed the required resources. In the 1820s and from the 1870s through the 1890s, loans were relatively easy to obtain. Increasingly throughout the 19th century, almost all the Latin American economies became integrated into a global economy through the export of a mineral or agricultural commodity. In any case, whenever the state did try to extract greater domestic resources, it was universally defeated. The European pattern includes a basic organizational capacity that was missing in Latin America. The wars occurred too soon after independence and were fought by countries not capable of responding in the pattern described by Tilly and others.

Most important, we might wish to recall the distinction between a dominant and a ruling class.<sup>27</sup> Latin America possessed the first but arguably not the second. In general, Latin American countries lacked a single class able to impose its will and organize the capacities of the state toward war. At best, military caudillos, urban merchants, and large landowners made

<sup>27</sup> I owe this point to John Womack.

temporary and unstable alliances. The independence wars failed to produce the kind of hegemony that would have been required for the conjunction of military action and internal extraction. No faction of the dominant class was able to establish a strong enough hegemony so as to prioritize *national* collective interests (even if still defined in class terms). Because of the absence of this dominion, the state apparatus was not truly fiscally sovereign.

*Within* the Latin American pattern there are, of course, relative exceptions, and these serve to prove the rule. The result of the violence of the wars between 1860 and 1880 was clearly a much more powerful Argentine state. The key difference is that unlike in the 1810s and 1820s, the later Argentina did possess a semblance of a central government that could and did use the war both against Paraguay and the Indians to stamp out provincial opposition and impose uniform control over the entire country. More important, by the later half of the century, the central state had found its social ally whose interests it could serve: the export of meat and wheat to European markets required much more political and institutional infrastructure than the sale of salted beef to the slave owners of Brazil. While the rural oligarchy of Argentina remained unwilling to pay for the new state, they were also unwilling to accept challenges to its authority. With this narrow support, Mitre and his successors were able to establish their domination.

The key to Brazil's relative unity would seem to lie in its avoidance of the struggles for independence. Neither the Brazilian economy nor its polity were destroyed by years of civil war, nor did the Brazilian Empire have to maintain an absurdly large military in order to establish its authority. Conflicts during the reign of Dom Pedro I helped resolve the intraelite struggle between the "native" aristocracy and the Portuguese courtiers brought by Pedro's father and thereby consolidated the creation of a Brazilian political class. While there is considerable debate regarding the autonomy of this sector (Graham 1987), there is no doubt about the existence of an "imperial" class that gave Brazil a particular coherence. The secessionist wars in the 1830s and 1840s helped consolidate this group. By the time of the War of the Triple Alliance, Brazil possessed enough institutional coherence to survive, if not necessarily prosper.

While Chile did experience considerable political dislocation during the independence wars, its economy was not crippled by them but may have even grown (Cariola and Sunkel 1991, p. 25). More important, even before the rule of Diego Portales and certainly afterwards, the Chilean elite displayed a remarkable cohesion (Collier 1969). To what extent this was the result of the small size of the country, the concentration in a single city, the pervasiveness of dense interfamilial networks, or just sheer luck is the subject of debate. For our purposes, what is most important is that the



Chilean state did precede war and thus was able to extract some benefits from it. Yet, it is important to note that even the Chilean "exception" still fits the general Latin American pattern discussed above. Even as the state expanded, it did so without extracting from the domestic economy. Overall, the wars helped make Chile, not by a combination of blood and iron, but by allowing a fiscal improvisation fueled by duties on exports of commodities (Bethell 1987a, p. 610).

Paraguay represents perhaps the most interesting exception to the Latin American pattern. Following the requisite period of instability following independence in 1814, the country was ruled by three dictators: José Francia until 1840, followed by Carlos Antonio López and his son Francisco Solano López until the death of the latter in 1870. Francia created an all-encompassing state that dominated every aspect of public life and that was completely controlled by him. The state owned all the land and largely managed all external trade. During the first López, the state was also involved in economic development, building some infrastructure and attempting to achieve self-sufficiency in several industrial goods. López *fil*s encouraged military development to the point that the small country had arguably the strongest army in South America (Williamson 1992, p. 273).

The early Paraguayan state enjoyed a rare degree of autonomy. Unlike the other Latin American cases, there existed an agent within the state that drove it to impose itself on the society (Pastore 1994b, p. 587). Francia served as the structural equivalent of an absolutist monarch. This helped assure the continuance of Paraguayan autonomy (White 1978, pp. 101–2). Francia's centurions allowed him to funnel all social resources toward his political apparatus.<sup>23</sup> If we follow White (1978) that the rise of the military was a direct response to external threat, it would then appear that early Paraguay was perhaps the only example of the classic European variant of war-led state development. Paraguay could maintain this independence in part because the state's needs were covered by the revenue that it could gather. Unlike its neighbors, the Paraguayan state ran a consistent surplus during the entire postindependence period prior to the War

<sup>23</sup> The one consistent demand on the Paraguayan state during Francia's rule was the military budget. Despite the fact that the army never included more than 2,000 men, the military absorbed an average of at least 64% of government expenditures during this period (White 1978, p. 104). Much of the cost was associated with maintaining the military industries that supplied the armed forces. This relative self-sufficiency also helped protect Paraguay's international autonomy. While there are obviously different interpretations of the role of the Paraguayan army (White 1978, p. 107; Pastore 1994b, pp. 591–92; Williams 1979, pp. 60–61), it is clear that it served to protect the state (or perhaps better said, Francia) from both external and internal enemies.

of the Triple Alliance. This reflected the limited demands placed on it but also the monopoly that the state enjoyed over almost all economic activity.

While it is no longer possible to speak of Paraguay as the best example of antidependency and of a successful state-led development (Whigham 1991, pp. 83–84; Pastore 1994a, pp. 321–24), it is nevertheless clear that the Paraguayan state was a very different institutional animal than its continental counterparts. The Paraguayan experience in building a much more powerful state apparatus than its neighbors even in the absence of war prior to the 1860s indicates again that while conflict does provide a stimulus and an opportunity, what matters is the organizational and political base of the state and its sources of support.

### CONCLUSIONS

Wars did not make states in Latin America. The best states could do was to survive wars or gain enough of their neighbors' territory so as to finance expansion. Nowhere did military action generate the kind of societal penetration seen in the European cases. Latin America was caught in an inertial equilibrium: No class was powerful enough to impose its domination, and no state was strong enough to enforce its control. The path to the modern state required one or the other.

The combination of weak central power and external economic direction is the defining characteristic of postcolonial states. The delegitimation of political authority as associated with the colonial power, the fragility of elite coalitions, and the lack of national cohesion or even identity and the orientation toward a metropole and away from the interior and regional neighbors, all have characterized, in one form or another, the experiences of independent countries in Latin America, Africa, and Asia. Many of these have also experienced considerable violence without the benefits of organizational development seen in Western Europe. This pattern should make us wonder about the advisability of using such an idiosyncratic experience as early modern Western European for the construction of universalistic paradigms. At the very least, the experience of Latin America should make us more curious about the particular circumstances that allowed states to flourish following the military revolution of the 16th and 17th centuries.

As discussed above, there are several special conditions that allow wars to make states. First, pressure on the state to respond to the financial challenge of war through increased domestic extraction. There is no reason to expect states to undertake the political and organizational challenge of penetrating their societies if resources can be found more easily. Second, enough of an administrative core must already be in place that the state can use as a base on which to develop its strength. The chaos and violence

of war do not provide the appropriate incubation for underdeveloped polities. Third, no political body can amass enough authority to coerce and extract without social allies. Domestic threats to sovereignty have to be resolved prior to "productive" conflict. Further research might test the relative significance of these three factors through their application to a variety of geographical and historical cases. Certainly these might help explain why it took nearly a millennium of violence for war to produce states in Europe. The case of Poland and the Balkans, suffering from both war and relatively weak states, would also merit attention.

The central lesson to be drawn from the Latin American experience is that we cannot assume political autonomy simply because the symbols of independence are there. States are not actors in and of themselves. They make nothing happen. They are shells—potentially powerful shells—but nevertheless hollow at the core. The machine of the state needs a "driver" that can use the stimulus provided by war to expand its reach and power. This may be state personnel, a dominant class, or even a charismatic individual. But without such a driver, the political and military shell of the state has no direction. Without this direction, wars do not present opportunities for growth but mere challenges to survival. What is required for a fiscal system is not simply constitutional powers but a bureaucratic capacity to enforce these. This will not appear without an alliance between a political institution and a significant social sector. Without such an identification of interests, it is practically impossible for the state to grow, no matter the stimulus of violence.

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# Structuring a Theory of Moral Sentiments: Institutional and Organizational Coevolution in the Early Thrift Industry<sup>1</sup>

Heather A. Haveman  
*Cornell University*

Hayagreeva Rao  
*Emory University*

The authors investigate the coevolution of organizations and institutions—they study how institutional definitions, rules, and expectations unfold in tandem with the organizational structures and processes that embody those institutions. The research site is the early thrift industry. Changes in thrifts' technical environment (the rise of a transient and heterogeneous population) and institutional environment (the rise of Progressivism) propelled this coevolutionary process. The coevolution of thrift organizations and institutions proceeded primarily through selection. Adaptation was constrained by both institutional factors (it was difficult to adopt a novel institutional logic) and technical factors (early thrifts were generally small, so entry and exit were easy).

Over the past two decades, organizational sociologists have produced many studies of the functioning of institutions in organizations—of norms, beliefs, values, rules and taken-for-granted assumptions about the way the world works.<sup>2</sup> DiMaggio and Powell (1991, p. 32) opined that the

<sup>1</sup> We thank the *AJS* reviewers and the participants at the University of Arizona Workshop on Institutional Analysis for their comments. This article has been much improved through comments by Terry Amburgey, Joel Baum, Penny Becker, Frank Dobbin, Shin-Kap Han, Mark Mizruchi, Anand Swaminathan, Mark Suchman, and Jim Wade. We appreciate the data-management help of Ron Harris, and the research assistance of David Fischer, Sasha Lisovsky, and Robert Kadel. Direct correspondence to Heather Haveman, Johnson Graduate School of Management, Cornell University, 525 Malott Hall, Ithaca, New York 14853-4201. E-mail: hah6@cornell.edu

<sup>2</sup> Although institutions may be borne by regimes, cultures, organizations, or routines (Jepperson 1991; Scott 1995), organizations are arguably the most important carriers of institutions because organizations are the most powerful and pervasive elements of modern society (March and Simon 1958; Coleman 1974; Aldrich 1979). Furthermore, over the past century, formal organizations have become institutions themselves

application of "institutional models to the adoption of structural elements . . . has become a growth industry." A smaller body of work (e.g., Coleman 1974; Leblebici and Salancik 1982; DiMaggio 1991; Dobbin 1992) has begun to explain the creation of institutions in organizations.

Despite vibrant traditions of work on the creation and diffusion of institutions in organizations, few studies have captured the full sequence of institutional building, maintenance, and destruction (Jepperson 1991). Studying the coevolution of institutions and organizations (Scott 1995, pp. 146–47)—examining how institutional definitions, rules, and expectations unfold in tandem with organizational structures and processes—is essential, not only because the building up and tearing down of institutions cannot be separated analytically or empirically (Eisenstadt 1968), but also because studying coevolution can shed light on the fragility of institutions and detail how institutionalized organizational forms fall into disfavor (Oliver 1992). Studying the coevolution of institutions and organizations would reveal how the deployment of organizational forms by institutional entrepreneurs destabilizes existing arrangements and shapes new conventions (Clemens 1993).

We fill this gap in the literature by analyzing the historical development of one organizational population—the early thrift industry—whose varying forms embodied a series of social and economic institutions. In the next section, we describe our research site and chronicle the establishment of the first thrifts in America. We depict thrifts as embodiments of particular institutional logics, which we term *theories of moral sentiments*. We demonstrate that a theory of moral sentiments incarnate in one organizational form was replaced, over 60 years, by a series of theories of moral sentiments incarnate in newer organizational forms. We then show to what extent the adaptation of existing organizations or the differential death of old organizations and birth of new organizations drove the coevolution of these institutions and these organizational forms; we then explain why one mode of coevolution dominated. Next, we show how two aspects of modernization—the rise of a transient population and the expansion of the Progressive movement—contributed to the downfall of organizational forms embodying theories that emphasized mutuality and rigidly enforced effort and supported organizational forms embodying theories that emphasized bureaucracy and flexible voluntary effort. We argue that pressures for technical efficiency and institutional appropriateness operated in tandem to shape the content of organizational and institutional coevolution. In conclusion, we summarize our findings, discuss our study's impli-

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(Zucker 1983), the "cultural engines of modern social systems" (Zucker 1988, p. 24). Accordingly, studying institutions most often involves studying organizations.

cations for organizational theory, and propose fruitful avenues for future research.

#### THE EARLY THRIFT INDUSTRY: ORGANIZATIONAL FORMS AND INSTITUTIONS

Thriffs first appeared in the United States in 1831. Early thriffs were mutual self-help organizations whose central goal was to organize individuals to improve their material standing, specifically to make it easier for organizational members to save money and use the accumulated savings to build or buy houses (Clark and Chase 1925, pp. 3–9; Bodfish 1931, pp. 1–18; Kendall 1965).<sup>3</sup> One prominent thrift proponent eloquently described these nascent organizations as “an organized effort to extract concrete wealth from aggregated poverty” (Durack, cited in Clark and Chase 1925, p. 4).

Thriffs in the United States were directly influenced by the building-society movement in England, which exploded between 1780 and 1850. Like England after the Industrial Revolution, America during the early 19th century experienced a sharp increase in the demand for housing due to a fourfold rise in population during the Revolutionary War, an explosion of factories, and the rapid growth of cities. Many settlers from Europe who sought to convert their savings into home financing were familiar with British building societies. For example, the first U.S. thrift—the Oxford Provident Building Association, founded near Philadelphia in 1831—was conceived by two factory owners and a doctor from England (Bodfish 1931).

When U.S. thriffs were first founded, and for many years afterward, few other financial intermediaries were willing to make loans to individuals for home construction. Commercial banks were generally uninterested in consumers and home mortgages:

[Thrift] associations have been partly the outgrowth of an incomplete banking system. There was a crying need for a convenient method of financing home building, at the same time a demand for a flexible method of investment for the man of small means. The banker believed he furnished the latter through a 4 per cent savings account, but the rate was so low it invited investment in too many “wild-cat” stock selling schemes, and of necessity there should be some better plan.

<sup>3</sup> Thriffs were known under many different names. In their guide to thrift history and operations, Clark and Chase (1925, pp. 13–14, 518–20) listed 143 variations, with these being among the most common: building and loan association, savings and loan association, cooperative bank, and homestead association. Many labels emphasize links between thriffs’ two core functions: to encourage saving by members and to help members purchase or build houses. We refer to all such organizations as thriffs.

Banks have never looked with favor upon construction loans, and as a general thing have declined them, unless the borrower was in a position to give security through the deposit of bonds, or some other form of collateral. (California 1926, p. 309)

The growing demand for home financing represented an opportunity for institutional entrepreneurs. Commercial banks were not prepared to manage construction loans and therefore were neither capable nor willing to assist prospective home owners; their knowledge base and competitive advantage was in short-term credit (Clark and Chase 1925, pp. 19–21).

Table 1 compares thrifts' investments and investors (uses and sources of funds) with those of other financial institutions.<sup>4</sup> Thrifts' domain—the territory thrifts staked out in terms of products offered and clientele served—was closest to that of savings banks. Savings banks, like thrifts, invested in home mortgages. But this was generally a small fraction of savings banks' investments; government and industrial bonds were far more important. Trust companies typically specialized in handling trusts and other transactions outside the normal realm of banking. Mortgage companies made mortgage loans, primarily on industrial or commercial properties. Because they were agents for large investors, mortgage companies seldom invested in small home mortgage loans to wage or salary earners. Other financial institutions, such as insurance companies, did not overlap with thrifts materially.

Thrifts grew rapidly. In 1888, when the first census of thrifts was conducted, they numbered more than 3,000 and held over \$300 million on deposit. By 1893, 5,838 thrifts operated throughout the United States (U.S. Commissioner of Labor 1894); by 1924, 10,744 thrifts were in operation (Clark and Chase 1925, pp. 514–17). Membership rose even faster than the number of associations, from 1.75 million in 1893 to 7.20 million in 1924. As thrifts grew, their importance in the U.S. financial system also increased. By 1929, thrifts controlled \$7.4 billion in assets (Goldsmith 1958, pp. 73–75).

### Thrift Organizational Forms

The various methods of administration used by early thrifts were termed *plans*. Thrift plans were contracts between organizations and their members concerning the roles and responsibilities of members and the procedures for regulating how incoming funds were invested and earnings were distributed (Clark and Chase 1925, pp. 32–33). Thrift plans, described in

<sup>4</sup> This is based on a table and supporting narrative found in an influential thrift textbook (Clark and Chase 1925, pp. 19–32). We also draw on discussions in Dexter (1889) and Bodfish (1931).

TABLE 1  
COMPARING VARIOUS FINANCIAL INSTITUTIONS, CIRCA 1920

Financial Institution	Type of Investment	Principal Class of Investor or Depositor
Thrifts .....	Home mortgages; federal and state government bonds; other bonds	Small savers
Savings banks .....	Federal and state government bonds; industrial bonds; foreign government bonds; mortgages on business properties and homes	Small savers; conservative investors
Trust companies .....	Federal and state government bonds; industrial bonds; foreign government bonds; mortgages on business properties and homes	Conservative investors
Mortgage companies .....	Mortgages on all types of real estate	Large investors
Investment companies, insurance companies .....	Industrial bonds; federal and state government bonds; foreign government bonds; industrial, railroad, and utility stocks; mortgages on farms, business properties, and apartments	Large investors; speculators
Stockbrokers .....	Industrial stocks; industrial bonds; federal and state government bonds; foreign government bonds	Speculators; large investors
Commercial banks .....	Commercial paper of all kinds (short-term credit only)	Individuals and corporations using checking accounts

SOURCE.—Adapted from Clark and Chase (1925), p. 23.

NOTE.—Investments are listed in approximate order of importance for each institution.

thrifts' articles of incorporation, shaped the institutions' goals, authority structures, financial-intermediation technologies, and product offerings; thrift plans thus determined the core features of organizational form (Hannan and Freeman 1989). In the following sections, we use the terms "plan" and "organizational form" interchangeably.

Early thrifts were a form of collectivized agency arrangement (Shapiro 1987). Accordingly, transactions that could not be embedded in personal relationships between thrift members were embedded instead in impersonal trust relations between thrift members and organizers. Collectivized agency relations permeate modern societies: corporate shareholders elect boards of directors to oversee managers, parents entrust children to day-care centers, children entrust parents to nursing homes, individual investors entrust assets to mutual-fund managers, workers join unions and elect officials to represent their rights and better their job opportunities, and firms in particular industries or regions join associations to promote their mutual interests. In all of these, strangers come together to repose trust and confidence in agents. Thus, the essential feature of such institutions is that diffuse and impersonal cooperation among numerous principals is coordinated by agents.

Like other collectivized agencies, early thrifts were formed when depositors, often strangers to each other, banded together and entrusted funds to agents—in this case, thrift organizers. Like all collectivized agencies, a central feature of early thrifts was the coupling of direct cooperation between principals and agents with diffuse cooperation among principals. Like all collectivized agencies, thrifts evolved arrangements to reduce uncertainty for principals and induce them to repose faith in each other and in the agents responsible for operating these enterprises (Shapiro 1987; Bradach and Eccles 1989; Stinchcombe 1990).

### Thrift Institutions

Early thrift plans—their structures and processes—were incarnations of beliefs and values concerning saving and home ownership. In other words, thrift plans were “theories in use” (Argyris and Schon 1974), notions of *what* thrift is (not) and *what* thrift should (not) do. For example, the original thrift plan embodied an expectation that people should save to buy or build homes and not for other, less consequential purchases. Thrift plans also embodied theories of financial coordination that had clear normative overtones; that is, expectations about *how* thrift should (not) be done. For instance, the original thrift plan’s rules and procedures celebrated mutual cooperation and rigidly structured saving; this plan was predicated on opposition to flexible saving and lending through for-profit organizations like banks. Because early thrift plans embodied such normative expectations, they took on value far beyond the technical requirements of the financial-intermediation task at hand (Selznick 1957, p. 17).

Each thrift plan, infused with meaning and value, was an incarnation of what Adam Smith (1976) termed a “theory of moral sentiments” to guide the conduct of “prudent men”; a system of logic and ethics that combined

Stoic prudence and self-command with Christian benevolence.<sup>5</sup> Contemporary descriptions of early thrifts and their social function illustrate how these theories of saving and lending resonated with moral sentiments:

A man who has earned, saved, and paid for a home will be a better man, a better artisan or clerk, a better husband and father, and a better citizen of the republic. (Dexter 1889, p. 11)

The impetus of association in a common purpose, the desire to maintain his standing among his associates, the pride and satisfaction of acquiring a home, will nerve [a thrift member] to his best efforts; will make him more frugal, more industrious, more painstaking in his daily life, and cause him to get ahead in the world, when under different circumstances his earnings might have been entirely frittered away. In this manner the Building and Loan system helps to make good citizens. (California 1896, pp. 4–5)

Thrift is a disciplinarian. It breeds virility. It strikes at sensuality, self-indulgence, flabbiness. It teaches the heroism of self-denial, temperance, abstemiousness, and simple living. It is the way to success and independence. It makes for happy homes, contented communities, a prosperous nation. (*American Building Association News*, quoted in Clark and Chase 1925, p. 5)

The building and loan association is more than a business concern; it is a great social institution conducive to the cultivation of those habits of thought and life which make for national progress and prosperity and for the advancement of civilization in general. . . . The greatest achievement[s] of this movement . . . are those large personal and social values which I designate as the spiritual accomplishments of this great movement, namely: . . . its development of the spirit of self-respect and confidence in one's own personality; . . . the immeasurable personal satisfaction which comes from the sense of home ownership; the contribution it makes to the stabilization of population and the consequent stabilization of those other great institutions—industry, the home, the state, the school, and the church. . . . In encouraging thrift and home ownership, the building and loan movement is making more solid the foundations of the greatest civilization the world has yet known. Yours is not merely a business function; it is a mission which seeks to achieve social justice in an evolutionary manner. Yours is not just a job, but a profession of the highest order. (Address by Professor Gordon Watkins to the California Building-Loan League, quoted in California 1926, p. 314)

As these quotations make clear, thrift organizers considered their charge to be one in which economy *and* morality inhered: moral worth was improved by economic advancement.

<sup>5</sup> We follow Smith in using "sentiments" to indicate opinions, beliefs, and judgments rather than emotions. We do not suggest that the first thrifts were designed according to or motivated by Smith's particular theory of moral sentiments. We merely wish to make clear the parallels between Smith's theory of moral sentiments and the theory inherent in the earliest thrift plans.



## The Duality of Organizational Form and Institution

Early thrifts' forms of organization (membership rules, governance structures, financial-intermediation technologies, and products) could not be decoupled from their institutional logics (the norms, ways of thinking, and rules that constituted their theories of moral sentiments). Changes in institution had to be accompanied by changes in organizational form, and changes in organizational form inevitably yielded changes in institution. Hence, the building up and tearing down of thrift institutions operated in tandem with the emergence and extinction of thrift organizational forms. For this reason, studying the evolution of institutions requires studying the evolution of organizational forms and vice versa (Scott 1995).

The duality of organizational form and institution yields a recursive relationship. On one hand, because organizations render institutions material and thus potent to shape human behavior, the fates of organizational forms determine the fates of institutions; that is, the persistence and evolution of organizational forms makes possible the concurrent persistence and evolution of institutions. On the other hand, the legitimacy of organizational forms' goals and structures partly determines their fate, and this legitimacy requires congruence between the institution embodied by a particular organizational form and the normative, cognitive, and regulatory character of wider society.

Institutions operate at multiple levels of jurisdiction in society (Scott 1995; Jepperson 1991). Meyer and Rowan (1977, p. 343) pointed out that "norms of rationality are not simply general values. They exist in much more specific and powerful ways in the rules, understandings and meanings attached to institutionalized social structures." Following this logic, we propose that organizational form is tied to two distinct types of institutions that function at different levels of society. First, organizational forms make incarnate *specific institutions*—particular regulations, norms, and ideas that structure the actions of individuals and groups. In the case of the early thrift industry, the relevant specific institutions are particular theories of moral sentiments concerning how one should save and borrow; that is, beliefs about how thrift should (not) be done. In other circumstances, organizational theorists have been concerned with how corporations manifest norms and values concerning hostile takeovers through jargon and official communication (Hirsch 1986) or how employing organizations make concrete regulations and social expectations concerning equality of opportunity, due process, and employee rights (Edelman 1990; Dobbin, Sutton, Meyer, and Scott 1993). Second, organizational forms are legitimated to the extent that they accord with *general institutions*—broadly accepted norms, values, and belief systems that constitute

the master principles of society, such as truth, equality, and justice. In the early thrift industry, the relevant master principles are notions of citizenship, progress, and trust. In other circumstances, scholars have analyzed generally accepted ideas about rational economic behavior and legally sanctioned organizational forms (Delaney 1992; Creighton 1996).

The essence of institutional entrepreneurship is to align skillfully an organizational form and the specific institution it embodies with the master rules of society (Swidler 1986) so as to ensure that the organization thrives. There is often conflict about which master rule should determine the design of organizations and institutions (Friedland and Alford 1991). The design with the most support will be privileged. Proponents of losing designs, those embodying precarious beliefs and norms, can take several courses of action: they can exit the arena, focus on small specialized niches, or embrace the accepted design and transform their own organizations. Organization builders therefore play a crucial role in resolving the issue of which master logic is applied to the design of organizations and their institutional content. The effects of general institutions are thus filtered through interactions between the designers of organizational forms and the specific institutions these forms render incarnate. The indirect relationship between general institution and organizational form is necessarily weaker than the direct relationship between organizational form and specific institution.

The fate of an organizational form (i.e., the fate of the many organizations exhibiting that form) largely determines the fate of the specific institution it embodies in two important respects.<sup>6</sup> First, specific institutions affect behavior through their organizational embodiments. Hence the influence of institutions embodied by organizations expands when their organizational manifestations proliferate; that is, when new organizations are created or when established organizations adopt structural devices that embody the norms, values, and beliefs in question. For example, industrial firms developed personnel departments and formal procedures predicated on ideas about bureaucratic control of employees' careers (Baron, Dobbin, and Jennings 1986). By the same token, institutions decline when their organizational infrastructures are dismantled. For example, the firm-as-portfolio logic embodied in the conglomerate form decayed as corporations sold off unrelated businesses and shunned diversification (Davis, Diekmann, and Tinsley 1994). Second, specific institutions acquire social facticity—a taken-for-granted character—and

<sup>6</sup> Note that this logic holds most strongly when a single organizational form embodies the specific institution under study. If, however, a specific institution resides in multiple organizational forms, that institution's fate is not entirely tied to the fate of any single form. We thank an *AJS* reviewer for pointing out this caveat.

amass sociopolitical support when many organizational instances exist (Hannan and Freeman 1989; Hannan and Carroll 1992).

Below, we explain the rise and fall of theories of thrift (specific institutions) by analyzing the fates of their organizational incarnations. We then show how the fates of the organizational incarnations of these theories of thrift were shaped by a debate about which master logic of wider society (i.e., which general institution) should be applied to organize thrift.

### Starting and Ending Points for Organizational and Institutional Coevolution

We study thrifts in one state—California. We limit the geographic scope of our analysis to make the task of gathering and analyzing data manageable: assessing the features and success of various organizational forms that embodied various theories of moral sentiments requires detailed data on individual thrifts. California is a large state and so offers a large number of thrifts for analysis. Moreover, state agencies published serially reliable organization-level records extending from 1890 (California, various years). Although we have detailed data on California thrifts only, our descriptions of thrift plans and many of the quotations we present from contemporary observers relate to the United States in general.

The period covered by our detailed empirical analysis does not include the earliest history of thrifts in California. The first California thrifts were founded some 25 years before our records begin. We would obviously prefer to analyze the complete history of thrifts in California, but no data on individual thrifts and their organizational forms are available before 1890. Since we are concerned with tracing the coevolution of organizational forms and institutions in this industry, and since we expect that current institutional and technical conditions determine the success of various forms, our data are adequate to our needs.

At the start of our observation period, in 1865, all California thrifts were based on the terminating plan; at the end, in 1928 (the year before the Great Depression), California thrifts were predominantly organized on the Dayton/guarantee-stock plan. Concomitantly, at the start of this period, a theory of moral sentiments that emphasized mutuality and enforced saving dominated thinking about thrift; at the end, a theory that emphasized bureaucratic control and celebrated voluntary saving held sway. We describe these two plans and their theories of moral sentiments in turn and then analyze their differences.<sup>7</sup>

<sup>7</sup> Our descriptions of thrift plans, presented here and below, are compiled from reports by a number of contemporary observers and later industry analysts (Wrigley 1873; Dexter 1889; Winters 1890; California 1890–1929; Myers 1921; Clark and Chase 1925; Bodfish 1931; Kendall 1965; Teck 1968; Rasmussen 1988).

*The first form, the terminating plan.*—The terminating plan was, as the name implies, a self-liquidating organizational form. It was pioneered in 1831 by the founders of the first U.S. thrift, the Oxford Provident Building Association. People—primarily wage earners such as clerks and artisans, but also professionals with moderate incomes—met at regular intervals to save money, borrowed from the growing communal fund to build or buy houses, and then dissolved the association when their joint task was completed (Dexter 1889, pp. 9–15; Bodfish 1931, pp. 2–3). Members of terminating-plan thrifts were shareholders, not depositors, because their contributions represented equity rather than debt. Shareholders' equity stakes were to be invested over the life of the association. Members wishing to withdraw early had to give one month's notice and pay a penalty.

In return for their dues, members owned shares that had a certain par value. Members subscribed to enough shares so that the par value equaled their proposed loan amount. Members borrowed in turn, paying interest on their loans and continuing to pay installments on their shares. Precedence in borrowing was established by competitive bidding; the winning bid included a loan premium. Earnings consisted of premiums, interest paid by borrowers, and fines charged for delinquency; these were divided among members on the basis of their investments. When the contributions for each share accumulated to the par value, the association was terminated and its assets divided among members in proportion to the number of shares they owned. Members used their matured shares to cancel their loans.

As a theory of moral sentiments, the terminating plan emphasized mutuality. These thrifts resembled clubs: small groups of individuals cooperated to achieve the common goal of home ownership. Indeed, the inhabitants of the city where the first U.S. thrifts were founded referred to them as "building clubs" (Clark and Chase 1925, p. 459). In this nascent institution, profit existed neither as a goal nor as an accounting entity (Bodfish 1931). All members of terminating-plan thrifts had the same dual role: saver and borrower. In addition, management of the organization's quotidian tasks was performed by volunteers; there was no formal management role other than that of trustees elected from among thrift members.

As a theory of moral sentiments, the terminating plan also emphasized enforced saving and structured individual effort. Structural constraints were strong: members joined the association together at its inception; they paid regular weekly or monthly contributions to the common fund; delinquency in payment was punished by fines and forfeiture of membership.

Terminating-plan thrifts resemble rotating savings and credit associations (ROSCAs), which are found in many societies around the world. ROSCAs are typically formed by people who know each other well and who agree to make regular contributions to a communal fund that is lent

to each contributor in sequence. Like ROSCAs, the first thrifts depended on social relations among participants to facilitate economic relations, "to recruit, provide information on creditworthiness, instill obligation, discipline, and monitor participation" (Biggart, Castanias, and Davis 1996, p. 12). So, like ROSCAs, the first thrifts were communitarian organizations that relied on social relations to create and sustain a market for credit.

It is not surprising that institutional entrepreneurs established the first U.S. thrifts as highly personal nonprofit associations ("building clubs") rather than as impersonal for-profit corporations. In 19th-century America, mutual self-help was highly valued and organized in several domains of social and economic life, so this general approach to organized problem solving was available to be applied to thrift. Contemporary observers likened thrifts to mutual savings banks, mutual fire-insurance organizations, producers' and consumers' cooperatives, volunteer fire departments, community subscription libraries, and community hospitals (Bodfish 1931, pp. 4–18). Moreover, the founders of the first U.S. thrifts had specific examples to copy: English building societies. Indeed, some observers labeled thrifts "friendly societies," just like their English fore-runners.

*The last form, the Dayton/guarantee-stock plan.*—In dramatic contrast to the terminating plan, the Dayton/guarantee-stock plan emphasized neither mutual cooperation nor enforced saving; instead, it celebrated bureaucracy and voluntary saving. This plan deemphasized the mutuality of thrift members in several ways, all stemming from divisions between members' roles. First, this plan created sharp distinctions between manager and member, and between depositor and borrower: associations using this plan employed a cadre of paid managers and did not require all savers to be borrowers. Second, this plan offered individual deposit and loan accounts that made possible individual entry and exit, thereby minimizing members' temporal interdependence and shared fate. Third, this plan distinguished between ordinary installment stock and guarantee (or contingent-reserve) stock, which was not withdrawable and which was used to insure earnings on ordinary installment stock. Because two risk classes existed (guarantee stockholders took higher risks than regular installment stockholders), two return classes existed (excess income, if any, accrued to guarantee stockholders, not to installment stockholders).

The Dayton/guarantee-stock plan represented a radically different theory of individual effort than the terminating plan. Specifically, this plan was a concrete expression of the idea that prudence and virtue did not require forced, clocklike regularity, but could be achieved through voluntary and occasional saving. This plan mandated no regular payment of prescribed amounts; instead, payment could be made at any time and in any amount.

*Contrasts in function.*—Our descriptions show striking differences between the terminating and Dayton/guarantee-stock plans. (1) In terminating-plan thrifts, exchange occurred between individuals who played the same dual role: borrower and saver. In Dayton/guarantee-stock thrifts, exchange occurred between individuals who played dramatically different roles: borrower, saver, or guarantee stockholder. (2) Terminating-plan thrifts were managed by ordinary members elected by their fellows, while Dayton/guarantee-stock thrifts employed paid managers. (3) The Dayton/guarantee-stock plan created a distinction between owners and depositors (i.e., between holders of guarantee and installment stock), and so created the possibility of conflict between different classes of stakeholders (Teck 1968); no such distinction existed in the terminating plan. (4) The terminating plan imposed a rigid structure on individual members, while the Dayton/guarantee-stock plan allowed flexible schedules. The shift in dominant organizational form from the terminating to the Dayton/guarantee-stock plan was therefore attended by a shift in the theory of moral sentiments, specifically an erosion of the central elements of mutual cooperation and rigidly structured individual effort.

Note that the strong moral nature of thrift remained long after the original pillars of mutuality and enforced saving had eroded. Although the terminating plan's ideological trappings of mutuality and structured individual effort were weakened over time, they were replaced by the Dayton/guarantee-stock plan's novel, but still ideological, trappings of bureaucracy and voluntary effort. Quotations from commentators in the 1920s concerning the moral nature of thrift (see above) make this clear. Hence the *content* of the theories of moral sentiments made material by thrift plans shifted over time, but even the Dayton/guarantee-stock plan was strongly invested with ethical character.

### The Path of Thrift Coevolution

The terminating plan was not abruptly extinguished by the Dayton/guarantee-stock plan. Instead, many intermediate organizational forms and institutions emerged and were extinguished. Hence, the coevolution of this organizational form and institution was a gradual process. Contemporary observers (e.g., Dexter 1889; Clark and Chase 1925; Bodfish 1931) noted that four basic plans (serial, permanent, Dayton, and guarantee stock) and four hybrids (serial/Dayton, serial/guarantee stock, permanent/guarantee stock, and serial/Dayton/guarantee stock) emerged before the dominance of the Dayton/guarantee-stock plan. Table 2 summarizes the attributes of the basic plans in order of their appearance.

The *serial plan* was identical to the terminating plan, with two differences: cohorts of members were liquidated instead of the entire associa-

TABLE 2  
BASIC PLANS IN THE EARLY THRIFT INDUSTRY

Plan	Features
Terminating	<p>All members were both savers and borrowers.</p> <p>Members made periodic payments to a common fund (dues); fines were charged for late dues payments.</p> <p>Members subscribed to shares with a matured value equal to the value of the loan they wanted.</p> <p>Precedence in borrowing was established by bidding.</p> <p>When all shares reached their matured value, the association dissolved and assets were divided among members in proportion to shares owned.</p>
Serial	<p>Was a self-perpetuating association comprising a series of terminating associations.</p> <p>New series of shares were issued at periodic intervals, rather than a single group of shares at founding. Hence, cohorts of members entered and left the association on set schedules.</p> <p>The association continued to operate after each series of stock matured. A cadre of professional managers offered continuous administration of the association.</p> <p>Retained other attributes of the terminating plan: regular saving was enforced by fines, there was bidding for precedence in borrowing, saving and borrowing were coupled.</p>
Permanent	<p>Each member had an independent account; his or her shares started and matured without regard to other members' shares. Hence, individual members entered and left the association on their own schedule.</p> <p>Installment dues were uniform and at regular intervals; penalties were enforced for late payment.</p> <p>Members bid for loans; winning bids paid premiums.</p> <p>Savers did not have to borrow, so borrowing was decoupled from saving.</p>
Dayton	<p>Each member had an independent account that could be closed at any time.</p> <p>There were two types of accounts: installment and paid-up stock, which had the face value of the share paid in at the start and subsequently accrued interest.</p> <p>Payment on accounts could be made at any time; there were no fees for late payment. Hence, this plan introduced a new theory of individual effort: prudence and virtue did not require clocklike regularity, but could be achieved through occasional saving.</p> <p>There were no premiums for loans, loans were made in order of application; interest rates varied with demand.</p> <p>Shareholders could just save and were not forced to borrow.</p>

TABLE 2 (Continued)

Plan	Features
Guarantee stock	<p>Guarantee (capital) stock was paid in at time of founding by officers, it was not withdrawable and was used to insure earnings on ordinary installment shares. As a consequence there were two risk and two return classes, guarantee stock and installment stock. Guarantee stockholders took higher risks than installment stockholders; earnings in excess of contract liabilities accrued to guarantee stockholders, not to installment stockholders.</p> <p>Installment stock accounts could be withdrawn at any time. Individual accounts made individual entry and exit possible.</p> <p>Installment stockholders could just save, not borrow.</p> <p>Loans were made in the order of application; interest rates were adjusted periodically.</p>

SOURCES —Wrigley (1873); Dexter (1889), pp. 70-112; Clark and Chase (1925), pp. 32-78; Bodfish (1931), pp. 32-231, 317-27.

tion, and a cadre of paid managers ran the association. In turn, the *permanent plan* differed from the serial plan in one key respect: instead of cohorts being started and liquidated, individual members' accounts were freely opened and closed. The *Dayton plan* represented a remarkable break from previous plans in that it created distinctions between savers and borrowers and discontinued the system of fines and forfeitures for delinquency.<sup>8</sup> The *guarantee-stock plan* differed from other plans by distinguishing between guarantee stock (nonwithdrawable stock paid in at founding that was used, as its name suggests, to guarantee the association's loans) and ordinary installment stock. This plan offered its two types of stockholder different risks and returns: guarantee stockholders took higher risks and received greater returns than installment stockholders.

*Hybrid plans*, which are not shown in table 2, were recombinations of various basic plans. Some of these variations arose through planned change to meet idiosyncratic local needs; others were simply due to unplanned errors in replication (Clark and Chase 1925, p. 33). The *Dayton/guarantee-stock hybrid*, described earlier, took from the Dayton plan the ideas of voluntary saving and of savers not having to borrow and, from the guarantee-stock plan, the idea of a contingent-reserve fund. To give a second example, the *permanent/guarantee-stock hybrid* incorporated most

<sup>8</sup> This plan originated in Dayton, Ohio; hence, the name.



elements of the permanent plan: it allowed individual accounts, it enforced saving by penalizing late dues payments, and it had members bid for loans. It also incorporated elements of the basic guarantee-stock plan: the nonwithdrawable capital stock paid in by the founding officers at start-up was used to insure ordinary installment stock.

In comparing the theories of moral sentiments embodied in these organizational forms, we focus on how the plans differed in their emphasis on mutuality and structured individual effort. With respect to mutuality, we ask (a) did all members have a dual saver/borrower role, (b) was this role temporally identical for all members, (c) was there a paid managerial staff, and (d) did all organizational members accept the same risks and returns? With respect to structured individual effort, we ask (a) could members pay on flexible or irregular schedules, (b) could members pay varying amounts, and (c) could members save without having to borrow? Table 3 summarizes our analysis.

Table 3 shows that the terminating plan was the material realization of a theory of thrift based on the pillars of mutual cooperation and external control. This plan allowed for no role differentiation between members—all members were both savers and borrowers; all members entered the organization at its founding and left at its dissolution; there was no professional managerial cadre, as ordinary members assumed responsibility for administrative tasks; and all members took the same risks and received the same rewards. Note that this plan carried the notion of shared fate to its logical conclusion—associations were liquidated when all members had built homes and repaid loans. Finally, this plan was based on the idea that individuals would save only when forced to do so; hence, a system of punishments and fines was key.

Other thrift plans weakened the pillar of mutuality, the pillar of enforced saving, or both. The serial plan preserved enforced saving but attenuated collaboration—it weakened common bonds among members by making the cohort of members the entity that was liquidated (rather than the entire organization) and by introducing paid managers (rather than unpaid volunteers). The permanent plan was an incremental extension of the serial plan and further weakened collaboration because individuals were the entities that entered and exited the association rather than cohorts of individuals. The Dayton plan undermined enforced saving: saving was voluntary, a real choice rather than the inevitable consequence of fixed constraints. Finally, the guarantee-stock plan shattered the original model of collaboration by distinguishing between the owners of two different classes of stock.

Numerous hybrid plans sought to blend different theories of thrift. For instance, the serial/guarantee-stock hybrid preserved the model of enforced saving with a weakened notion of mutuality. In this plan, mutuality

TABLE 3  
COMPARING THE THEORIES OF MORAL SENTIMENTS IN THE BASIC THIEFT PLANS

Plan	Mutuality	Structured Individual Effort
Terminating .....	All members played a dual saver/borrower role; all members shared the same temporal position—all entered and left the association at the same time; there was no managerial cadre; all savers were in the same risk and return categories. <i>Conclusion:</i> Mutuality was extremely strong.	Members were forced to pay on set schedules; members were forced to pay set amounts; all members had to save and borrow. <i>Conclusion:</i> Structured individual effort was very strong.
Serial .....	All members played a dual saver/borrower role; not all members shared the same temporal position—members entered and left the association with their series cohort; there was a managerial cadre, all savers were in the same risk and return categories. <i>Conclusion:</i> Mutuality was strong (but weaker than in the terminating plan).	Members were forced to pay on set schedules; members were forced to pay set amounts; all members had to save and borrow. <i>Conclusion:</i> Structured individual effort was very strong.

Permanent .....	<p>All members played a dual saver/borrower role; members did not share the same temporal position—they entered and left the association on their own schedule; there was a managerial cadre; all savers were in the same risk and return categories.</p> <p><i>Conclusion:</i> Mutuality was strong (but weaker than in the serial plan).</p>	<p>Members were forced to pay on set schedules; members were forced to pay set amounts; not all members had to borrow.</p> <p><i>Conclusion:</i> Structured Individual effort was strong.</p>
Dayton .....	<p>Some members were only savers and others were both savers and borrowers; members did not share the same temporal position—they entered and left the association on their own schedule; there was a managerial cadre; all savers were in the same risk and return categories.</p> <p><i>Conclusion:</i> Mutuality was weak (but not nonexistent).</p>	<p>Members neither had to pay on set schedules or in set amounts—they could pay any amount, any time; not all members had to borrow.</p> <p><i>Conclusion:</i> Structured Individual effort was virtually nonexistent.</p>
Guarantee stock....	<p>Some members were only savers, others were both savers and borrowers; members did not share the same temporal position—they entered and left the association on their own schedule; there was a managerial cadre; all savers were not in the same risk and return categories since guarantee stockholders took higher risks than installment stockholders; excess profits, if any, accrued to guarantee stockholders, not to installment stockholders.</p> <p><i>Conclusion:</i> Mutuality was very weak.</p>	<p>Members were forced to pay on set time schedules; members were forced to pay set amounts; not all members had to borrow.</p> <p><i>Conclusion:</i> Structured Individual effort was strong.</p>

was weakened because paid managers existed and cohorts of members were dissolved (both elements of the serial plan), and because excess risks and profits accrued to guarantee stockholders (the central element of the guarantee-stock plan).

Figure 1 plots the number (density) of California thrifts operating under each plan between 1890 and 1928; figure 2 plots the aggregate size (mass) of each plan.<sup>9</sup> The number of thrifts more than doubled during the 40 years for which we have detailed data, rising from 111 in 1890 to 225 in 1928. Although the first California thrifts, established in 1865, were organized on the terminating plan (Bodfish 1931), few terminating-plan associations operated in California in 1890; this form disappeared at the turn of the century. This is consistent with trends in other parts of the country (Bodfish 1931; Clark and Chase 1925, p. 60). In 1890, when our detailed data begin, the serial plan dominated the California thrift industry with 101 out of 111 thrifts operating (91% of existing thrifts). The serial plan continued to be the modal plan until near the end of the observation period. The Dayton plan had modest success, growing in numbers after 1901 and reaching a peak of 26 (23% of thrifts operating) in 1909. The permanent plan also had modest success, growing from four in 1890 to a peak of 10 between 1900 and 1902, before disappearing after 1916. In contrast, the guarantee-stock plan never grew large in numbers. This plan was followed by the serial/guarantee-stock hybrid, which achieved some success after 1920. Finally, the Dayton/guarantee-stock hybrid grew slowly from 1901 to 1920, then rapidly became the dominant plan. By the end of our observation period, this plan had reached a peak of 157 members, which constituted 69% of thrifts operating at that time. California moved relatively early to embrace this plan. The serial plan was still the dominant form across the United States as late as 1923 (Clark and Chase 1925, p. 60). Not until the early 1930s did the Dayton/guarantee-stock plan dominate nationwide.

Figure 2 shows that thrift mass (total assets) increased even more dramatically than density, from \$15,800 in 1890 to \$376,400 in 1928. At the start of our observation period, the average California thrift held \$145 in investments, while at the end, the average thrift held \$1,680. The serial plan's dominance in the California thrift industry was less than its sheer numbers would suggest. Serial-plan thrifts were limited in size by their unwieldy bookkeeping systems and so had relatively small asset bases:

<sup>9</sup> These plots are based on data from *Annual Reports* of the California State Commissioner of Building and Loan 1890–1928. To make these plots legible, we aggregated five plans having few adherents into the category "other": terminating, guarantee stock, serial/Dayton, permanent/guarantee stock, and serial/Dayton/guarantee stock.



FIG. 1.—Density of early California thrift plans, 1890–1928

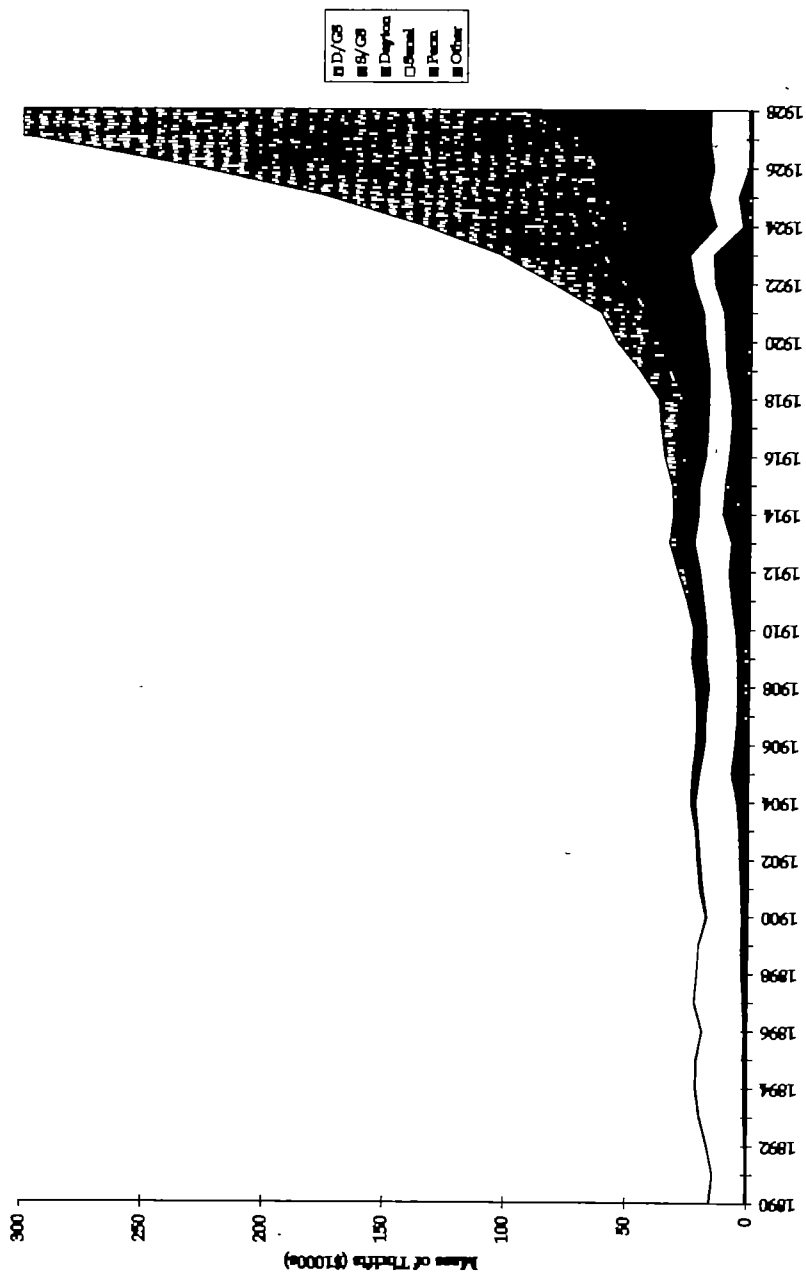


FIG. 2.—Mass of early California thrifts, 1890–1928

\$527 on average in 1928. In contrast, the average Dayton/guarantee-stock thrift had assets of \$1,854 in 1928.

Figures 1 and 2 give rise to two questions. *By what process* did coevolution operate: adaptation, meaning the adoption and abandonment of elements of organizational form and theories of moral sentiments by established thrifts, or selection, meaning the birth of new organizations embodying novel theories of moral sentiments and the death of existing organizations embodying old ones? And *why* was a theory of thrift emphasizing mutuality and enforced saving replaced by one celebrating bureaucracy and voluntary saving?

## EXPLAINING THE COEVOLUTION OF THRIFT ORGANIZATION AND INSTITUTION

### Adaptation versus Selection

Figures 3 and 4 offer insight into the mechanisms by which the composition of the California thrift industry shifted from 1890 to 1928: adaptation or selection. Figure 3 plots the number of thrift births versus conversions by existing thrifts to new plans; figure 4, the number of thrift failures versus conversions.<sup>10</sup> There were 311 founding, 173 failure, and 83 conversion events. Counting each conversion twice (once as entry to a plan and once as exit from a plan) and adding foundings and failures, we see that selection events constituted 74% of the evolutionary process (484/650 events), while adaptation events constituted just 26% (166/650 events). Thus in this setting, the dominant engine of institutional and organizational coevolution was selection.

Figures 3 and 4 suggest unambiguously that selection dominated adaptation in this organizational population in terms of the *number* of evolutionary events. But these figures do not address another obvious question: Was adaptation more likely for larger thrifts and was selection mostly confined to the birth and extinction of small thrifts (Hannan and Freeman 1989, pp. 80–84)? Figures 5 and 6 address this question by plotting the *mass* of adaptation and selection events, weighting each event by thrift size as measured in terms of assets.<sup>11</sup> These figures show that a greater

<sup>10</sup> Although our data are rich, there are a few holes in the records. *Annual Reports* were not published in 1897, 1898, 1901, 1902, 1903, or 1904. We have exact birth and death dates, but not exact dates of form conversion. We used a random number generator with a uniform distribution to assign form-change dates to thrifts within the year (or, in the early part of our observation period, the half year) between records. If thrifts converted from one plan to another during a gap in our records, we randomly assigned change dates to some point during that gap.

<sup>11</sup> Following the lead of earlier researchers (e.g., Carroll and Huo 1986) we interpolated missing size data for each organization (missing due to gaps in the records) by re-

mass of investment in thrifts was involved in adaptation events than in selection events. Over the years 1890–1928, selection events involved only assets of \$112,662 (adding the sizes of all thrifts born and dissolved), while adaptation events involved assets of \$216,960 (including the size of each conversion twice, once as exit from a plan and once as entry to a plan). So if we consider *organizational scale*, adaptation accounted for two-thirds of the thrift assets involved in vital events, while selection accounted for one-third.

Current thinking and a growing body of empirical evidence suggest complex causal relationships between adaptation and selection (Singh, House, and Tucker 1986; Miner, Amburgey, and Stearns 1990; Kelly and Amburgey 1991; Haveman 1992, 1993; Amburgey, Kelly, and Barnett 1993; Barnett and Carroll 1995). Whether adaptation increases or decreases the likelihood of subsequent failure depends on the kind of change undertaken (core or peripheral), the circumstances that prompt change (incremental or radical environmental shift), and characteristics of the organization itself (size, age, ties to other organizations). Moreover, some researchers distinguish between process effects (always positive) and content effects (positive or negative) of change on failure (Amburgey et al. 1993; Barnett and Carroll 1995). To the extent that adaptation precipitates or prevents failure, comparing the frequency of births, deaths, and transformations is misleading. This simple calculus places too much weight on birth and death and insufficient weight on change, because it ignores the second-order consequences of change; that is, it weights the second-order consequences of change at zero by assuming that change does not raise or lower the risk of failure.

To take this reasoning into consideration, we analyzed whether adaptation altered the chance of failure for thrifts. Using event-history methods (Tuma and Hannan 1984; Tuma 1993), we estimated models of organizational death of the form  $r(t) = \exp(\beta'x)$ , where  $r(t)$  is the hazard rate of death and  $x$  is a vector of explanatory and control variables. A dummy variable accounts for the initial effect of form conversion. Following previous research (Kelly and Amburgey 1991; Amburgey et al. 1993), we set this variable equal to "0" for thrifts that had not yet changed their plan and "1" for thrifts that had changed. We computed time since form conversion, to determine whether the effects of this change lessened as time passed. These variables were highly correlated ( $r = .76$ ) because there were few repeated form conversions; to avoid problems with multicollinearity, we included these variables in separate models. Our controls included organizational and industry factors that may impel or impede fail-

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gressing size against time. We used the TRANSREG procedure (SAS Institute 1994) to perform this nonlinear interpolation.



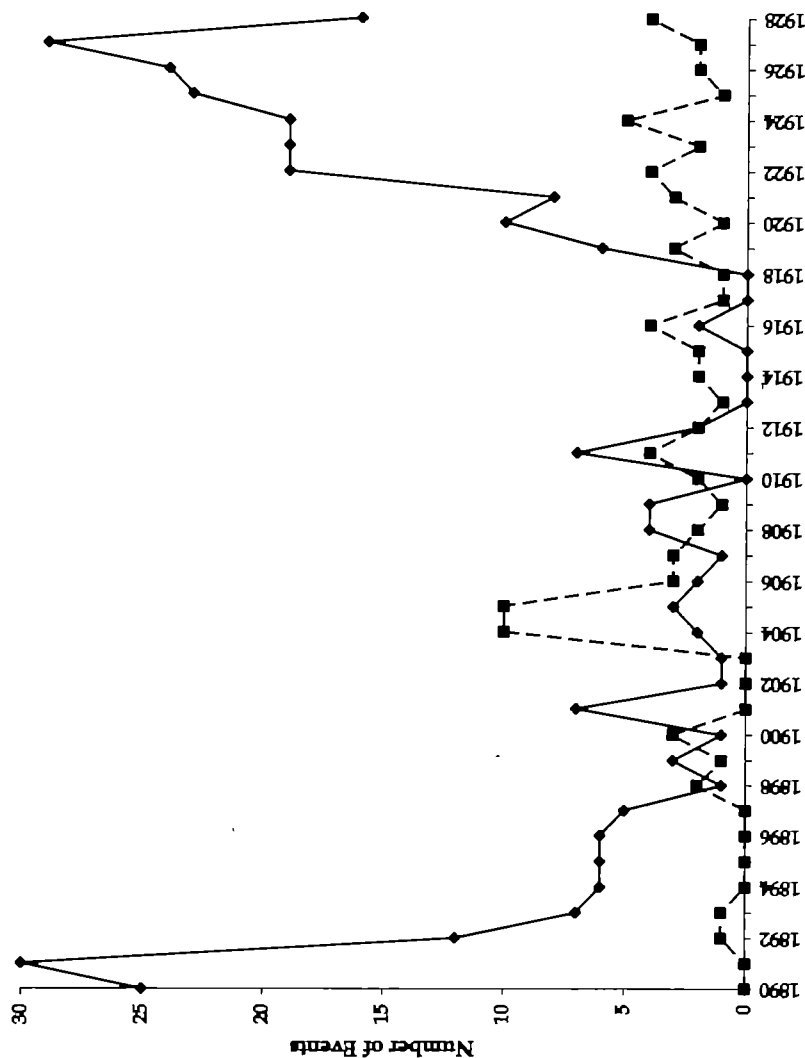


FIG. 3.—Foundings and transformations in the California thrift industry, 1890-1928



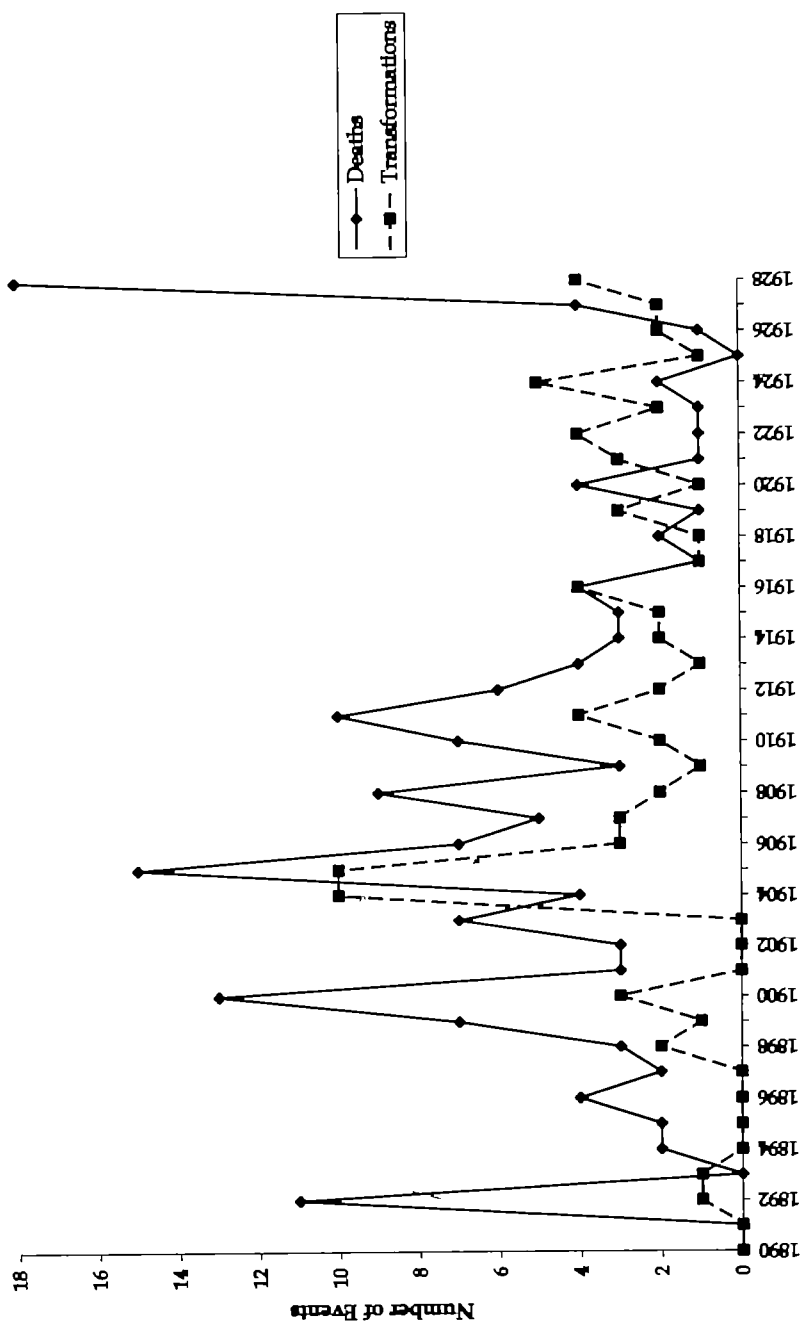


FIG. 4.—Deaths and transformations in the California thrift industry, 1890–1928

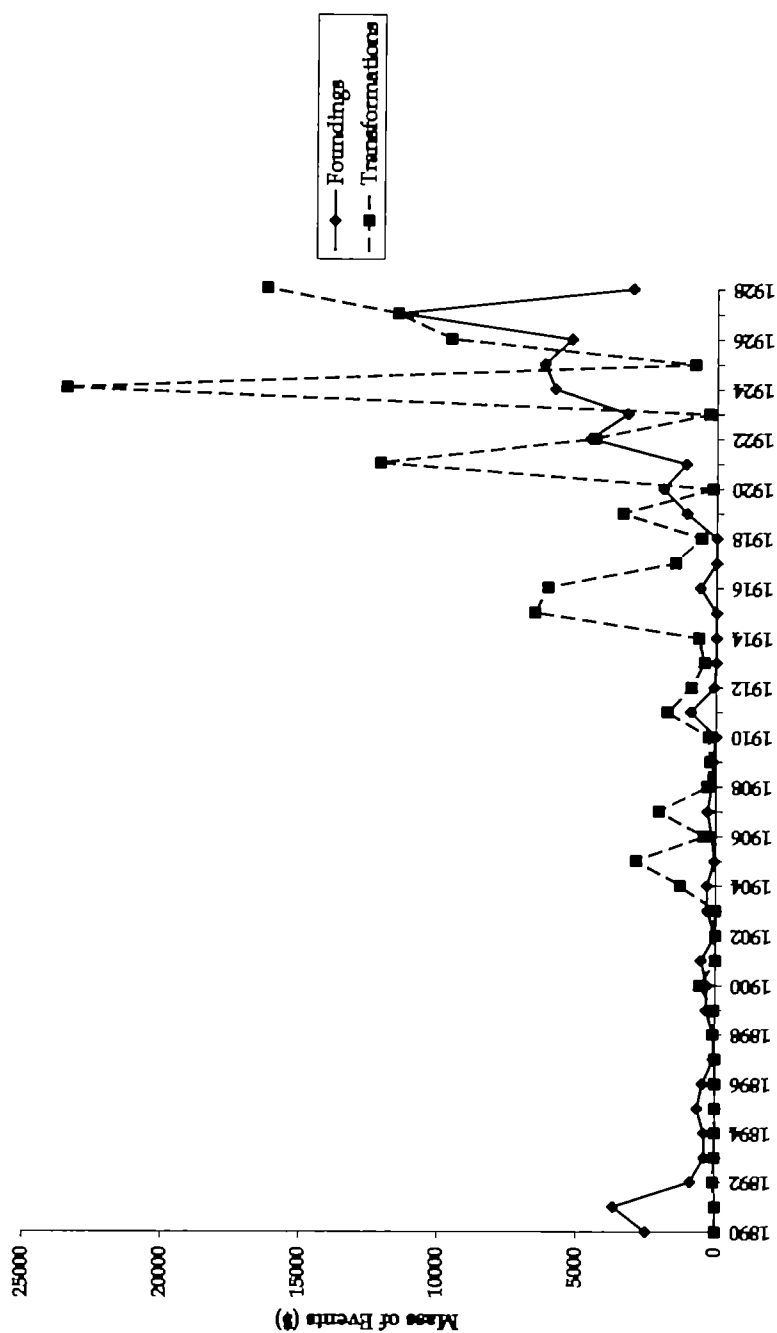


FIG. 5.—Mass of founding and transformations in the California thrift industry, 1890–1928 (no. of events weighted by size)

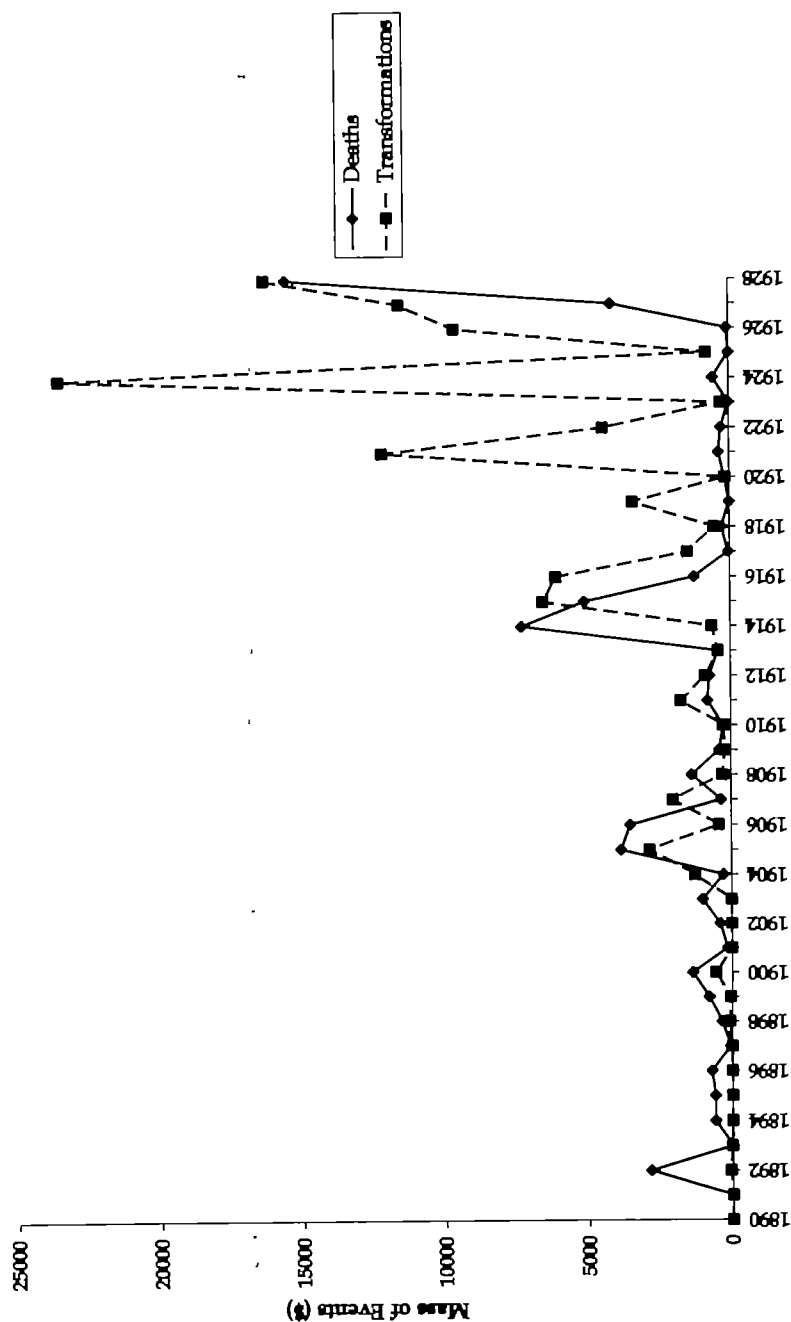


FIG. 6.—Mass of deaths and transformations in the California thrift industry, 1890–1928 (no. of events weighted by size)

ure: age (years), size (assets), five indicator variables for plan (five plans with low density—terminating, guarantee stock, permanent/guarantee stock, and serial/Dayton—were combined into one category), number of thrifts (density) with the focal thrift's plan each year, and the aggregate size of thrifts (mass) with the focal thrift's plan each year. We divided the data into one-year spells, including one record per thrift for each year of its life, so that we could update the values of independent and control variables annually. We lagged all explanatory variables by one year to ensure their temporal priority.

Table 4 shows that adaptation (form conversion) did not significantly alter the hazard of death for thrifts: the parameter estimate for the form-change dummy is nonsignificant. But, over time, the effect of form change became beneficial: the parameter estimate for time since form change is negative, as expected, and marginally significant ( $P < .08$ ). These results suggest that adaptation indirectly attenuates selection, although the effect is weak in magnitude and significance. On average across our sample, converting from one form to another reduced the risk of failure by 1%.<sup>12</sup>

These results indicate that our analysis of simple event counts reported above truly reflects the relative impact of adaptation versus selection for the evolution of this organizational population. Even after taking into consideration the second-order consequences of adaptation, selection predominates.

### Why Did Selection Predominate?

There are two explanations—one institutional, one technical—for why thrifts' theories of moral sentiments and their organizational embodiments coevolved primarily through selection. The institutional explanation hinges on the nature of thrift plans themselves. Each thrift plan was infused with value beyond the technical requirements of the financial-intermediation task at hand; each plan embodied a particular theory that reflected in varying ways the values of mutuality and structured effort. Adapting to a new plan required accepting new beliefs about how a thrift ought to operate and new ideas about what a thrift's goals and scope of authority ought to be. For example, converting from the serial to the Dayton plan entailed shifting from an association comprising many cohorts

<sup>12</sup> The multiplicative effect of any variable  $x$  on the rate of organizational death is  $\exp(\beta x)$ , where  $\beta$  is the focal parameter estimate. The effect estimate on the clock counting time since form conversion is negative, so thrifts' likelihood of failure declined at an increasing rate as time passed after conversion. The average observed time since conversion was 1.1 years. Therefore, for the average thrift, the multiplier of the death rate following change was  $\exp(-0.07 \times 1.1)$ , or 0.992.

TABLE 4

THE IMPACT OF FORM CONVERSION ON THRIFT FAILURE RATES

	MODEL		
	1	2	3
Constant .....	-3.60*	-3.88*	-3.89*
	(.472)	(.519)	(.505)
Dayton plan .....	-.140	.214	.219
	(.380)	(.454)	(.427)
Permanent plan .....	.891	1.14*	1.10*
	(.493)	(.524)	(.509)
Dayton/guarantee-stock plan .....	-1.23*	-.942*	-.984*
	(.535)	(.576)	(.558)
Serial/guarantee-stock plan .....	-1.28	-.843	-.995
	(.105)	(1.09)	(1.06)
Other plans .....	.764	1.15*	.978
	(.636)	(.692)	(.643)
Age .....	.009	.017	.018
	(.011)	(.012)	(.012)
Size/1,000 .....	-.172	-.155	-.145
	(.129)	(.130)	(.125)
Plan density/100 .....	-.249	-.083	-.086
	(.349)	(.369)	(.362)
Plan mass/10,000 .....	.101*	.087*	.090*
	(.035)	(.037)	(.036)
Form conversion dummy .....		-.557	
		(.391)	
Time since conversion .....			-.071*
			(.041)
$\chi^2$ .....	25.71	27.89	29.61
df .....	9	10	10

NOTE.—The omitted thrift plan was the serial plan, which was the most prevalent plan during most of our observation period. The small plans (those with few adherents) included the terminating, guarantee-stock, permanent/guarantee-stock, and serial/Dayton plans. Nos. in parentheses are SEs of the parameter estimates.

\*  $P < .05$ , one-tailed test.

of members, spaced at regular intervals, to an organization in which members entered and exited at their own pace. It meant eliminating rigid payment schedules, fines for late payment, and the requirement that all savers also be borrowers. It meant accepting paid-up stock, which was similar to modern certificates of deposit and which therefore did not encourage accumulation of savings over time. Finally, converting from the serial to the Dayton plan entailed establishing interest rates for borrowers based on demand for loans rather than having borrowers bid for loans and pay

loan premiums. As this example illustrates, adjusting to a different organizational form involved repudiating strongly held norms (mutuality based on common bonds within cohorts, between borrower and saver, and among savers; the value of rigidly structuring members' efforts to save). Converting from one plan to another essentially entailed an ideological shift by thrift members akin to religious conversion or a scientific paradigm shift. In a similar vein, Hannan and Freeman (1989, pp. 53–65) argue that institutionalization serves as a segregating mechanism that creates boundaries between organizational forms: the more institutionalized the form, the more salient the difference between that form and others. It is more difficult to change from one form to a very different other form than to change from one form to a very similar other form, since different forms are representatives of incompatible institutions.

The technical reason for the predominance of selection is that many thrifts were small and inexpensive to establish and dissolve—they had few physical assets or other forms of specialized capital investment. Little would be wasted if small thrifts built on old plans were closed and new thrifts built on new plans were opened in their place. If this logic was sound, thrifts involved in selection events would typically be smaller than thrifts involved in adaptation events. To assess this possibility, we computed the average size of thrifts after founding, before failure, and before conversion. Entries to a plan through *de novo* birth and exits from a plan through failure tended to be smaller than entries through conversion: the average asset size of thrifts in the first year after birth was \$173; the average asset size in the year before failure was \$338; and the average asset size in the year before conversion was \$1,307. Our *t*-tests of differences between size at birth and at failure, on the one hand, and size at conversion, on the other, were highly significant ( $P < .003$ ). These results accord with our earlier analysis of the mass of adaptation versus selection events (figs. 5 and 6) and indicate that small thrifts were more likely to be subject to selection pressures (to be born *de novo* or fail outright), while large thrifts were more likely to undergo adaptation (to convert from one plan to another).

Taken together, institutional impediments to adaptation (normative and cognitive) and technical factors facilitating selection (low setup and exit costs) explain why selection events drove the coevolution of theories of thrift and their organizational incarnations. But this logic does not explain the *content* of coevolution; that is, it is silent on the issue of *why* the terminating plan was replaced by the Dayton/guarantee-stock plan. Why did the terminating plan and its descendent, the serial plan, fail? Why did the permanent, Dayton, or guarantee-stock plan not take root instead? The following section answers these questions.

The Content of Coevolution

Modernization killed the theory of thrift manifested in the terminating and serial plans and gave birth to a new theory embodied in the Dayton/guarantee-stock plan.<sup>12</sup> Modernization promoted expanded patterns of human interaction with the rise of ties over wider geographical areas and ties involving impersonal understanding (Hays 1980, p. 240). Two principal dimensions of modernization contributed to this development. First, advances in transportation and communication fostered immigration and internal migration and so fashioned a society of strangers. These demographic changes struck at the foundations of the terminating and serial plans. Second, the Progressive movement promoted values of "continuity and regularity, functionality and rationality, administration and management" as solutions to the problem of social order (Wiebe 1967, p. 295). Progressivism modernized conceptions of cooperation and saving and legitimated the Dayton/guarantee-stock plan's bureaucratic and voluntaristic theory of moral sentiments, enabling it to thrive.

*Demographic change and the demise of the terminating and serial plans.*—From the founding of the first U.S. thrift in 1831 to the 1870s, small-town life was America's norm. Wiebe (1967, pp. 2–4) provided the following cogent summary of mid-19th-century towns:

Usually homogeneous, usually Protestant, they enjoyed an inner stability that the coming and going of new members seldom shook. Even when new towns were established in fresh farm country, the gathering families brought the same familiar habits and ways so that a continuity was scarcely disturbed. . . . If there was an American philosophy of the seventies, it was a corrupted version of Scottish common-sense doctrines, taking as given every man's ability to know that God had ordained modesty in women, rectitude in men, and thrift, sobriety and hard work in both. People of very different backgrounds accommodated themselves to this Protestant code which had become so thoroughly identified with respectability.

A theory of thrift emphasizing cooperation and rigid structures to induce saving, embodied in the terminating and serial plans, was an effective solution in communities where people knew each other and valued frugality and hard work. Terminating- and serial-plan thrifts flourished as "a way to borrow rather than a way to lend, overcoming the problems of moral hazard and adverse selection by pooling the resources of acquaintances" (Rasmusen 1988, p. 415). In a similar vein, after reviewing detailed anthropological and sociological studies, Biggart et al. (1996) noted that

<sup>12</sup> We are concerned with explaining the mortality of plans rather than of individual associations; in particular, we are concerned with the mortality of the terminating plan rather than individual members of this plan. Although terminating-plan thrifts were mortal by design, the form was not.



TABLE 5  
POPULATION GROWTH IN CALIFORNIA, 1870-1930

Decade	Population at Beginning of Decade	Natural Increase (Births - Deaths)	Gain by Internal Migration	Gain by Immigration	Total Increase during Decade
1870-80 . . . . .	561,400	135,200	62,400	110,200	307,800
1880-90 . . . . .	869,200	93,300	137,200	118,400	348,900
1890-1900 . . . . .	1,218,100	94,000	113,800	65,400	273,200
1900-10 . . . . .	1,491,300	115,000	477,800	297,600	890,400
1910-20 . . . . .	2,381,600	170,200	595,300	285,200	1,050,700
1920-30 . . . . .	3,432,200	370,500	1,438,300	443,900	2,252,700
1930-40 . . . . .	5,685,000	. .	. .	. . .	. . .

SOURCE —Thompson (1955), p 25

rotating savings and credit associations, which closely resemble terminating- and serial-plan thrifts, comprise individuals who are financially stable and geographically immobile; these authors reasoned that social ties forged by stable economic status and geographic location are required to develop the high levels of trust found in such organizations. Like rotating savings and credit associations, terminating- and serial-plan thrifts relied on process-based trust (Zucker 1986); that is, they relied on the common background expectations of members to define, consensually and automatically, the standards of reciprocity.

This stable, localized society was displaced between 1870 and World War I, as the railroad, telegraph, telephone, automobile, newspaper, and airplane greatly diminished geographic distance by reconceptualizing physical space as time (Trachtenberg 1982). These advances in communication and production technologies promoted migration from countryside to city, migration across the United States (especially to California), and culturally heterogeneous immigration from Europe.

Table 5 shows that internal migration and immigration accounted for 56% of the increase in California's population in the decade 1870-80; by 1920-30, these processes accounted for 84% of the population increase (*Historical Statistics of the United States* 1976). Internal migration and immigration transformed California into a society of strangers. As newcomers flooded the state, the neighborhoods that sustained social action were eroded. Individuals shared insufficient history to develop strong mutual trust. Zucker (1986) argued that trust built on past exchange or expected future exchange (process-based trust) was disrupted by immigration and internal migration and was replaced by trust tied to formal social structures, such as organizations, professions, and mediating technologies

(Institution-based trust). In sum, between the 1870s and the 1900s, casual cooperation among friendly acquaintances was eroded and so was no longer available to underpin collectivized agency relations such as those embodied in terminating- and serial-plan thrifts; instead, permanent and rationalized systems of administration were required.

If immigration and internal migration undermined the terminating- and serial-plan models of mutual cooperation among friendly acquaintances, the growing prevalence of irregular employment undermined their shared feature of rigidly enforced saving. There are no data on the extent of regular employment in California; the closest proxy is the number of people on the payroll of manufacturing establishments. In 1890, of the total state population of 869,200, only 84,101 (9.6%) were employed in regular jobs in the manufacturing sector. By 1929, the state population had risen to 5.53 million and only 353,000 (6.4%) were employed in regular jobs in the manufacturing sector (*California Statistical Abstract* 1970). People who held jobs for short durations could not join associations that insisted on regular payments and that levied fines to punish late payments; they needed associations that offered flexible savings schedules.

*Institutional conflict and aborted alternatives to the serial plan.*—The technical requirements that were imposed by a transient and heterogeneous population in need of ways of saving to purchase homes induced entrepreneurs to craft new organizational solutions. These innovations undermined mutuality, enforced saving, or both; in doing so, they attracted opposition from defenders of spontaneous cooperation rooted in a small-town ethos.

The *permanent plan* emerged in the 1880s as one of the first incremental modifications of the serial plan and was designed to solve many of its technical problems.<sup>14</sup> In these associations, separate accounts were maintained for each shareholder rather than for each series of shares; the shares of each member were initiated and matured without regard to the shares of other members. For the organization, the permanent plan was more flexible with respect to the timing of financial flows, as it made possible the continuous subscription of new shares. For members, however, it was only slightly more flexible, as it retained uniform payment schedules and late-payment penalties. Although this plan did not suit individuals with irregular incomes, it accommodated those who wished to save and not to borrow.

One unanticipated consequence of the permanent plan's less cumbersome account books was that this type of association could grow far larger

<sup>14</sup> A plan of operation for permanent associations had been developed in South Carolina just before the Civil War (Walker and Cox 1852), but it failed to grow or diffuse in the wake of Reconstruction.

than could any serial-plan thrift. This fact allowed thrift organizers to expand beyond the boundaries of a single town, city, or state. The permanent plan, therefore, was the basis of the national movement that spread across the country in the 1880s and 1890s. Members of national thrifts made monthly payments that were allocated to an expense fund, an insurance fund (to make payments in case of members' deaths), and a loan fund for disbursement of loans. Shares could not be surrendered or withdrawn before maturity, and if payments were not kept up, the delinquent member lost everything. The expense fund and forfeitures for nonpayment enhanced organizers' profits. National thrifts attracted the attention of speculators at a time when schemes designed to make promoters and their agents rich were rampant (Bodfish 1931, p. 100). Many nationals operated like Ponzi schemes, paying out interest and making loans to one group of members by using the savings of other, subsequent groups of members. And nationals often made loans by mail without having association officers inspect property or investigate creditworthiness; hence, borrowers with poor reputations who could not secure loans from local associations became nationals' main customers (Clark and Chase 1925, pp. 465–66).

The organizers of locals (mostly serial-plan associations) attacked nationals for diluting mutuality and exploiting structured effort for unworthy ends. One contemporary author (Winter 1890, pp. 6–7) averred that nationals were “not cooperative,” and characterized them as “diabolical.” One of the principal tasks of the U.S. League of Building and Loan Associations, formed in 1893, was to oppose the nationals. Its president, Judge Seymour Dexter, attacked the nationals vigorously (*Proceedings of the United States League* 1894, pp. 34–35, quoted in Bodfish 1931, p. 107). Managers of both types of thrift instigated collective action, establishing peak associations and lobbying state legislatures. Regulatory agencies arose to bar the entry into many states—including California—of associations with headquarters outside the focal state (Clark and Chase 1925, p. 470). Other regulations required a deposit to guarantee performance of contracts made in the state (Clark and Chase 1925, pp. 470–71). California, for example, required a deposit of \$100,000 in local mortgages, which effectively prohibited new national associations from entering that state. In 1891, the California State League lobbied the board of banking commissioners to investigate nationals. The commissioners strongly disapproved of nationals (*Report on National Building and Loan Associations* 1892, quoted in Bodfish 1931, p. 114).

Eventually, the national variant on the permanent plan failed. Real estate values crashed across the country in 1893, and all lenders—locals and nationals alike—were hit hard; however, nationals were hit harder than locals, since their loans constituted higher percentages of property value (up to 75%, rather than the 50% maximum loan percentage for

locals) and were often made on properties of unknown value and to borrowers with shaky creditworthiness. The demise, in the spring of 1897, of one of the largest nationals destroyed the credibility of the permanent form. After this, all permanent-plan associations, local and national alike, were viewed as speculative.

The debacle of the national (permanent) associations jeopardized the legitimacy of all thrifts. Some institutional entrepreneurs felt that asking founders to inject resources in the form of a guarantee-stock or contingent-reserve fund would instill confidence in skittish depositors. Although the first *guarantee-stock thrift* was established in 1884 in Minneapolis, its appeal widened after the crash of 1897 (Bodfish 1931, p. 116). Observers of the thrift industry noted: "One of the reactions . . . against the old national association of the 90's was the lack of public confidence in the . . . mutual plan. . . . It was felt that these mutual associations could not be safe because they had no reserve. It was thought advisable to inaugurate a plan that would restore confidence in the associations" (Clark and Chase 1925, p. 50). The financial cushion provided by guarantee stock proved essential to the repair of local thrifts' credibility after the demise of the nationals (Clark and Chase 1925, p. 47).

Despite its many technical advantages, the guarantee-stock plan did not thrive. A debate over the issue of mutuality ensued, because all stockholders did not receive a proportionate share of profits (Clark and Chase 1925, p. 59). The novelty of guarantee-stock thrifts and their departure from pure mutuality aroused public concern. One defender noted: "Those who do know conditions may criticize. To many, some of our associations seem irregular and unorthodox. But any institution which teaches thrift and loans its money to promote home getting is justified, provided it operates upon ethical standards" (Myers 1921, p. 555). Its "irregular and unorthodox" nature dissuaded potential founders from treating the guarantee-stock plan as a natural or acceptable way of organizing.

The *Dayton plan* emerged as an entrepreneurial solution to facilitate thrift for people with irregular employment and unsteady income. In doing so, it eroded mutuality and eliminated structured saving entirely. The Dayton plan was somewhat less mutual than early plans because some shareholders were only savers, while others were savers and borrowers, and because individual accounts made possible individual entry and exit, which reduced members' temporal interdependence. This was the first plan to replace the term *member* with the term *shareholder* in its articles of incorporation. The two types of savings accounts, paid up and installment, represented another bifurcation of roles, this time among savers, and further reduced mutuality. Finally, this plan represented a radical new voluntaristic theory of individual effort and imposed far less structure

on members than any of its predecessors. Hence, this plan was well suited to the social and economic climate of California.

Despite its technical advantages, the Dayton plan came under attack because it lacked the character-forming quality of enforced, persistent, systematic saving. The morality of making saving easy was questioned by managers of established thrifts; in response, Dayton-plan advocates argued that late-payment penalties imposed in other plans discouraged saving and so worked against thrifts' espoused goals. The Dayton plan also suffered from the stigma of being "merely a *bank*" (Clark and Chase 1925, p. 57; emphasis in the original), because relationships between members were closer to those between debtor and creditor than to those between cooperating peers (Clark and Chase 1925, p. 49). The charge of being a bank was especially deadly because during the Panic of 1907, banking was vilified by muckrakers (Wiebe 1962, pp. 186–88) and the yellow press—so much so that the "name of a bank" was "synonymous with all that is dishonest, corrupt, and criminal" (contemporary account cited in Wiebe [1962], p. 188). In sum, because it was viewed as a bank and therefore foreign to thrifts' original theory of moral sentiments, the Dayton plan never thrived.

*Progressivism and the rise of the Dayton/guarantee-stock plan.*—By 1928, the Dayton/guarantee-stock plan was the dominant thrift form. How did this plan succeed when it drew on ancestors whose moral worth was controversial? From a technical perspective, this plan was a practical solution to the problem of saving in a society of transients: it allowed members the option of saving and not borrowing, made individuals its building blocks, and had a specialized managerial cadre. The plan's flexible savings system (no fines for late or irregular payment) was also well suited to the needs of people with irregular incomes. Moreover, this plan was premised on impersonal, institution-based trust rather than personal, process-based trust (Zucker 1986; Shapiro 1987): interactions among members relied on standardized formal structures and stable rules. The Dayton part of this plan generated trust among members by appealing to well-understood rational-bureaucratic procedures and arguing in efficiency terms. And the guarantee-stock part of this plan relied on the existence of a nonwithdrawable fund to create trust among installment shareholders. In their textbook on how to organize a thrift, Clark and Chase (1925, pp. 57–58) summarized the Dayton/guarantee-stock plan's advantages so:

This plan makes it possible to reach greater numbers of savers, since it brings in both periodic and intermittent accumulation. The safety and profitableness [*sic*] of building and loan shares are made available to all who are in a position to save to any extent and in any manner . . . [Its

advantages] are best appreciated and understood in the West where people are strange and strangers; in territory where few men of capital and leadership seem to have the time and inclination to promote a form of cooperative financial institution, which promises little reward in either cash or appreciation.

Technical advantages were a necessary but not sufficient condition for the growth of the Dayton/guarantee-stock plan; it also had to become institutionally appropriate. The rise of this plan was made possible by modernized conceptions of cooperation and saving rooted in the Progressive movement. As Progressive ideas and values gained ground after the turn of the century, cooperation evolved from a spontaneous act between friends to a bureaucratized relationship among strangers; saving shifted from the outcome of external controls to the outcome of voluntary and rational decisions by members. The formalization of government provided an impetus to the proliferation of impersonal mechanisms that generated trust among strangers (Zucker 1986). The proponents of Progressivism sought to use impersonal mechanisms and the principles of continuity, efficiency, reliability, and service to transform government. As these ideas gained ground, they were reflected in the impersonalization of many forms of economic organization, including thrift (cf. Creighton [1995, 1996] on the corporate form). For instance, political reform shifted from simple moral principles to complex procedural principles, social work was transformed from a moral act into systematic case work, philanthropy was routinized by foundations, behaviorism became ascendant in the social sciences, and industrial engineering gained ground. Observers exulted the growth of formal, rational-bureaucratic organizations: "We are living in an age of organization, an age when but little can be accomplished through organization; an age when organization must cope with organization, an age when organization alone can preserve your industrial freedom and mine" (Kirby 1906). This bureaucratic orientation reached its peak just after World War I (Wiebe 1967).

A useful indicator of support for Progressive ideas is the extent of electoral support received by Progressive candidates—who sought to promote a civil service of trained professionals who would oversee the efficient operation of public bodies—in gubernatorial, senatorial, and presidential elections.<sup>15</sup> Table 6 shows how Progressive candidates fared in the elections from 1910 to 1926 across California and demonstrates clearly that Progressivism was a vibrant force in that state, especially between 1906 and 1922 (see also Link 1959).

The modernizing temper of Progressivism was reflected in the Dayton/guarantee-stock plan. The Progressive movement supported bureaucra-

<sup>15</sup> We thank John Meyer for this suggestion.

TABLE 6  
THE SHIFTING BASIS OF PROGRESSIVE STRENGTH IN CALIFORNIA: PERCENTAGE OF VOTES TO PROGRESSIVE CANDIDATES

	1910	1912	1914	1916	1922	1924	1926
	Gubernatorial Primary (Johnson)	Presidential Primary (Roosevelt and La Follette)	Gubernatorial Election (Johnson)	Senate Primary (Johnson)	Senate Primary (Johnson)	Presidential Election (La Follette)	Gubernatorial Election (Young)
Farming counties .....	63.7	77.5	41.9	51.1	64.4	36.9	44.0
Urban counties .....	46.6	76.3	51.7	52.3	60.9	30.2	45.9
Immigrant counties .....	49.1	68.7	47.8	53.9	64.1	39.8	49.9
Native stock counties .....	60.3	77.4	43.5	48.4	64.4	35.8	42.4

SOURCE.—Rogin (1968), p. 306.

NOTE.—Farming counties are defined as those in the top quartile of counties with respect to the percentage of farm dwellings (31%–46%); urban counties comprise 11 counties that were more than 50% urban in 1910, immigrant counties comprise 16 whose population of foreign-born residents and their children was over 50%, and native-stock counties comprise 14 whose population of white Americans of native stock was over 60%.

tized cooperation among strangers, such as was found in this type of thrift. Moreover, by making saving a voluntary act, this plan reflected the belief that individuals were rational decision makers. The legitimacy of this plan depended on the legitimacy of its institutional building blocks, and this grew over time with the expansion of Progressivism (Meyer and Rowan 1977; Creighton 1995, 1996). This plan grew slowly after 1908, when it received legal certification from the California State Legislature, and exploded in the 1920s. By 1928, this plan and its attendant theory of thrift had garnered enough adherents to become the dominant social fact in the thrift landscape.

*Summary.*—The rise of a transient and heterogeneous population and the bureaucratic spirit that attended Progressivism contributed to the downfall of thrifts based on mutuality and enforced effort and the rise of thrifts celebrating bureaucracy and voluntary effort. The growth of a heterogeneous population with irregular employment created pressure to build thrifts around individuals rather than communities and to make saving flexible and voluntary rather than inflexible and forced. The Progressive movement imparted moral and theoretical cogency to a practical solution of bureaucratizing cooperation and allowing rational decision makers the freedom to save as they wished.

## DISCUSSION AND CONCLUSION

This article was motivated by a desire to study the genesis and functioning of institutions in organizations. Accordingly, we analyzed how various forms of thrift institution were built up and destroyed in tandem with the organizational forms that rendered them concrete. We first showed that the replacement of the terminating plan by the Dayton/guarantee-stock plan was an incremental process in which guiding principles of mutuality and enforced saving were gradually dismantled and replaced by guiding principles of bureaucracy and voluntary effort. We also revealed that the succession of the terminating plan by the Dayton/guarantee-stock plan was driven primarily by selection (births and deaths) rather than adaptation (conversions from one plan to another). Finally, we showed that the theory of moral sentiments embodied in the Dayton/guarantee-stock plan became dominant not only because it was a technically efficient answer to shifting human demography (bureaucratic control facilitates trust among strangers; voluntary effort meets the needs of transient workers with intermittent employment) but also because it was institutionally appropriate in the wake of the Progressive movement (bureaucratic control is modern; voluntary effort is rational).

Our study holds lessons for two traditions in the sociology of organizations, the new institutionalism and organizational ecology. Although



scholars have often called for the integration of these traditions, most integrative work has imported concepts from the new institutionalism into organizational ecology, such as the many studies that demonstrate how organizational legitimation and ties to institutional actors influence the vital rates of organizations (e.g., Miner et al. 1990; Hannan and Carroll 1992). To date, much less has been done to advance the new institutionalism by introducing ideas from organizational ecology. Hence, organizational ecology has benefited from this exchange but the new institutionalism has not. By examining how institutions and the organizational forms that embody them coevolve, we have constructed a two-way flow of ideas between the new institutionalism and organizational ecology, a flow that benefits both perspectives.

*Benefits for the new institutionalism.*—Although much has been done to explain how institutions in organizations arise and diffuse, previous research has revealed little about the process by which institutions in organizations fall into disfavor (but see Oliver 1992; Davis et al. 1994). Our study fills this gap by demonstrating empirically how various forms of thrift institution rose *and* fell as their organizational incarnations (i.e., thrift plans) emerged, grew, and disappeared.

Our research also offers evidence to resolve a debate concerning the complex relationship between technical and institutional processes. The new institutionalists first held that technical pressures (the efficiency or effectiveness of newly developed structures or procedures) and institutional pressures (the legitimacy of the rules and normative expectations that underlie structures and procedures) exert contradictory effects on organizations (Meyer and Rowan 1977; DiMaggio and Powell 1983). Later, however, they acknowledged that these pressures may generate convergent effects on organizations, as technical criteria are disputed by institutional entrepreneurs and technical choices are constrained by broad institutional frameworks (Powell 1991). Our study focused on an industry that was characterized by *both* strong technical pressures for efficiency and strong institutional pressures for legitimacy. Although technical pressures created by internal migration, immigration, and the scarcity of permanent jobs called for more formalized structures to generate trust and more flexibility than was possible in the terminating and serial plans, attempts at modifying these plans were blocked by institutional pressures. The principle alternatives to the terminating and serial plans—the permanent, guarantee-stock, and Dayton plans—eroded the keystones of mutuality and enforced saving. Hence proponents of serial-plan thrifts battled defenders of permanent-plan thrifts (nationals) by lobbying for legislative restrictions and launching public attacks, condemned the guarantee-stock plan as an anathema to the principle of mutuality, and disparaged the Dayton plan as “a mere bank” because it relied on voluntary saving and

abandoned the principle of structured effort. Moreover, ideological conflict made it difficult for established thrifts with terminating and serial plans to convert to new plans, because conversion involved the abandonment of dearly held theories of thrift. Our analysis revealed, however, that the institutional environment did not exert a single consistent force to thwart alternatives to the serial and terminating plans. If institutional entrepreneurs seeking to defend small-town America thwarted the rise of some plans, the modernizing temper of the Progressive movement legitimated bureaucratized conceptions of cooperation and voluntary saving, thereby propelling the rise of the Dayton/guarantee-stock plan. Thus, basic technical choices were altered by an emerging general institutional framework.

*Benefits for organizational ecology.*—Our study addresses the issue of whether adaptation or selection predominates; that is, whether the evolution of organizational populations comes about through prompt adjustment of the structures and activities of flexible organizations to suit shifting environmental conditions (Aldrich and Pfeffer 1976) or whether inertia is so strong that organizations cannot change without great cost or in a timely fashion and the members of one stable organizational form are replaced by the members of another (Hannan and Freeman 1989, pp. 66–90). Recent thinking and evidence suggest that organizational evolution reflects *both* adaptation and selection (e.g., Baron, Mittman, and Newman 1991; Amburgey et al. 1993; Levinthal 1997). We continued in this vein and provided several important advances to understanding organizational evolution. First, we demonstrated that the creation of new organizations and the death of old ones (selection)—not the restructuring of existing organizations (adaptation)—was the primary engine of organizational evolution in the early thrift industry. Second, we explained why evolution occurred primarily through selection and showed how selection processes in a population reflected the wider institutions in society. Thrifts were generally small and had little specialized capital, so there was no great benefit gained by preserving individual associations through conversion. Moreover, thrifts' theories of moral sentiments were too rigidly ethical in nature to permit easy conversion. Although technical pressures stemming from demographic changes rendered terminating- and serial-plan thrifts inefficient, defenders of small-town mores thwarted endeavors to diffuse alternative designs. Eventually, the rise of Progressivism made possible the acceptance of the Dayton/guarantee-stock plan, which bureaucratized cooperation and saving. In sum, we demonstrated the importance of understanding *both* the institutional and the technical factors that shape the mode and trajectory of organizational evolution.

Most previous studies of organizational evolution have been at the organizational level (see Baum [1996] for a review). These studies have exam-

ined the founding, transformation, and failure of individual organizations, but have not determined the combined impact of these organization-level processes on the population-level processes of adaptation and selection. To remedy this, our study shifted to the population level of analysis. Only aggregate, population-level analysis that considers all three organization-level processes *simultaneously* can reveal to what extent the restructuring of existing organizations (adaptation) and the creation of new organizations and the death of old ones (selection) are competing modes of evolution. Our research built on recent work that studies relationships between adaptation and selection (e.g., Haveman 1992; Amburgey et al. 1993). But our research differed from this work in that we emphasized tracing the consolidated effects of adaptation and selection on entire organizational populations, rather than on individual organizations.

Although thrift plans were generally inert and incapable of timely adaptive changes, institutional entrepreneurs proved adept enough to mobilize resources to build novel forms to replace what was selected out. That is, just as masters deploy their tools to create works of art, institutional entrepreneurs in the thrift industry deployed organizational forms to create collectivized agency relations. In bringing this to light, we address a fundamental criticism of organizational ecology, namely that the theory glosses over the primary source of action and change in organizational populations—organizational entrepreneurs and designers.

*Future research.*—Some limitations of this study deserve elaboration and point to future research possibilities. First, this study examined institutional change in just one industry and just one location in the United States. Comparative studies of thrifts in multiple locations (e.g., Biggart et al. 1996) and studies of many other societal sectors are needed to more fully understand the coevolution of institutions and organizations. Second, this study focused on a sector characterized by strong technical and institutional pressures; we did not venture into sectors that experience weak technical or weak institutional pressures. It may be that adaptation dominates selection in environments characterized by strong technical but weak institutional pressures because such settings involve relatively few cognitive, regulatory, or normative impediments to change. By the same logic, it may be that selection dominates adaptation in environments characterized by strong institutional but weak technical pressures because such settings contain many impediments to change. Moreover, it may be that environments characterized by weak technical pressures will see less change of any kind (less adaptation *and* less selection) than sectors characterized by strong technical pressures, to the extent that technical requirements provide the primary impetus for organizational change efforts. Comparative studies of such settings are clearly warranted if we are to construct a comprehensive theory of organizational and institutional coevolution.

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# Sex Segregation and Occupational Gender Inequality in the United States: Devaluation or Specialized Training?<sup>1</sup>

Tony Tam

*Academia Sinica*

This article examines two hypotheses of the wage effects of occupational sex composition in the United States: the devaluation and the specialized human capital hypothesis. With data from an expanded version of the May 1988 Current Population Survey, this study finds that differences in the length of specialized training across occupations and industries, together with a few demographic and human capital attributes, are able to completely explain all of the sex composition effects among women and men, whites and blacks. The central results are difficult to reconcile with the devaluation hypothesis but are remarkably consistent with the specialized human capital hypothesis.

## I. INTRODUCTION

Gender inequality in the labor market is a complex subject that raises many issues. Two big questions have preoccupied research in this area for the past 15 years: (1) Why are men and women occupationally segregated? (2) Why do occupations with larger numbers of female workers

<sup>1</sup> This article is dedicated to the dear memory of James S. Coleman—a generous mentor, an intellectual giant, and a warrior with impeccable integrity. An earlier version of this article was to be presented in a seminar at Jim's home on March 27, 1995. Sadly, Jim lost his battle with cancer two days before the seminar. I am indebted to Paula England and the *AJS* referees for extensive and helpful comments. For useful discussions and comments on earlier drafts, I thank Chin-fen Chang, Ly-yun Chang, Tom DiPrete, Claudia Goldin, Stanley Lieberman, Chin-ming Lin, Aage Sørensen, Donald Tomaskovic-Devey, Chris Winship, and many participants at the 1995 meeting of the Society for the Advancement of Socioeconomics (SASE), the 1995 meeting of the American Sociological Association, both in Washington, D.C., and the 1995 spring conference of Research Committee 28 of the International Sociological Association, Zurich. Ronald Chen, Amy Luckey, and I-feng Hsu provided research assistance. This research was supported with grants from the Faculty of Arts and Sciences at Harvard University and from the Central Advisory Committee on Research at Academia Sinica. Direct correspondence to Tony Tam, Institute of European and American Studies, Academia Sinica, Taipei, Taiwan. E-mail: tam@sinica.edu.tw



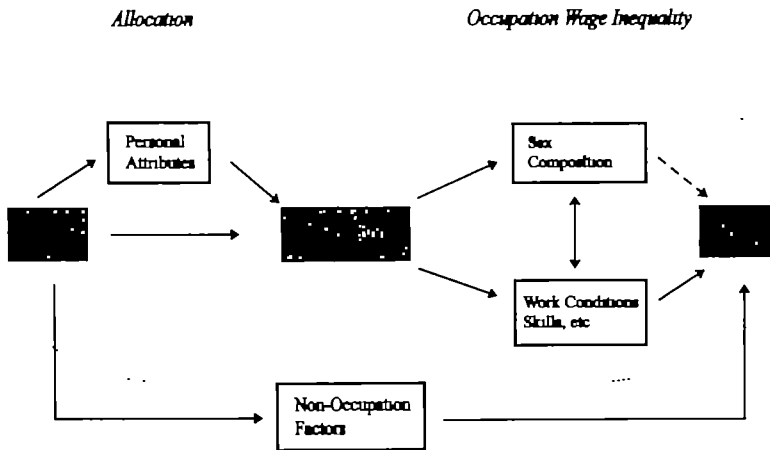


FIG. 1.—Allocation, occupational wage, and gender inequality

pay less? The two questions are related but logically distinct. Figure 1 schematically locates the two questions within the context of gender wage inequality.<sup>2</sup> The first question focuses on the linkage between gender and occupation and is concerned about the allocation of men and women across occupations. Whether the allocation process is plagued by gender discrimination is of course a central issue of lawsuits, policy debates, and scholarly research.<sup>3</sup> The second question addresses the linkage between occupation and wages. In particular, the question is concerned about occupational gender inequality—the sources of the apparent occupational sex composition effects on wages. Whether occupational allocation is free from gender discrimination or not, the sex composition effects suggest that occupational wage inequality may reflect subtle gender discrimination. Instead of wage discrimination against *female workers*, gender discrimination may take the form of wage discrimination against *female work*.

<sup>2</sup> The figure is just a heuristic device to stress what the article is and is not able to address. It is not meant to represent any coherent causal theory about gender inequality. The important message is that the present study does not address the allocative question, nor does it address any of the nonoccupational factors that may explain gender wage inequality.

<sup>3</sup> Even when sex segregation is largely driven by sex discrimination, it remains to be empirically determined whether there is any discriminatory wage penalty against *any occupation*. It is hard to overemphasize the fact that segregation does not necessarily lead to wage inequality. This is one of the fundamental results in the economic theory of discrimination (see, e.g., Becker 1971; Arrow 1973; Stiglitz 1973). Attitudinal bias against women, even if widespread in a population, does not in itself lead to the persistence of economic discrimination against women in the labor market. Some kinds of segregation are associated with wage inequality while others are not.

While the first question of allocation is increasingly a major focus of sociological and economic research,<sup>4</sup> the second question of occupational gender inequality is far from settled and will be the focus of this study. The apparent wage effects of occupational sex composition is important because of its role in the theoretical debates of gender inequality and its critical importance as a support for comparable worth policies. For over a decade, the comparable worth movement has identified occupational gender inequality as a major source of gender inequity in the labor market (see, e.g., England 1992b). Irrespective of the policy role of the comparable worth movement,<sup>5</sup> the movement epitomizes the belief that there is widespread valuatative discrimination against female work. The main generalizable evidence for this belief is the finding that sizable wage effects of occupational sex composition cannot be explained by any stable worker or occupational characteristics (see, e.g., Bergmann 1974; Treiman and Terrell 1975; England and McLaughlin 1979; England, Farkas, Kilbourne, and Dou 1988; Sorensen 1990; England 1992a; Kilbourne, England, Farkas, Beron, and Weir 1994).

This study will address two competing hypotheses for the occupational sex composition effects: (1) the devaluation hypothesis and (2) the specialized human capital hypothesis. Conceptually, the devaluation hypothesis simply refers to a *general cultural devaluation of women's labor* (see, e.g., Reskin 1988; Baron and Newman 1989; England 1992a, 1992b; Bellas 1994; Kilbourne et al. 1994). If an occupation is predominantly done by women, *all* workers in the occupation are subject to the devaluation effect.<sup>6</sup> As an explanation of the occupational sex composition effects, the devaluation hypothesis asserts that no economic factors can fully explain the sex composition effects because cultural bias against women's labor, to a substantial extent, modifies or overrides market considerations. By contrast, the specialized human capital hypothesis claims that the apparent wage penalties against female occupations arise mainly from the occupational differences in specialized training. In essence, the specialized human capital hypothesis asserts that the same worker is expected to receive different wages for occupations associated with different training requirements for specialized human capital, which may be occupation specific, industry specific, or firm specific.

The rest of the article is organized as follows: Section II introduces the

<sup>4</sup> Much of the existing research is devoted to the explanation of sex segregation rather than the microallocative question of who gets into which occupation (see, e.g., Reskin 1993).

<sup>5</sup> For a critique of comparable worth policy as a tool for social change, see, e.g., Fischel and Lazear (1986).

<sup>6</sup> Section VI will consider other variants of the devaluation hypothesis.

two competing hypotheses. Section III offers a critique of previous research that is widely interpreted as supportive of the devaluation hypothesis. Section IV describes the data and analytic strategy of this study. Section V presents four main results that, in tandem, are hard to reconcile with the devaluation hypothesis but strongly confirm the specialized human capital hypothesis. Section VI takes stock of the theoretical implications of the findings and Section VII concludes the article.

## II. THEORETICAL BACKGROUND

### The Devaluation Hypothesis

Studies based on different samples, units of analysis, and statistical techniques almost always find the same general result: significant wage effects of occupational sex composition (hereafter, sex composition effects) remain after controlling for a wide range of personal attributes and occupational characteristics (see, e.g., Sorensen 1990; England 1992a).<sup>7</sup> If female occupations tend to receive substantially lower wages than male occupations without any apparent justification, then the wage penalties on female occupations may simply be a form of sex discrimination. The assignment of lower wages to occupations done mostly by women (hereafter, female occupations) may reflect a culture of discrimination against women's work, as suggested by research in the laboratory (e.g. Major, McFarlin, and Gagnon 1984) and by surveys of prestige ratings of occupations (e.g., Bose and Rossi 1983). The finding of a persistent sex composition effect is a crucial starting point for advocates of comparable worth policies (see, e.g., Treiman and Hartmann 1981; Johnson and Solon 1986). The typical comparable worth policy in the United States seeks to equalize wages for occupations (within a firm) that differ mainly in sex composition.

Whether one believes in the effectiveness of comparable worth policy or not, the robust sex composition effects remain a puzzle to be explained. Sociologists have tended to believe that the explanation lies in a *general cultural devaluation of women's labor*, which may be called the devaluation hypothesis.<sup>8</sup> While experimental (Deaux 1985) and nonexperimental

<sup>7</sup> Filer (1989) appears to be an exception: the sex composition effects for men and women seem to be attributable to a large number of occupational characteristics. However, due to various design problems in Filer's analysis (see, e.g., Smith 1989; England 1992a; and Macpherson and Hirsch 1995), the finding has not been well accepted by most economists and sociologists.

<sup>8</sup> A general devaluation does not, of course, imply a homogeneous devaluation throughout a population. Variation is expected. But the hypothesis does suggest that occupations with more female workers on average pay less than comparable occupations.

(Bose and Rossi 1983) evidence shows that people ascribe lower value to work performed mostly by women, it is not clear that such subjective valuation carries over to the wage-setting process in the labor market. The devaluation hypothesis suggests that no economic forces can fully explain the sex composition effects and that subjective valuations play a significant role. The cultural devaluation of women's work should give rise to a substantial level of unexplained sex composition effect.

The establishment of general empirical support for the devaluation hypothesis as an explanation of the sex composition effects essentially follows a "residual" approach. The more comprehensive are the theoretically relevant control variables and the larger are the unexplained (i.e., residual) sex composition effects among workers with comparable characteristics, the stronger is the evidence in favor of the devaluation hypothesis. However, advocates of the devaluation hypothesis are notably silent about how small a residual sex composition effect is small enough to cast doubt on the devaluation hypothesis. Nor is there any word on how large the contribution of economic factors to the gross sex composition effects is large enough to support the view that the occupational sex composition effects are driven by market rather than cultural forces. It is therefore unclear what it really takes to refute the devaluation hypothesis as an explanation of occupational sex composition effects. In practice, this ambiguity favors the defense of the devaluation hypothesis. To unambiguously disconfirm the devaluation hypothesis, a theoretically relevant set of variables should, among other things, fully account for all of the sex composition effects. This is a tough condition for any competing hypothesis to meet. (As we shall see, this tough condition can indeed be met—in fact, even tougher conditions will be met by the competing hypothesis examined in this article.)

### The Specialized Human Capital Hypothesis

Cultural bias is, to be sure, only one of the possible explanations of the wage effects of occupational sex composition. The most influential market theory of wage inequality is human capital theory (Becker 1975). Yet it is well known that schooling does not account for the apparent wage effects of occupational sex composition. Nor does any study show that other kinds of general human capital can explain the effects (see, e.g., England et al. 1988). However, it would be premature to conclude that human capital is irrelevant to the explanation of occupational gender inequality. General human capital is but one dimension of human capital. It is important also to consider the role of specialized human capital in accounting for the occupational wage inequality between male and female occupations.

In empirical research, specialized skills have long been classified along

two major dimensions: occupation and industry.<sup>9</sup> These two dimensions are, in a way, merely convenient tools to talk about the diversity of tasks and skills in the workplace. In reality, the classification of occupations and industries is designed to group together similar tasks, skills, and jobs and separate different ones. Without pretending that any of the existing classification schemes for the two dimensions are perfect in any sense, this study explores the extent to which these two dimensions of specialized human capital may explain the occupational gender inequality in the United States.

A hypothesis of specialized human capital can be formulated by drawing upon two basic ideas of human capital theory (Becker 1975). First, investment in any human capital is costly and thus has to be compensated to ensure its adequate supply. Just as employers have to compensate for workers' investments in general human capital required for their work, employers have to compensate for workers' investments in specialized human capital. Second, the wage premium for specialized human capital depends on the supply and demand for the particular kind of specialized human capital. The supply and demand for a skill are contingent on a wide range of factors. The investment cost of a skill is often an important factor.<sup>10</sup>

Although similar to general human capital with respect to the two central ideas above, specialized human capital is fundamentally different from general human capital. Investment in general human capital is costly, but investment in specialized human capital may be even more costly because the investor of specialized human capital faces an additional opportunity cost that may arise from forgone opportunities for re-

<sup>9</sup> Occupation is designed to capture differentiation by function, whereas industry captures differentiation by work setting and product. A third dimension of specialized human capital is the firm. In Becker's (1975) human capital theory, firm-specific human capital is an important concept. Given the limitations of the data, however, this study will not be able to investigate specifically the role of this kind of specialized human capital. We do know that firm-specific human capital by definition must vary across firms whereas occupation-specific human capital does not.

<sup>10</sup> With these two basic ideas, human capital theory may offer an alternative interpretation of a phenomenon often discussed in the comparable worth literature: some widely and culturally useful female skills often have low market values (Steinberg 1990; Kilbourne et al. 1994). Human capital theory makes a sharp distinction between market value and cultural/functional value. In line with the neoclassical tradition, human capital theory emphasizes that the functional importance of a skill alone does not determine its market value. Air is essential to human survival yet the market price of air is trivial. Similarly, to the extent that a skill is in abundant supply or requires little training cost, the market value of the skill may be well below that of a functionally less important but costly and scarce skill. Such a paradoxical situation is not an evidence of cultural bias because it would occur even in the absence of any gendered cultural bias in the labor market.

coupling one's investment. By definition of specialization, a worker is committed to accumulating a skill that has restricted applicability. By specializing in medicine, for instance, a medical student is restricting future means of recouping the investment cost. The student cannot expect to recoup the training cost by being a high school teacher, even if the medical doctor may one day find teaching in high school the most personally gratifying occupation. A medical doctor gives up virtually the entire investment in medical training if she or he quits the medical profession and enters the high school teaching profession. In general, therefore, specialized human capital tends to be *more costly* and to provide *higher return* than general human capital that requires the same amount of training time. Given this fundamental difference in general and specialized human capital, we expect that different social groups may reach different optimal mixes of general and specialized human capital. For instance, to the extent that labor market discrimination persists and systematically affects the potential returns to general and specialized human capital, a disadvantaged group may pursue a different mix than that of an otherwise comparable group.

We may now define the specialized human capital hypothesis as an explanation of the wage effects of occupational sex composition: occupational differences in the investment cost (and its correlates) of specialized human capital explain the apparent sex composition effects on wages.<sup>11</sup> Central to the hypothesis is the premise that the investment cost in specialized human capital is associated with both occupational sex composition and the market return to the specialized human capital. The devaluation hypothesis of course does not deny the existence of market forces in wage determination.<sup>12</sup> The key point of contention is whether the gendered cultural bias has sufficiently distorted the market's wage-setting process to generate substantial, unexplained sex composition effects. The specialized human capital hypothesis claims that it does not and that all previously unexplained sex composition effects can be explained after taking proper account of occupational differences in specialized human capital. This hypothesis is a plausible application of the human capital theory but, unfortunately, has never been properly tested in the literature.

<sup>11</sup> To simplify the exposition, I will often speak of occupational differences in specialized human capital without specific reference to the investment cost (and its correlates) of specialized human capital.

<sup>12</sup> Few researchers would doubt that specialized training may account for part of the sex composition effects (Blossfeld 1987). In fact, the sex difference in the amount of on-the-job training, which is a form of specialized training, has long been recognized as one of the factors contributing to the gender wage gap (e.g., Duncan and Hoffman 1979; Gronau 1988; Barron, Black, and Loewenstein 1993).

### III. EMPIRICAL BACKGROUND

The sociological literature on occupational gender inequality has taken for granted the soundness of the evidence often cited in support of the devaluation hypothesis. To my knowledge, no study has seriously specified the limitations of the accumulated evidence. Were the accumulated evidence truly unproblematic, there is really little reason for the present study. However, I will argue that the existing evidence provides an unreliable basis to conclude that the devaluation hypothesis is well supported empirically. To avoid the distraction of technical details, this section will only sketch the argument. Available upon request is a research note (Tam 1996) that provides a formal and detailed critique of the limitations and deficiencies of the previous evidence for the devaluation hypothesis.

#### Limitations of Previous Research

As emphasized in the definition of the devaluation hypothesis, the empirical support for the hypothesis crucially hinges on the existence of substantial and unexplained negative sex composition effects against female occupations. This evidential requirement is biased in favor of the devaluation hypothesis because of the potential for omitted-variable bias. This is a real possibility because it is simply impossible for any researcher to explicitly control for everything potentially relevant to the sex composition effects.

To partially circumvent this difficulty, research over the last decade has focused on the use of longitudinal data. With panel data of individuals, many studies have used fixed-effects models to control for all *stable* unmeasured *worker* differences (e.g., unobserved ability) that may be responsible for the apparent wage penalties against female occupations (e.g., Gerhart and El Cheikh 1991; Macpherson and Hirsch 1995).<sup>12</sup> Other studies have sought to control for all *stable* unobserved *occupational* attributes by relating the changing sex composition of occupations with the changing wages of occupations (Pfeffer and Davis-Blake 1987; Baron and Newman 1989, 1990). Without denying the potential contributions of these longitudinal designs, it is important to bear in mind their limitations and deficiencies.

First, longitudinal studies do not take account of *unstable* unobserved factors. Under ideal conditions, longitudinal designs can deal with *stable* unobserved worker or occupational attributes. Yet not all theoretically

<sup>12</sup> Fixed-effects models in this context are essentially linear regression models that include a dummy variable for every person in the data. In effect, all parameter estimates are based on changes: i.e., the covariance between changes in the levels of the independent and dependent variables (see, e.g., Greene 1993).

relevant and unobserved factors are stable across occupations or over time. For instance, if productivity depends on a person-job match, then occupational changes could mean better or worse matches for a worker whose wage will be enhanced or adversely affected. Similarly, if the skill requirements of an occupation change over time, changes in sex composition will be confounded with changes in requirements.

Second, panel models (such as fixed-effects models) are typically much *more unreliable* than their cross-sectional counterparts. The basic reason is that panel models rely on changes in the dependent and independent variables to estimate the effects of the independent variables. The changes of an attribute of a worker may be infrequent or small in size relative to the variance of the attribute across workers. The lack of variance in change often imposes a limitation on the precision with which the parameters of interest may be estimated.

### Potential Flaws

It is well known that measurement errors produce biased parameter estimates and biased significance tests (Greene 1993). In the imperfect world of empirical analysis, measurement error is often an inescapable reality. In the study of occupational sex composition effects, we are confronted with the unflattering situation that most of the skill variables commonly used in the literature are plagued with substantial measurement errors. Neither the point estimates of sex composition effects nor the reported standard errors take any account of measurement errors in the skill variables or any independent variable. The wage effects of occupational sex composition may therefore appear to be substantial and significant even if they are small and insignificant after taking into account the consequences of measurement error. Most important, two common conditions often aggravate the negative consequences of measurement error: (1) the fixed-effects approach to estimating sex composition effects and (2) the use of noisy correlated measures of occupational attributes. Unfortunately, the serious consequences of the two conditions are not well recognized in the sociological literature.

First, the results of a fixed-effects model may be more biased than those of a cross-sectional model. Bias may arise from one of two situations: (a) one or more of the skill variables are subject to substantial measurement errors, or (b) occupational changes are not purely exogenous. The first situation is about the vulnerability of fixed-effects models to measurement error and it is well-known in econometrics and labor economics (Griliches 1979; Mellow and Sider 1983; Freeman 1984). The basic problem is that the loss of variance due to the use of a panel design will aggravate measurement-error bias. In the presence of substantial measurement



errors, then, fixed-effects models may be counterproductive if they are subject to serious biases that yield misleading results.

The second situation is the focus of Gibbons and Katz's (1992) analysis of interindustry wage differentials. There are good theoretical reasons to believe that many job changes are endogenous in the sense that job changes are motivated and guided by wage changes anticipated by the workers. If a researcher estimates a wage model based on wage changes associated with job changes, the estimates would be fundamentally affected by the dynamics of endogenous switches. In the presence of selective matching and job change,<sup>14</sup> we cannot be confident of the efficacy of fixed-effects models.

Second, multicollinearity among occupational attributes may aggravate and complicate measurement-error bias, especially in the context of a fixed-effects model. The basic source of the problem is that multicollinearity entails the lack of independent variance. When two explanatory variables are highly correlated, there is little independent variance available to separate the effects of each on the dependent variable. Griliches (1986) shows that multicollinearity between two or more explanatory variables (a) can greatly aggravate the size of measurement-error biases on individual parameter estimates and (b) transmit biases across all directly and indirectly correlated variables. Thus the error of one variable can have negative repercussions throughout a model.

This problem is particularly salient in the context of occupational gender inequality. Researchers have routinely included in their models many similar and correlated measures of skills and occupational attributes. The intention is noble: to explain as much as possible the sex composition effects and, simultaneously, to avoid potential omitted-variable bias that may inadvertently favor the devaluation hypothesis. However, this conventional practice of adding many control variables is harmless *only if measurement error is absent from all control variables and their correlates in the model*. This error-free condition often fails in the study of sex composition effects. Further, a basic motivation for the inclusion of a control variable is that it is correlated with some of the variables of interest. If omitted, it may introduce an omitted-variable bias. Ironically, this requirement also means that if the control variable is subject to substantial

<sup>14</sup> Some recent evidence suggests that both employers and employees are selective and that their preferences jointly affect the sex composition across occupations (see, e.g., Padavic 1992; Gupta 1993). Furthermore, if the quality of a job-person match is not guaranteed to be optimal, there is the need to experiment with different job-person matches. As a result, many job changes may be due to mismatch and the search for better matches and are therefore endogenous. Indeed, Jovanovic and Moffitt's (1990) empirical findings suggest that most of the job changes among young males in the United States are due to mismatch.

measurement error, it can have dire consequences: creating rather than reducing biases in the parameters of interest. This problem can be devastating even in a cross-sectional model (e.g., Filer 1989). When coupled with the loss of independent variance in a fixed-effects model, the inclusion of the noisy control may be especially counterproductive. In short, the inclusion of multiple noisy controls is likely to be self-defeating.

#### A Cautious Interpretation

Virtually all of the quantitative evidence for the devaluation hypothesis is centered on one finding: the presence of unexplained sex composition effects against female occupations. Unfortunately, the sociological literature has been uncritical toward the fixed-effects approach and the use of extensive controls. Consequently, past research has failed to guard against the serious problems of measurement error bias afflicting models with correlated controls or fixed effects. The problems may well lead to an inflation of the negative sex composition effects and thus unfairly favor the devaluation hypothesis and introduce bias against the specialized human capital hypothesis. In light of these measurement error problems, it is not at all surprising that past research has found a robust presence of unexplained sex composition effects. The accumulated evidence thus fails to provide any critical support for the devaluation hypothesis or to offer any reliable refutation of the specialized human capital hypothesis. The confidence in the empirical support for the devaluation hypothesis is premature.

#### IV. DATA AND METHOD

This study uses the May 1988 Current Population Survey (CPS) as the main source of data, the natural logarithm of hourly wages as the dependent variable, and a number of demographic and conventional skill variables as controls. This particular CPS is selected because it covers a time when the unexplained sex composition effects are among the highest for both sexes (see table 1 of Macpherson and Hirsch [1995]), it is reasonably current and yet close to the periods covered by other studies, and it contains data on worker tenure with current employer.<sup>15</sup> The survey provides

<sup>15</sup> For the purposes of this article, tenure is an important variable to include in the analysis for two reasons. First, tenure captures much of the gender difference in actual work experience that is not captured by potential work experience. (Note that the CPS does not provide information on the actual years of work experience, only potential work experience can be derived.) Elsewhere (Tam 1994), I have demonstrated that the bias on the gender wage gap due to the use of potential work experience virtually disappears once tenure is taken into account. Second, and most important,

TABLE 1  
DESCRIPTION OF VARIABLES

Variable	Description	Mean	SD
Wage .....	Hourly wage is the ratio of weekly earnings and usual weekly hours.	9.679	5.497
SMSA .....	1 = residing in a standard metropolitan statistical area, 0 = otherwise.	.725	.446
South .....	1 = residing in the South, 0 = otherwise.	.303	.459
West .....	1 = residing in the West, 0 = otherwise.	.181	.385
Midwest .....	1 = residing in the Midwest, 0 = otherwise.	.272	.445
Veteran .....	1 = ever in the armed forces, 0 = otherwise.	.160	.366
Union .....	1 = union member or under union contract, 0 = otherwise.	.187	.390
School .....	Years of schooling completed	14.028	2.632
Experience .....	Potential work experience, estimated by age - school - 5.*	17.702	12.177
Tenure .....	Years with current employer	6.779	7.721
Full-time .....	1 = usually work at least 35 hours a week, 0 = otherwise.	.829	.376
SC .....	Women as a proportion of workers in respondent's occupation	.465	.324
SVP .....	Years of specific vocational training for an occupation†	1.977	1.611

NOTE — The data are for whites in the expanded May 1983 CPS (see text). Most of the valid *N*s are 23,019 or slightly less. Valid *N*s for experience and tenure are 19,666. Not listed are 22 industries: construction, education, financial/insurance/real estate, wholesale, retail trade, medical (except hospital), hospital, agriculture, mining, manufacturing of durable goods, manufacturing of nondurable goods, postal, other transportation, other utilities, private household service, business/repair, personal services (except private household), entertainment/recreation, welfare/religious, other professional services, forestry/fisheries and public administration/armed forces.

\* For consistency, negative values of experience are recoded "0."

† From England and Kilbourne's (1985) match of SVP (*DOT*, 4th ed.) with three-digit 1980 census occupations.

about 20,000 whites for the main analysis and about 1,900 blacks for cross-validation.

Appended to the CPS are two constructed variables: the sex composition of the three-digit census occupations, and a metric measure of occupation-specific training derived from the fourth edition of the *Dictionary of Occupational Titles* (hereafter *DOT*; see U.S. Department of Labor 1977; see also Cain and Treiman 1981). Together with a set of dummy variables for major industries, these two constructed variables are the focal set of independent variables for the empirical analysis of this article. Table 1

the gender difference in tenure is substantial (table 1). Since occupational sex composition is highly correlated with gender, the two considerations make it necessary for any analysis of the wage effects of sex composition to control for tenure.

describes the key variables used in the analysis and provides the basic descriptive statistics for the main sample. The appendix presents the pairwise correlations among selected variables.

*Sex composition (SC).*—The wage effects of occupational sex composition are the key parameters of interest in the analysis. Since the bulk of empirical studies testifying to the persistence of sex composition effects is based on the three-digit census occupational classification, this article will do the same. The NBER Annual Merged CPS File for 1988 (NBER 1992) provides a convenient basis (about 200,000 cases) to estimate the sex composition of each 1980 three-digit census occupation. For each occupation, the variable is measured as the proportion of workers who are women.

*Specialized training.*—One of the basic ideas of human capital theory is that the investment cost of a skill has to be compensated in the labor market. To the extent that this is true, differences in the investment costs of specialized skills may account for the sex composition effects. Thus central to the testing of the specialized human capital hypothesis is the measurement of investment cost in specialized human capital.

It would be ideal if we could directly measure the investment cost of specialized skills. In practice, investment cost is difficult to gauge. However, research on the returns to schooling has demonstrated that the opportunity cost of schooling proves to be a good proxy for the investment cost of the skills acquired in school (e.g., Becker 1975). Years of schooling is a measure of the opportunity cost of a person's schooling, which in turn is a simple and effective measure of the investment cost of schooling. Despite the fact that the factors determining the supply and demand for schooling are complex, years of schooling indeed is a powerful and robust predictor of wages across societies and over time, indicating that investment cost is a general and robust determinant of wages. This remarkable fact about schooling suggests that a similar pattern may hold for specialized skills. It would therefore be very useful if we can obtain data on the actual training time a worker spent in acquiring occupation- and industry-specific skills.

*SVP.*—Fortunately, occupational analysts have codified the typical occupation-specific training time required for each occupation in a very detailed classification. The result is a measure called *specific vocational preparation*, commonly abbreviated as SVP. The variable has been incorporated into the DOT (U.S. Department of Labor 1977). By definition, "SVP is the amount of time required to learn the techniques, acquire the information, and develop the facility needed for average performance in a specific job-worker situation. SVP includes training acquired in a school, work, military, institutional, or vocational environment, but excludes schooling without specific vocational content" (England and Kilbourne

1988, app. B, p. 9).<sup>16</sup> This measure was originally coded at the level of nine-digit *DOT* occupations, which is much more detailed than any census occupational classification. Since most labor surveys do not record occupations at the *DOT* level, researchers often cannot make use of the SVP measure without aggregating to the level of the three-digit census occupations. England and Kilbourne (1988) have greatly facilitated this task by providing the average SVP scores, among other *DOT* measures of occupational characteristics, for the three-digit occupational categories of the 1980 census.

The original scale of SVP has nine ordered levels, each of which corresponds to an interval of training time.<sup>17</sup> England and Kilbourne's (1988) data include an alternatively coded measure of SVP based on the midpoint of each time interval, except that the lowest level was coded as zero and the highest level was coded as 10 years. Contrary to some previous studies, the measure of specific vocational preparation used here is coded as the amount of time (years). Reviewing the SVP for a sample of occupations may be instructive. For financial managers, the proportion of women in that specific occupation (SC) in 1988 is about .4 and the SVP is 5.5 years. For public relations specialists, SC = .6 and SVP = 3.4 years. For computer operators, SC = .7 and SVP = 1.5 years. For child-care workers, practically an all-female occupation, SVP is .4 years (.2 if working for private households). For physicians, SC = .2 and SVP = 6.7 years. For mechanical engineers, a practically all-male occupation, SVP = 6.1 years. For construction laborers, also male dominated, SVP = .4 years. Because of the tremendous cost of compiling the *DOT* scores, the *DOT* measures cannot be frequently updated. It is important to recognize this temporal limitation of all *DOT* measures, especially in the context of studying occupational change.

*Industry.*—Until Johnson and Solon's (1986) notable study, it was not well recognized that industrial mix is associated with the wage effects of occupational sex composition. By contrast, it is well established that industry has autonomous wage effects that cannot be reduced to stable worker attributes, occupational mix, and other common economic variables. In fact, no existing theory can explain all the known findings about

<sup>16</sup> Despite the potential for overlap, there is no evidence of much statistical redundancy between years of schooling and SVP.

<sup>17</sup> Specifically, 1 = short demonstration only; 2 = anything beyond short demonstration, up to and including 30 days; 3 = over 30 days, up to and including three months; 4 = over three months, up to and including six months; 5 = over six months, up to and including one year; 6 = over one year, up to and including two years; 7 = over two years, up to and including four years; 8 = over four years, up to and including 10 years; 9 = over 10 years.

interindustry wage differentials.<sup>18</sup> However, a new model that stresses industry-specific training could indeed reconcile with the previous findings in the literature on interindustry wage differentials (Katz, personal communication, May 23, 1995). To the extent that (a) occupations of varying sex composition are associated with a different mix of industries and that (b) industry has autonomous wage effects, occupational sex composition effects may well reflect the wage consequences of a different mix of industry-specific human capital. Unfortunately, I do not have a measure of industry-specific training time. The best I can do is use dummy variables of industry to capture the potential effects of industry-specific training time.<sup>19</sup> As a starting point, I consider the two-digit classification of 22 industries defined by the 1988 CPS.<sup>20</sup>

*Methods.*—The empirical analysis of this article is straightforward. Following the convention in the literature, the analysis will be done separately by sex. The main analysis is based on whites because there are about 20,000 whites but only about 2,000 nonwhites in the CPS. The dependent variable,  $y$ , is the logarithm of hourly wages whereas the parameter in focus is the coefficient of the fraction of workers who are women in an occupation (SC). Ordinary least squares (OLS) regression is applied to estimate the sex composition effects after controlling for common worker and demographic variables ( $X$ ), and after adding the control for industry and occupation-specific training, separately and jointly. Formally, four kinds of models will be estimated:

<sup>18</sup> There is a large and high-quality empirical literature on the puzzle of interindustry wage differentials. See, e.g., Dickens and Katz (1987), Krueger and Summers (1987, 1988), Katz and Summers (1989), and Gibbons and Katz (1992). The literature has produced many findings, yet no theoretical model to date fits all the known facts without resorting to ad hoc assumptions. As Gibbons and Katz (1992, p. 530) put it: "Unmeasured-ability models do not motivate findings of strong pairwise correlations between industries that pay high average wages and industries that earn large profits, have high capital-to-labor ratios, and are populated by large firms. . . . Efficiency-wage models do not motivate the observed high correlation of the industry wage premium across occupations. And rent-sharing models do not motivate the observed similarity of the industry wage structure across countries with very different market systems, such as Eastern and Western Europe." Neal's (1995) intriguing findings provide indirect support for the interpretation that the industry wage effects arise from industry-specific training time. See also Kletzer (1996) for related evidence.

<sup>19</sup> If industry mix proves to be an important source of occupational sex composition effects, an obvious topic for future study would be to test specifically whether the industry effects are due to industry-specific training time.

<sup>20</sup> The 22 industries are: construction, education, financial/insurance/real estate, wholesale, retail trade, medical (except hospital), hospital, agriculture, mining, manufacturing of durable goods, manufacturing of nondurable goods, postal, other transportation, other utilities, private household service, business/repair, personal services (except private households), entertainment/recreation, welfare/religious, other professional services, forestry/fisheries, public administration, and armed forces.

$$y = f_1(X, SC), \quad (1)$$

$$y = f_2(X, SC, INDUSTRY), \quad (2)$$

$$y = f_3(X, SC, SVP), \quad (3)$$

$$y = f_4(X, SC, INDUSTRY, SVP). \quad (4)$$

Taken together, the analysis will allow us to evaluate the extent to which occupation- and industry-specific training contribute to the sex composition effects. Further analysis will be conducted to cross-validate the central findings.

There are many differences between this study and previous studies. Some differences are related to design. This study is cross-sectional whereas many of the previous studies are longitudinal. Other differences are related to the measurement of the key independent variables (such as SVP) or sample coverage. For instance, England et al. (1988) use selected cohorts of the labor force for their fixed-effects analysis rather than a nationally representative sample comparable to the CPS. Moreover, their estimation is based on the patterns of wage changes associated with workers' switching of occupations. Thus panel design usually restricts attention to a fraction of the entire universe of workers.

Between this and any single study in the past, there are always multiple differences. The most critical difference, however, is the fact that previous studies ignore the danger of including noisy and correlated measures of occupational attributes whereas this one does not. Measurement error is especially destructive when there is multicollinearity among noisy control variables. The issue is not whether it is desirable to include more measures of skills. Under ideal conditions, more controls would have been innocuous. With measurement error, however, including more control variables may create new and serious problems. The really critical issue here is whether the control for additional skills has been done properly. Previous studies have largely ignored the issue even under conditions where the problems of measurement error are likely to be severe. By contrast, the simple models of this article would largely avoid those problems.

## V. FINDINGS

To give a sense of the factors that may be responsible for the sex composition effects, table 2 presents the average characteristics of whites. Specifically, the means of variables are presented separately for two groups of workers: those in occupations in which over 75% of workers are women and those in occupations in which fewer than 25% are women. The two groups of workers certainly have notable differences in hourly wages, occupation-specific training and industrial mix, and substantial differ-

TABLE 2

## MEAN CHARACTERISTICS OF WORKERS IN MALE AND FEMALE OCCUPATIONS

Variable*	Male Occupations† (Mean)	Female Occupations† (Mean)
Wage .....	10.606	7.634
SMSA .....	.705	.733
South .....	.348	.329
West .....	.158	.179
Midwest .....	.264	.256
Veteran .....	.281	.030
Union .....	.261	.157
School .....	13.447	13.985
Experience .....	18.252	17.162
Tenure .....	7.627	5.588
Full-time .....	.923	.711
SC .....	.090	.898
SVP .....	2.200	1.128
Construction‡ .....	.159	.014
Manufacture-durable .....	.190	.033
Wholesale .....	.068	.022
Resale .....	.099	.173
Finance/insurance/real estate .....	.010	.093
Medical .....	.006	.101
Hospital .....	.009	.105
Education .....	.017	.148

NOTE: — *N* for workers in predominantly male occupations: 6,443 for experience and tenure; 7,598 for veteran; 7,597 for SVP, and 7,599 for all else. *N* for workers in predominantly female occupations: 5,948 for experience and tenure; 7,007 for veteran; 7,017 for SVP, and 7,019 for the rest. Data are for whites in the May 1988 CPS.

\* SC = proportion of women in the respondent's occupation; SVP = years of specific vocational preparation for an occupation. The other variable labels are conventional. See table 1 for details.

† For this table, male occupations = occupations with at most 25% female workers; female occupations = occupations with at least 75% female workers.

‡ Construction to education = dummy variables for eight of the 22 industries in the 1988 CPS, listed for illustrative purposes.

ences in tenure and full-time status. Workers in male occupations receive higher wages and higher occupation-specific training than do those in female occupations. Workers in male occupations are also much more likely than those in female occupations to be in the construction, manufacturing, and wholesales industries, but less likely to be in the resale, financial/insurance/real estate, medical, hospital, and education industries. These are, of course, only zero-order differences. The contributions of these zero-order differences to explaining the sex composition effects must be evaluated within a multivariate model. To help adjudicate between the devaluation and specialized human capital hypotheses, this section will examine four central findings.



## Basic Results

Table 3 presents the findings for whites in the sample. For simplicity, I have omitted from this and subsequent tables the coefficients of all the conventional variables and the intercept.<sup>21</sup> The unlisted variables are described in the footnote to table 2. The coefficients of three variables will be the focus:

1. SC refers to the fraction of workers in an occupation who are women. A negative coefficient indicates a wage disadvantage for occupations with a higher proportion of female workers.
2. SVP refers to specific vocational preparation, the number of years of occupation-specific training. A positive coefficient reflects a positive return to occupation-specific training time.
3. INDUSTRY refers to the 22-category industry classification of the 1988 CPS, incorporated as a set of dummy variables.

**RESULT 1.**—*The sex composition effect is virtually zero among white women, suggesting that there is no net sex composition effect once occupation-specific training is added to a set of basic control variables.*

The first major finding is focused on white female workers. The gross sex composition effect ( $-.255$ , not shown in the table) is substantially reduced ( $-.158$ ) after controlling for a set of standard variables. The residual effect is still negative and substantial, indicating a wage disadvantage against female occupations. The conventional evidence in support of the devaluation hypothesis is apparently confirmed.

After taking into account occupational differences in two measures of specialized human capital, however, column 4 of table 3 shows that there is virtually no occupational sex composition effect left to be explained among white women. The key explanatory variable of the sex composition effect is SVP. This fact is obtained by a comparison of the coefficients of SC across columns 1–4 of table 3. Without controlling for SVP, the estimated sex composition effects are  $-.158$  and  $-.187$  without and with control for industry-specific effects, respectively. This indicates that industry-specific effects cannot explain much of the sex composition effects among white women. After controlling for SVP, however, the sex composition effects are dramatically reduced to  $.011$  and  $-.007$  without and with control for industry effects, respectively. These sex composition effects are not just statistically insignificant, they are virtually zero among women, suggesting that there is no net sex composition effect once occupation-specific training is taken into account. This finding therefore contradicts

<sup>21</sup> Tables with the full set of coefficients are available upon request.

TABLE 3  
REGRESSIONS OF LOG HOURLY WAGES ON DEMOGRAPHIC, HUMAN CAPITAL, AND INDUSTRY VARIABLES

	WHITE WOMEN				WHITE MEN			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SC	-.158 (.016)	-.187 (.016)	.011* (.017)	-.007* (.018)	-.224 (.018)	-.094 (.019)	-.141 (.018)	.002* (.019)
SVP			.088 (.004)	.078 (.004)			.065 (.003)	.069 (.003)
INDUSTRY		Yes (56.792)		Yes (50.145)		Yes (51.883)		Yes (54.429)
Adjusted R <sup>2</sup>	.403	.470	.440	.496	.452	.503	.482	.532

NOTE: Definition of variable: SC = proportion of women in the respondent's occupation; SVP = years of specific vocational preparation for an occupation; INDUSTRY denotes the control for 21 industries. Other variables included are South, West, Midwest, SMSA, veteran, union, school, experience and its square, tenure and its square, and full-time. Not in parentheses are SEs, except that *F*-statistics are reported for industry effects. *N* = 9,269 for (1) and (2); 9,265 for (3) and (4); 10,383 for (5) and (6); 10,372 for (7) and (8). Data are for whites in the May 1968 CPS.

\*Not significant at the .05 level; other presented coefficients are significant at the .05 level

the devaluation hypothesis by any reasonable level of statistical significance.

### Gender Specificity

One of the central features of the devaluation hypothesis is that the devaluation of women's work is general, implying that it adversely affects *both* men and women working in female-dominated occupations. Result 1 has indicated that women do not really face any sex composition effect. It is therefore important to see if men do. The devaluation hypothesis also maintains that both men and women are affected for the *same* theoretical reason: Whether a worker is male or female, so long as the work is typically done by women, the line of work is devalued and the worker will be paid less. It is therefore useful to investigate whether the evidence supports the view that the sex composition effects among men and women follow from a single process.

RESULT 2.—*The sex composition effect is absent among white men but the sources of the apparent sex composition effects differ substantially across gender groups.*

Columns 5–8 of table 3 address this question by identifying the sources of the sex composition effect among white men. It is interesting that the gross sex composition effect among white men ( $-.134$ , not shown in table 3) appears to be substantially smaller than that among white women ( $-.255$ ). The effect becomes much larger ( $-.224$ ) once a standard set of controls are included in the model of column 5. In this model, the residual sex composition effect is substantially higher than that among white women ( $-.158$ ).

However, after controlling for specialized human capital (SVP and industry), the unexplained sex composition effect among white men becomes practically zero ( $.002$ ) as it does among white women ( $-.007$ ). Equally significant, adding occupation-specific training to a standard wage model does not fully account for the sex composition effect among white men. The unexplained gap in column 7 remains very substantial ( $-.141$ ). If the model controls for industry-specific effects but not occupation-specific training, column 6 shows that the gap is also substantial ( $-.094$ ). When both industry-specific effects and occupation-specific training are incorporated, by contrast, the unexplained sex composition effect is statistically insignificant despite a large sample size and a small standard error for the coefficient. In fact, the sex composition effect becomes virtually zero ( $.002$ ). The evidence thus confirms the hypothesis that there is no net sex composition effect if both occupation-specific and industry-specific human capital are taken into account.

Most notably, the industry dummies are crucially important for ex-

plaining the apparent sex composition effect among white men. Industry effects simply do not contribute to explaining the apparent sex composition effect among white women. These results therefore suggest that the sex composition effects are unlikely to be driven by a general process such as the cultural devaluation of women's work. Nonetheless, as predicted by the specialized human capital hypothesis, all the previously unexplained sex composition effects among men and women are similarly attributable to occupational differences in specialized human capital. Even though the size of the previously unexplained sex composition effects differ for men and women, each can be fully accounted by a gender-specific combination of specialized human capital. This pattern is at odds with the devaluation hypothesis.

Finally, it is worth emphasizing that industry wage effects are found among *both* men and women. What is gender specific is the fact that, among men but not among women, the sex composition effect is substantially due to an association between sex composition and industry-specific training. The finding may reflect a gender-specific process of occupational allocation.<sup>22</sup> Unfortunately, it is beyond the scope and data of this article to seriously examine this question.

<sup>22</sup> At this point, we can only speculate over the possible reason for this specificity. To illustrate, consider a labor market where women can freely choose industry according to personal preference but their occupational choices are highly constrained. Specifically, occupational choices among women in this hypothetical world are dependent on employers' gender preferences for different occupations. Assume further that employers' gender preferences for an occupation are mainly dependent on the level of occupation-specific training for the occupation (presumably because of concerns about turnover cost). We would therefore expect that the level of occupational training is correlated with sex composition but not with industry. Now suppose that a different process holds among men: men are free to make *both* occupational and industry choices. For simplicity, assume that the choices mainly depend on the level of specialized training involved. Because of ability differences and financial constraints, some men choose careers with much specialized training opportunity, some prefer those with little specialized training. Across men, there would be a positive correlation between the level of occupation-specific and industry-specific training requirement. A correlate of occupation-specific training would also be a correlate of industry-specific training. If sex composition is correlated with occupation-specific training, it would be correlated with industry-specific training, too. Hence occupational sex composition would capture the wage effects of both occupation-specific and industry-specific training among men if a model does not properly control for specialized training. In this world, the cultural devaluation of female labor is not at work. The sex composition effects among women and men are all attributed to occupational differences in specialized training, though the social processes giving rise to this correlation differ for women and men. This is one of the plausible stories that future research should investigate.

## Validation and Robustness

The devaluation hypothesis maintains that cultural devaluation throughout the labor force is responsible for the apparent sex composition effects, that is, the apparent wage penalties against female occupations. Under this hypothesis, one would expect the wage penalties against female occupations to be generally observable, even if not constant, within different segments of the labor force.<sup>23</sup> Specifically, the devaluation hypothesis predicts *substantially negative* sex composition effects for the labor force as a whole and *consistently negative* effects within different segments. By contrast, the specialized human capital hypothesis predicts the *absence* of sex composition effects in the labor force as a whole. If there appears to be a negative (or positive) sex composition effect in a segment, then by implication there must be an *offsetting* effect in another segment.<sup>24</sup>

Although results 1 and 2 are strongly consistent with the specialized human capital hypothesis, it is natural for advocates of the devaluation hypothesis to question whether the results are incidental aberrations. This section offers two new analyses to explore this possibility. The first analysis addresses this possibility with a replication of the previous two analyses using blacks in the sample. The second analysis examines the sex composition effects within different segments of the labor force.

The analysis of blacks is particularly interesting for two reasons. First, one would not necessarily expect the results for blacks to be similar to those for whites. Second, and most important, the two competing hypotheses have different predictions for the results among blacks. If the specialized human capital hypothesis is strictly correct, we would expect the sex composition effects to be absent in the specialized human capital models for whites and blacks. The results among blacks should be similar, except for sampling variability, to those among whites. Since the black sample is much smaller than the white sample, one would not necessarily find

<sup>23</sup> Indeed, a recent variant of the devaluation hypothesis stresses that devaluation is contingent upon local contexts (Baron and Newman 1990; Jacobs 1995). This *localized devaluation hypothesis* allows for variable levels of devaluation to exist in different local contexts, depending on factors such as job age, job idiosyncrasy, and local labor market conditions. Nevertheless, even the localized devaluation hypothesis does not give up the original and core thesis of the devaluation hypothesis: female occupations consistently face wage penalties because they are regarded as female work and on average the penalties are substantial.

<sup>24</sup> Compared to a representative sample of the labor force, segment-specific sex composition effects, if any, are more susceptible to omitted variable bias, depending on the definition of a segment. It is possible that sex composition effect may be spuriously negative or positive in a segment. The specialized human capital hypothesis implies that such an effect would be offset by an opposite effect in another segment.

the analysis of blacks to closely reproduce the findings of the analysis of whites even if the specialized human capital hypothesis is valid.<sup>25</sup> By contrast, if the devaluation hypothesis is valid, we would expect the sex composition effects to remain substantial even in the specialized human capital models for whites and blacks. If the results are inconsistent with the theoretical prediction of the devaluation hypothesis for any race group, the results may well be due to chance. We would not expect the results to hold up consistently in both race groups. The contrary predictions of the competing hypotheses therefore provide yet another test for the hypotheses.

*RESULT 3.—The roles of occupation-specific training and industry-specific effects in explaining the sex composition effects among blacks are the same as those among whites.*

Table 4 presents the results of the replication for blacks. Since the sample size of blacks is only one-tenth of that of whites and is quite small in absolute terms, the standard errors for the estimated sex composition effects are large. Still, insofar as the point estimates are concerned, the patterns of residual sex composition effects are strikingly similar to those of table 3 for whites. Column 3 shows that, among black women, the net sex composition effect ( $-.043$ ) is insignificant if occupation-specific training is taken into account. When industry-specific effects are also included, the effect ( $.051$ ) remains insignificant and, if anything, it is positive. Similarly, column 8 suggests that, for black men, the net sex composition effect ( $.014$ ) is trivially small and insignificant once specialized human capital effects are incorporated. When only occupation-specific training is included, the net sex composition effect is substantially negative although not statistically significant. The results are surprisingly close to those among whites. It is surprising because the black sample is much smaller than the white sample and thus sampling variability would have easily worked against a close reproduction of results across the two groups even if the specialized human capital hypothesis is valid.<sup>26</sup>

Overall, occupation-specific training remains crucial for explaining the sex composition effect among black women. Both occupation-specific training and industry-specific effects are important sources of the sex composition effect among black men. Like the patterns for whites, occupation-specific training is most crucial for women while industry-specific effects

<sup>25</sup> Ideally, a replication study would draw upon a CPS that is close to 1988 and contains the same set of variables. However, such a CPS is not available because the tenure variable is not a regular CPS item.

<sup>26</sup> Sampling variability is much larger among blacks than among whites. This is confirmed, e.g., by the SEs of the coefficients of sex composition. The SEs of SC in table 4 are generally three times the corresponding ones in table 3.

TABLE 4  
REGRESSIONS OF LOG HOURLY WAGES ON DEMOGRAPHIC, HUMAN CAPITAL, AND INDUSTRY VARIABLES

	BLACK WOMEN				BLACK MEN			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
SC .....	-.186 (.050)	-.130 (.053)	-.043* (.052)	.051* (.056)	-.151 (.055)	-.042* (.061)	-.101* (.054)	.014* (.059)
SVP .....			.091 (.012)	.094 (.012)			.071 (.009)	.079 (.010)
INDUSTRY .....		Yes (6.134)		Yes (6.347)		Yes (3.182)		Yes (4.428)
Adjusted R <sup>2</sup> .....	.414	.467	.446	.491	.445	.477	.479	.516

NOTE: For definition of variables, see table 3. Nos. in parentheses are SEs, except that *F*-statistics are reported for industry effects. *N* = 1,042 for cols. 1-4, 855 for 5-8. Data are for blacks in the May 1988 CPS.

\* *N*/*s* significant at the .05 level, other presented coefficients are significant at the .05 level

TABLE 5

REGRESSIONS OF LOG HOURLY WAGES ON DEMOGRAPHIC, HUMAN CAPITAL, AND INDUSTRY VARIABLES: RACE BY EMPLOYMENT STATUS AMONG FEMALE WORKERS

	WHITE WOMEN		BLACK WOMEN	
	Full-Time	Part-Time	Full-Time	Part-Time
SC .....	-.044** (.020)	.084** (.040)	.055* (.058)	.137* (.175)
SVP .....	.068*** (.004)	.109*** (.008)	.087*** (.012)	.136*** (.040)
INDUSTRY .....	Yes (38 209)	Yes (14.866)	Yes (6.380)	Yes (2.887)
Adjusted R <sup>2</sup> .....	.453	.440	.486	.328
N of cases .....	6,793	2,472	852	190

NOTE.—For definition of variables, see table 3. Nos in parentheses are SEs, except that *F*-statistics are reported for industry effects.

\* Not significant at the .05 level.

\*\* Significant at the .05 level.

\*\*\* Significant at the .01 level.

are most crucial for men. These results for blacks are strikingly similar to those for whites, demonstrating the remarkable robustness of the first two central findings. The analysis of blacks thus corroborates the specialized human capital hypothesis and at the same time offers yet another refutation of the devaluation hypothesis.

The next result calls attention to the fact that, even within a gender group, female-dominated occupations appear to offer a wage advantage for some segments of the labor market. The result is based on splitting each gender  $\times$  race sample into two groups: those working full-time versus those working part-time.

RESULT 4.—*Sex composition effects are consistently absent within labor force segments and, when there is a deviation, female occupations appear to offer a substantial wage advantage rather than penalty.*

Consider first the samples of women. In a model that excludes specialized human capital, there are indeed wage penalties against female occupations among white women working full-time (–.191) or part-time (–.039, not significant). While this pattern seems to confirm the devaluation hypothesis, it turns out to be spurious. Once SVP and industry are taken into account, table 5 shows that the sex composition effect (.084) is *substantially and significantly positive* among white women working part-time—a minor segment of the white women sample. This positive effect is offset by an insignificantly negative effect (–.044) among those working full-time—a major segment of the white women sample. The



pattern among black women tells a similar story. Unfortunately, there are too few black women in the data to yield reliable estimates. If one takes the segment-specific results seriously, then, female work does not always lead to a wage disadvantage for all segments of the labor force. In fact, restricting attention to women working part-time, female occupations appear to offer a wage advantage over male occupations. This is at odds with the devaluation hypothesis.

The results for white and black men contain no surprises and are similar to those of tables 3 and 4.<sup>27</sup> All of the sex composition effects are statistically insignificant for those working full-time or part-time. Wage penalties against female occupations are consistently absent. Whether the sex composition effects are positive or insignificantly different from zero, these results for women and men are inconsistent with the devaluation hypothesis. After taking proper account of specialized human capital, it is really difficult to find any evidence for wage penalties against female occupations.

### Potential Bias?

Unlike some of the studies most often cited for the devaluation hypothesis (e.g., England et al. 1988), this study is entirely based on simple models and cross-sectional data. One may argue that the analysis is susceptible to omitted-variable bias from unmeasured worker ability, skills, and other stable attributes that may affect wages. Like any other social science data analysis, omitted variables are always a source of potential problem. The really relevant question is whether the direction of bias is likely toward or against the conclusion reached by a study. If the direction of bias is toward the conclusion, the bias may have unfairly favored the conclusion. Then the key question is whether the bias is substantial enough to render the conclusion misleading.<sup>28</sup>

Fortunately, with respect to the absence of control for all stable unobserved worker attributes, we can address the question of bias with the help of recent research. The most meticulous and comprehensive fixed-effects analysis to date (Macpherson and Hirsch 1995) has shown that there is positive sorting based on unmeasured worker attributes. That is, workers with more valuable labor qualities are more likely to be found in male occupations than in female occupations. If anything, then, incorporating the control for stable unmeasured worker attributes will only

<sup>27</sup> The tables are available upon request.

<sup>28</sup> If the direction of bias is against the conclusion, the bias will not have unfairly favored the conclusion. In fact, if the evidence favors the conclusion despite the potential bias against the conclusion, then the conclusion is a conservative one.

leave weaker negative sex composition effects to be explained. Without allowing for the influence of stable unmeasured worker attributes, the direction of bias favors the devaluation hypothesis rather than the specialized human capital hypothesis. We may therefore conclude with confidence that the simple methodology of this study does not misleadingly produce favorable evidence for the specialized human capital hypothesis or unfavorable evidence against the devaluation hypothesis.

## VI. REEVALUATING THE DEVALUATION HYPOTHESIS

What have we learnt from the central findings? The first two results have strongly suggested that the sex composition effects are all mediated by specialized human capital differences and that the sources of the wage effects of sex composition are gender specific. Both findings contradict the devaluation hypothesis. The third result demonstrates that both findings are robust across racial groups. There is no real wage penalty against female occupations: not among women or men, not among whites or blacks. The fourth result reinforces these findings. The result indicates that, for a particular segment of the labor force, there appears to be a substantial and significant departure from the general pattern. Yet the departure does not fit well with the devaluation hypothesis because it shows a large wage advantage, instead of a disadvantage, for female occupations among women working part-time. To be sure, all the central results are not random deviations from the devaluation hypothesis. The deviations are either predicted by or perfectly compatible with the specialized human capital hypothesis.

In spite of the strong evidence against the devaluation hypothesis, four arguments in defense of the devaluation hypothesis have to be addressed. In part, the arguments are based on two theoretical variants of the devaluation hypothesis: the invisibility thesis and the biased return thesis. Supporting arguments also include statistical challenges for the four central results: aggregation bias and omitted variable bias.

### The Invisibility Thesis

The first natural defense for the devaluation hypothesis is what may be called the "invisibility thesis." While the original devaluation thesis stresses the negative wage consequences of the "femaleness" of an occupation, the invisibility thesis emphasizes not so much the "femaleness" of an occupation but the invisibility of the female skills in the occupation. Female skills are less visible than male skills. Thus female occupations are mistakenly rated as requiring less skill than male occupations.

Steinberg (1990) has argued that, in many traditional job evaluation

systems, the complexity and contributions of female skills are overlooked or grossly understated. Irrespective of the general validity of Steinberg's diagnosis of job evaluation systems, a gendered bias in the construction of SVP, for instance, may have created the impression that female occupations are lower in SVP. The measures of occupational complexity in the third edition of *DOT* were indeed subject to similar criticisms and, subsequently, much revised to avoid blatant gender biases. Cain and Treiman (1981) have critically compared the third and fourth editions of *DOT* and noted the improvements in the measures of job complexity. Nonetheless, it is infeasible for us to rigorously determine whether SVP is subject to any systematic gender bias.

Once the proposition of gendered bias is formalized, however, it can be shown that the hypothesis has to make some very implausible assumptions in order to be an explanation of the central findings of this study. To fix ideas, let  $T$  be the true time required for occupation-specific training (which may be correlated with  $SC$ ). Rating bias may be expressed by the following equation:

$$SVP = T + \alpha SC,$$

where  $\alpha$  is a parameter that sums up the way a rater distorts the SVP score purely according to the sex composition of an occupation (which proxies, e.g., for the extent of female skills required). Let the true wage model with occupation-specific training and sex composition effects be as follows:

$$y = X\beta + \eta T + \delta SC + e,$$

which implies

$$y = X\beta + \eta(SVP - \alpha SC) + \delta SC + e,$$

and

$$y = X\beta + \eta SVP + (\delta - \eta\alpha) SC + e.$$

The main results of this article suggest that the coefficient of  $SC$  is practically zero when SVP is used. This fact entails that if the true coefficient of  $SC$  is negative ( $\delta < 0$ ) rather than zero, then the rater's bias effect ( $\eta\alpha$ ) must have just offset the true sex composition effect ( $\delta$ ).

When  $\alpha$  is negative, female occupations will certainly appear to have lower occupation-specific training. Yet the proposition of gender bias more specifically claims that this may have been the reason why occupation-specific training seems to account for the sex composition effects. Indeed, Steinberg (1990, p. 462) explicitly postulates that the pattern of gender bias is designed to legitimate and reproduce an unfair wage structure. However, for this proposition to explain the absence of sex composition

effects here, it must assume that the raters of occupation-specific training acted as if they (a) know the true and irreducible sex composition wage effect ( $\delta$ ), (b) the true effect of training time ( $\eta$ ), and then (c) introduce gender bias ( $\alpha$ ) in such a way as to perfectly offset the true sex composition wage effect. There is simply hardly any ground to make these assumptions. Without these stringent assumptions, the proposition cannot explain why the previously unexplained sex composition effects completely vanish in the specialized human capital models of this study.

The invisibility thesis or the rater bias conjecture is even more implausible if we consider result 2 above. The finding shows that the apparent wage penalties against female occupations have different sources for men and women. Most important, occupation-specific training alone cannot account for the sex composition effects among men. A large portion of the sex composition effect among men is due to industry-specific effects. How are industry-specific effects measured? They are measured by a set of industry dummy variables that, by construction, are free from any rater's bias. The invisibility thesis therefore fails to offer a coherent challenge for any of the central findings of this article.

### Gendered Bias in the Returns to Skills

Yet another variation on the theme is the conjecture that devaluation takes the form of gendered bias in the returns to female skills: "If a type of skill or working condition has traditionally been associated with women's work in either the household or paid employment, it may come to be devalued via stigma that gets institutionalized into wage systems, so that this skill or working condition comes eventually to carry a low rate of reward, or a penalty, whether it appears in a male or female occupation" (England 1992a, pp. 39–40). Even if a skill normally associated with female occupations is properly acknowledged in the *DOT*, the returns to the skill may be biased downward due to devaluation.<sup>29</sup> In the extreme, female skills are even thought to garner negative returns as a result of the cultural devaluation of feminine skills (England 1992a; Kilbourne et al. 1994).<sup>30</sup>

<sup>29</sup> Strictly speaking, the concept of differential returns to male and female skills is ill-defined. Most skills (e.g., mathematical ability and social ability) are defined in terms of qualitatively different metrics. We will return to this compatibility problem shortly.

<sup>30</sup> The alleged negative returns to female skills are primarily based on the negative wage effects of nurturant skills. Yet the theoretical justification is dubious: Why should *any* skill of an occupation garner a negative wage effect? Since the negative return is *conditional* on a wide range of occupational attributes, it apparently suggests that *an occupation pays less for more of the skill that is demanded of a worker*. If so, how is this possible in any market? If more of a skill is worse, why should anyone supply the skill? Furthermore, the empirical evidence on a negative return to female

As a variant of the devaluation hypothesis, the argument holds that sex composition effects are the indirect consequence of omitted variables that capture the gender mix of unobserved skills for different occupations. When the gender mix of skills is unobserved, sex composition can be regarded as a proxy for the relative share of female skills in the unobserved mixture of skills. A predominantly female occupation is expected to have more female skills in the mixture. If female skills are less rewarded than male skills, the lower returns to female skills would give rise to an apparent sex composition effect against female occupations. Given the conjecture, we would expect to observe a negative sex composition effect against female occupations insofar as some valuable skills are left out of a wage model.

Two of the implications of this argument are noteworthy. (a) Since relevant skills are never completely measured, one will expect to find a persistent residual sex composition effect against female occupations. (b) Devaluation indirectly lowers the skill levels of female occupations. Lower returns to skills would induce lower incentives to invest. Even if female occupations are indeed lower in specialized human capital, this differential may be due to gendered bias in the returns to specialized skills.

This study has produced evidence relevant to both implications. First, we have examined whether there are persistent, unexplained sex composition effects. The central findings of this article consistently refute the first implication because the apparent sex composition effects are successfully explained by the simple specialized human capital models. Even though the models have certainly left out some unmeasured skills that are valuable in the labor market, they are irrelevant to explaining the sex composition effects. Thus biased returns cannot explain the previously unexplained sex composition effects. Occupational differences in specialized training remain the most parsimonious and effective explanation of all the central findings.

Second, the findings allow us to compare the returns to SVP for men and women. The returns to SVP are relevant for two reasons. On the one hand, SVP is one of the most important sources of the sex composition effects. On the other hand, it provides a meaningful benchmark to compare female and male skills as it is coded in the same metric of training

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skills is impaired by the highly problematic and confounded measure of "nurturant skill." Kilbourne et al.'s (1994) decomposition analysis also ignores the implications of sampling uncertainty, the biases of noisy correlated controls, and the potential biases of endogenous job changes. There is no reliable evidence on a negative return to nurturant skill and its substantial contribution to the gender wage gap. In fact, contrary to the expectations of the devaluation hypothesis, Kilbourne et al.'s (1994) table 2 shows a remarkably small (3%) net contribution of nurturant skill to explaining the gross gender wage gap of 50% (e<sup>41</sup>).

time for all kinds of occupation-specific skills. Tables 3 and 4 show that the returns to SVP for women are consistently higher (not lower) than those for men: .078 versus .069 among whites and .094 versus .079 among blacks.<sup>11</sup> As far as the training time in specialized human capital is concerned, there is simply no evidence for any gendered bias against the returns to investments in female skills.

Taken together, the refutation of both implications of the biased return conjecture casts doubt on the views that (a) gendered bias in the returns to skills may account for the previously unexplained sex composition effects and that (b) women may invest less in skills because of unfavorable returns to female skills. Not only is there evidence contradictory to the direction predicted by the devaluation of female work, there is hardly any evidence for the indirect devaluation of female work through biased returns to female skills.

### Aggregation Bias

One may argue that the evidence does not refute even the original formulation of the devaluation hypothesis because the evidence is only based on the sex composition of three-digit census occupations. By contrast, sex segregation is found to operate mainly at the level of very detailed occupations, and it operates even more extremely at the occupation-within-establishment level (Petersen and Morgan 1995). In addition, recent research appears to show that the sex composition effects at the job level are larger than those at the census occupation level (Tomaskovic-Devey 1993). Thus the defense for the devaluation hypothesis is that the empirical analysis of this article is inherently biased against the devaluation hypothesis by virtue of an aggregation bias—a downward bias on the estimated sex composition effect—from using more aggregated occupations than the hypothesis refers to.

In one sense, this defense is irrelevant to the concern of this study. When previous research fails to account fully for the sex composition effects, advocates of the devaluation hypothesis use the results as the key empirical basis for the hypothesis. Sex composition effects at the level of census occupations are inherently interesting and demand an explanation. The focus of this study is squarely on this finding and seeks to contrast two interpretations for it. Whether devaluation occurs at some other levels and contexts is beside the question. The question here is the proper inter-

<sup>11</sup> Since the parameter estimates for men and women are statistically independent and asymptotically normally distributed, we can conduct simple *t* tests to compare the coefficients of SVP for men and women. The differences of the coefficients are modest but not statistically significant at the .05 level.

pretation of the seemingly robust occupational sex composition effects at the level of three-digit census classifications.<sup>22</sup>

In another sense, the defense is simply ineffective because it is hardly conceivable that aggregation bias could have possibly been the cause of all the central findings in this study. Notice that I do *not* find a wage penalty *weaker* than expected under the devaluation hypothesis. Instead, I find that the apparent wage penalties against female occupation are completely explained across a variety of situations. One has to resort to some stringent assumptions to make the case that aggregation bias has coincidentally produced the absence of wage penalty in all of the situations.

Ultimately, the concern that the models of this article are subject to aggregation bias is misplaced. Although it is well known that regression models based on aggregated measures are vulnerable to ecological regression bias, our wage models are not ecological regressions because most of the variables are not aggregated measures. In fact, the kind of aggregation involved in the use of aggregated sex composition is unlikely to produce any bias that may have impaired the central findings. It can be analytically and empirically demonstrated that the errors resulting from aggregation will, under common conditions, be harmless for the purposes of this study. To see why this is the case, assume that *job* is the valid unit of measuring sex composition. Let OSC and JSC stand for occupational and job sex composition, respectively. Since OSC is based on an aggregation of jobs into occupations, we may write

$$\text{JSC} = \text{OSC} + u,$$

where  $u$  is a job-specific deviation from the occupational mean with  $E(u) = 0$  and  $\text{corr}(\text{OSC}, u) = 0$ . Now suppose the theoretically appropriate specification of a wage model is as follows:

$$y = X\beta + \delta\text{JSC} + e,$$

where  $\text{corr}(\text{JSC}, e) = 0$ . This implies

$$y = X\beta + \delta(\text{OSC} + u) + e,$$

$$y = X\beta + \delta\text{OSC} + v,$$

<sup>22</sup> If the focus of this article were the wage effects of sex composition of jobs, I would predict that the sex composition effects at this level would be much harder to explain away *even* if some kind of specialized human capital was the main underlying source of the effects. The reason is, while it is relatively straightforward to measure sex composition, it is not easy to measure job-specific human capital. In principle, we have to distinguish firm-specific, occupation-specific, industry-specific, and job-specific human capital. I am not aware of any data set that provides a measure of training time for job-specific human capital. The consequence is obvious: even if the specialized human capital hypothesis is valid at the job level, empirical tests will be biased against it and in favor of the devaluation hypothesis.

where  $v = \delta u + e$ . Even though OSC is an imperfect measure of JSC, OSC is uncorrelated with both  $e$  and the measurement error  $u$ . As a result, the classical measurement error bias does not apply here.

Most important, even if the causal structure operates at the job level while the data permit only a three-digit level occupational measure of occupation-specific training, there is unlikely to be any serious statistical bias when sample size is large. The key reason is that the simple process of aggregating occupation-specific training from the detailed occupational or job level to the three-digit occupational level renders the aggregated measure of occupation-specific training orthogonal to its measurement error. This orthogonality means that the aggregated measure can be interpreted as an instrumental variable for the unobserved occupation-specific training of a respondent's job. Thus OLS estimation of the coefficient of OSC provides a consistent estimator for the sex composition effect ( $\delta$ ; see, e.g., Griliches 1986). Contrary to the assumption of the first statistical defense of the devaluation hypothesis, aggregation may not have produced any downward bias on the estimated sex composition effect. Similar arguments hold for occupation-specific training.

Paradoxically, according to this perspective, the estimated parameters based on these aggregated measures of sex composition and occupation-specific training may be closer to the truth than those based on some noisy job-level measures of sex composition and occupation-specific training. To the extent that this is true, all of the results here are relevant even if devaluation is hypothesized to operate at a level more disaggregated than the three-digit census occupations.<sup>33</sup> In any event, the devaluation hypothesis would be hard to reconcile with the evidence here.

### Omitted-Variable Bias

The final defense for the devaluation hypothesis holds that the hypothesis postulates a gendered bias only after conditioning on a wide range of personal and occupational attributes.<sup>34</sup> Under this argument, all of the models in this study are inappropriate for testing the devaluation hypothesis because the models do not include proxies for the *occupational* requirements

<sup>33</sup> It is worth noting that the SEs of the parameter estimates will generally be understated. However, I have found that the coefficients of SC are already negligible and statistically insignificant when the SEs are understated, so if the SEs are properly adjusted upward to reflect the fact that occupational sex composition is just an instrument for the sex composition of jobs, none of the statistical tests of the coefficients of SC will yield a different conclusion.

<sup>34</sup> It is not clear, however, why a cultural devaluation process should operate in such a highly conditional manner. One cannot help but ask: Do people really hold all these things constant when acting their gendered bias against female work?



of general and cognitive skills. Presumably, the models are also faulty because of the omission of *worker's* general and cognitive skills other than years of schooling and labor force experience. The robust finding that the sex composition effects are small and insignificant should be deemed unreliable.

The omitted-variable bias argument is a convenient defense for the devaluation hypothesis since virtually all previous studies in support of the hypothesis include a wider range of occupational controls than those of this article. There are, however, at least two reasons why the central results here are more credible than previous results. First, bigger models are not necessarily more reliable. If noisy measures of occupational requirements are included, the estimation of sex composition effects may well be more biased than the simple models of this study. This is a real possibility because of the rarely recognized consequences of the interaction between measurement errors and multicollinearity (see "Potential Flaws" in Sec. III above for a critique of the uncritical inclusion of correlated occupational variables in previous research). Bigger is not necessarily better. Messy controls produce messy estimates.<sup>35</sup> The simple models here are not vulnerable to the same problems that plagued previous research. As it turned out, the results are robust and perfectly interpretable in terms of the specialized human capital hypothesis.

Second, if the models here include additional control for all stable unobserved worker attributes (including general and cognitive skills) without incurring the problems of most previous panel studies, the results will likely be even more favorable for the specialized human capital hypothesis (see "Potential Bias?" in Sec. V above). One of the robust findings of Macpherson and Hirsch's (1995) study is that there is positive sorting across male and female occupations. Workers with more valuable labor qualities are more likely working in male occupations than in female occupations. This positive sorting implies that additional control for stable general and

<sup>35</sup> General educational development (GED) is the commonly used DOT measure of general language, reasoning, and mathematical skills. It is a classic example of a DOT construct that is subject to serious measurement error, highly correlated with SVP (.734 among white women and .819 among white men), and invariably included with SVP in the oft-cited studies purportedly providing support for the devaluation hypothesis. The definition of the construct is telling (see also Tam [1996], available upon request, for a further analysis of GED). First, the GED is based on three component measures from the fourth edition DOT—general reasoning development (GEDR), general mathematical development (GEDM), and general language development (GEDL). Second, each of the components is measured on an ordinal scale of six levels, without any comparability across components. Third, and most significant, the GED for a DOT occupation is defined as the *highest* level of the three GED components, *despite the lack of comparability in the meaning of each level across the components*. The validity of the GED is therefore suspect.

cognitive skills will produce even weaker negative sex composition effects against female occupation than are found here. Thus proper control for stable, unmeasured general and cognitive skills of workers will likely reinforce rather than reverse the central findings of this article.

## VII. CONCLUSION

This article has reexamined the devaluation hypothesis and showed that there are strong reasons to question its validity as an interpretation of the previously unexplained sex composition effects against female occupations. Contrary to popular belief, the empirical literature to date provides only dubious support for the devaluation hypothesis. Previous evidence is plagued by the problems of measurement error that are aggravated by fixed-effects models and correlated control variables. Since previous research fails to guard against these problems, a cautious interpretation of the cumulative evidence would stress that previous findings are biased in favor of the devaluation hypothesis and do not provide any reliable refutation of the specialize human capital hypothesis. The new empirical findings reported here further demonstrate that the sex composition effects are largely attributable to the wage effects of occupation-specific training and industrial mix. All previously unexplained sex composition effects are now statistically explained—this holds at least in the late 1980s. The new evidence thus contradicts the devaluation hypothesis. Equally important, the patterns of deviations from the devaluation hypothesis are either predicted by or totally compatible with the specialized human capital hypothesis.

Two clarifications of these conclusions are in order. First, as emphasized in the introduction, this article only addresses the question of occupational gender inequality—the wage effects of occupational sex composition. This study does not address the question of occupational allocation, which is neither the focus of the devaluation hypothesis nor the apparent key to the puzzle of sex composition effects. Although the article does not address the question of why women typically are in occupations with lower levels of specialized human capital, the findings and conclusions clearly point to the usefulness of further research on the following allocative question: Why do men tend to get occupations that require more occupation-specific or industry-specific training than is required for traditionally female occupations? To be sure, both discriminatory and non-discriminatory forces may be operative in the allocation of workers across occupations.<sup>36</sup> The findings here *do not* contradict the view that there is

<sup>36</sup> In line with the expectation of economic theory, a recent econometric study by Gupta (1993) finds statistical support for the simultaneous operation of employer choice (el-

systematic gender discrimination in the allocation of workers across occupations and jobs (see, e.g., Bielby and Baron 1986; Jacobs 1989; Reskin and Roos 1990; Barron et al. 1993; Tomaskovic-Devey 1993). Although the results are inconsistent with the kind of gender discrimination embodied by the devaluation hypothesis, the findings are compatible with other forms of sex discriminatory practice that may affect the allocation of workers across occupations.

Second, the findings suggest that *on average* there is no wage discrimination against female occupations. It is important to underscore the distinction between (a) the absence of wage discrimination against female occupations on average and (b) on individual instances. This distinction is rarely made in the literature. Even if there is no wage discrimination on average, there may be substantial wage discrimination on individual instances. The reason is simple: wage discrimination against any female occupation may be offset, for instance, by wage discrimination against some male occupations. Conventional statistical procedures are designed to detect discrimination only if individual instances of wage discrimination aggregate to systematic occupational differences in wages.

The distinction may provide a way to reconcile the observations that (1) prejudice against women and gender discrimination against female occupations seem to be prevalent in real life and (2) the results here show that occupational sex composition does not have any average wage effect. The most convincing evidence for the first observation comes from anecdotes and case studies. However, anecdotes and case studies can be misleading about the generality and the aggregate consequences of individual incidents of discrimination. The two observations are not necessarily contradictory. In fact, they are likely complementary facts of the U.S. labor market. Both anecdotes and the quantitative evidence may correctly reflect parts of the reality. The quantitative evidence suggests that it is dangerous and misleading to generalize from individual incidents of discrimination against female occupations to systematic wage penalties against female work in general.

I have also carefully considered alternative defenses of the devaluation hypothesis. Once precisely formulated, the alternative defenses are untenable because they either require stringent assumptions, are factually wrong, or logically incoherent. Thus we are led to the same overall conclusion: in the late 1980s, there is no evidence that the devaluation of women's work is the source of the apparent wage penalties against female occupations. As far as occupational sex composition effect is concerned, then, the evidence does not support the devaluation hypothesis.

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ther because of taste or statistical discrimination) and worker self-selection in determining the occupational distributions of men and women.

Finally, the theoretical implications of the results can now be summed up in two statements:

1. Despite the anecdotal, qualitative, experimental, or survey evidence, previous research might have prematurely concluded that devaluation has been, and still is, the major force giving rise to the apparent wage penalties against female occupations.
2. Although conventional human capital variables fail to account for the sex composition effects, both occupation- and industry-specific training are critically important sources of the apparent wage penalties against female occupations.

While previous research has suggested that female occupations tend to require less specialized (occupation- and firm-specific) training, this study is the first to show that female occupations pay less than male occupations among women largely because of lower requirements in occupation-specific training and that female occupations pay less among men because of lower requirements in occupation-specific training and, apparently, in industry-specific training as well.

Partly because of previous theoretical oversight and partly because of the lack of data on specialized training, we know relatively little about the import of occupation-specific and industry-specific training in labor market processes. One of the central lessons from this study is that the role of specialized training in occupational gender inequality is much more critical than is commonly recognized. A sequel to this study will examine, for instance, the role of specialized human capital in gender inequality, including the mechanisms driving the gender differences in the distribution of specialized human capital. It will also examine why industry-specific training plays a critical role in explaining the sex composition effects among men but not at all among women.

## APPENDIX

TABLE A1  
CORRELATIONS AMONG KEY VARIABLES

	School	Experience	Tenure	SVP	FT	SC	Log Hourly Wage
<b>Women:</b>							
School .....	...						
Experience .....	-.210						
Tenure .....	.039	.490					
SVP .....	.457	-.017	.116				
Full-time (FT) .....	.127	.053	.182	.195			
SC .....	-.017	.009	-.029	-.368	-.130		
Log hourly wage .....	.439	.130	.331	.451	.310	-.119	...
<b>Men:</b>							
School .....	...						
Experience .....	-.196						
Tenure .....	.010	.556					
SVP .....	.459	.037	.123				
Full-time (FT) .....	.126	.194	.191	.174			
SC .....	.234	-.090	-.039	-.062	-.142		
Log hourly wage .....	.408	.274	.369	.422	.364	-.051	...

NOTE.—See table 1 for the definitions of variables. Sample is drawn from whites in May 1988 CPS.

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To conserve space for the publication of original contributions to scholarship, the comments in this section must be limited to brief critiques. They are expected to address specific errors or flaws in articles and reviews published in the *AJS*. Comments on articles are not to exceed 1,500 words, those on reviews 750 words. Longer or less narrowly focused critiques should be submitted as articles. Authors of articles and reviews are invited to reply to comments, keeping their replies to the length of the specific comment. The *AJS* does not publish commenters' rebuttals to authors' replies. We reserve the right to reject inappropriate or excessively minor comments.

### HISTORICAL SOCIOLOGY AND CORE DATA: COMMENT ON ROBNETT

In recent years, the concept of identity or collective identity has come to have significant meaning among social movement thinkers, and Belinda Robnett should be commended for her efforts to adapt that concept to a sociological/historical examination of women within the Civil Rights movement. But as identity is a relatively new conceptual tool in the theoretical array of movement scholars, there is bound to be confusion and misapplication associated with it. Robnett's article, "African-American Women in the Civil Rights Movement, 1954-1965: Gender, Leadership, and Micromobilization" (*AJS* 101 [1996]: 1661-93), regrettably illustrates the tribulation that scholars in this area must endure when employing new ideas. She also makes some historical generalizations regarding gender bias and leadership within Civil Rights movement organizations that are of questionable validity, bearing out the painful reality that in order to do good historical sociology, one must first do good history.

Robnett is correct in pointing out very early in her article (p. 1663) that, as a function of identity, movement experiences are different for a participant when that person's ethnicity, class, gender, or sexual preference are taken into consideration. Yet, in her examination of formal leadership dynamics in Civil Rights movement organizations (pp. 1669-77), she considered only gender as a factor in the exclusion of women from

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leadership posts. I would argue that, in the context of the Civil Rights movement, the class of a participant had more to do with that person's eventual informal prestige and formal leadership role than did gender. Second, in the course of her article, Robnett makes reference to specific civil rights organizations, the Southern Christian Leadership Conference (SCLC) and the Student Non-Violent Coordinating Committee (SNCC). As her title indicates, she allows her readers to believe that the policies toward women in these organizations were a representative sample of movement organizations in general. In fact, the SCLC and SNCC were quite different in many respects from the third member of the civil rights organizational trio, the Congress of Racial Equality (CORE), not least of all in their policies and practices concerning gender. Lastly, Robnett ignores the extraorganizational circumstances that prevented some women from taking leading roles in the struggle and is satisfied with viewing sexism as the unmitigated root of the problem.

Robnett states that "males, in particular ministers, dominated the upper ranks of the SCLC hierarchical structure" (p. 1670), as if all men were considered equals. She forgets that African-American ministers were among the best-educated and most respected members of their communities, along with the fact that their roles as clergymen gave them a partial immunity from racial violence that other African-Americans did not enjoy. Hence they were the most logical choices for such positions. While I would concede that a good deal of gender bias was at work within the SCLC leadership, it was operating almost completely within a circle of African-Americans who were largely middle class and where working-class or underclass participants, irrespective of gender, were disqualified out of hand. This is apparent when the reader considers that prominent women in the SCLC, such as Septima Clark, Dorothy Cotton, and Jo Ann Robinson, were all well-educated people and that their ranks never included anyone without the proper identity of genteel blackness that systematically excluded the very people that the larger organization was supposed to help. Clearly, the factor of class identification had as much weight in the leadership equation as gender.

In my own research involving CORE, an organization that was dominated through its first decades by middle- and upper-class whites, blacks that moved into leadership vacancies tended to be equally balanced between women and men. Poor black women were more common in the Northern urban chapters, but no evidence of systematic exclusion of women from formal leadership positions emerged in any region of the country. There was however a good deal of class animosity, where poorer participants resented efforts by CORE to desegregate apartments, colleges, and businesses for middle-class blacks in place of carrying forth

projects to assist the ghetto underprivileged. Also, from 1963 onward, CORE went through a painful period of expulsions of white members from the organization that was a function of the growing collective ethnic identity that characterized black nationalist philosophy. Yet in regard to gender, CORE had a tradition of female participation and leadership going back to its founding in 1942. The promotion of women through the organizational hierarchy was an accepted fact from the early years through 1959 and a policy that continued when the racial composition of the organization dramatically changed in the mid-1960s. CORE commonly had women in formal positions of national responsibility and influence:

Bonnie Barrow, national secretary and Morris County, New Jersey, CORE chapter chairperson;  
Bernice Fisher, national policy advisor and St. Louis CORE chapter chairperson;  
Ruth Turner Perot, National Action Council (NAC) member (who formulated the black power philosophy for CORE);  
Evelyn Rich, NAC member;  
Lula Farmer, NAC member and New York CORE chapter chairperson;  
Catherine Raymond, CORE national treasurer;  
Shirley Lacey, CORE National Steering Committee member;  
Velma Hill, CORE National Steering Committee member;  
Lillian Smith, CORE Executive Board member;  
Fredrika Teer, CORE National Programming Committee assistant director.

Women were commonly found in leadership positions at the regional level as well:

Mimi Feingold, St. Helena and Feliciana Parishes, Louisiana, CORE chapter chairperson;  
Anne Braden, Kentucky Louisville, CORE chapter chairperson and Southern Conference Educational Fund (SCEF) officer;  
Erosanna Robinson, Cleveland CORE chapter chairperson;  
Gloria Brown, Detroit CORE chapter chairperson;  
Oretha Castle, Louisiana and Mississippi CORE field coordinator and New Orleans CORE chapter chairperson;  
Mary Hamilton, rural central Tennessee CORE chapter chairperson;  
Constance Perry, Lillian Palenius, Lynn Seiter, and Grace Ito, Washington, D.C., CORE chapter founders;  
Harriet Lane, Minneapolis CORE chapter chairperson and CORE national chairperson, 1949-50;  
Lynn Coleman and Juanita Morrow, Cleveland, CORE chapter chairpersons;  
Gladys Harrington, Harlem, New York, CORE chapter chairperson;  
Flora Harris, Rochester, New York, CORE chapter chairperson;  
Lavernne McCummings, Philadelphia CORE chapter chairperson;

Marlene Wilson, Columbus, Ohio, CORE chapter chairperson;  
Liz Fusco, Mississippi Freedom Schools coordinator;  
Lenora Thurmond, Meridian, Mississippi, Mississippi Freedom Schools director;  
Annie Devine, CORE activist, Mississippi Freedom Schools Executive Committee member, and Mississippi state representative for the Mississippi Freedom Democratic Party (MFDP) in 1964.

The above women are only a sample of many who were vested with formal power in CORE during its long existence. While the *CORE Papers*, edited by August Meier and Elliott Rudwick (1973b), and the CORE Archive in Madison, Wisconsin (Meier and Rudwick 1984), hold voluminous evidence of female participation and leadership, the above names are easily accessible in Meier and Rudwick's *Core: A Study in the Civil Rights Movement, 1942-1968* (1973a), their definitive book on that organization. Clearly, Robnett's (p. 1689) statement that "African-American women, for the most part, did not share formal leadership" is in error when applied to CORE. Also, Inge Powell Bell (1968), herself a highly reputed historian of CORE, fully analyzes the class and race issues that plagued the organization in its later years following the ouster of James Farmer as executive director. However, in that analysis, Bell, like Meier and Rudwick (1973a), never develops any theme of conflict surrounding the issue of gender.

It was particularly evident to me that Robnett avoided discussing CORE and centered her examinations on the SCLC and SNCC. Had Robnett taken a broader look at all three of the major organizations in the Civil Rights movement, I doubt that she could have come to such generalizations as women exclusively taking the role of "bridge leaders" and men solely being vested with "formal" leadership positions. But this oversight does not keep her from making blanket statements about the movement that are not universally applicable, or even marginally true, in the case of CORE.

The most troubling aspect of this article, however, is its apparent error in applying well-known historical facts regarding the overall strategy of the movement for civil rights. Robnett does not consider the historical significance of the ministerial cloth and its importance in the efforts of the movement to maintain a moral legitimacy that was a critical factor in securing white, liberal support. The earlier phases of the movement had a number of charismatic, articulate, and able black men who never ascended to the role of "civil rights leader," as we think of it now, because they lacked the moral mantel of a "man of God" (e.g., W. E. B. DuBois, Marcus Garvey, and A. Philip Randolph). In the period Robnett examines, women had not gained access to the pulpit in any significant numbers and have yet to make meaningful advances in this area, particularly

in the more conservative Protestant denominations, both black and white.

Nor does Robnett deal with the fact that, since women had been universally excluded from the pulpit, an all-male leadership posture by the SCLC was as much an accommodation to the expectations of powerful white men as it was a function of black male chauvinism. Clearly, the SCLC's moral high ground and its appearance, over and above the ugly face of white racism, was critical to the success of the entire Civil Rights movement. When the white majority of the nation came to view the movement for African-American equality as one focused on economic restructuring and political power as opposed to a moral, nonviolent movement headed by religious leaders, the movement lost its unassailable legitimacy and wide multiracial support. Robnett does not credit SCLC leadership or Dr. Martin Luther King, Jr., in particular, with understanding this. By the same token, SNCC, which Robnett admits was not dominated by clerics, suffered a loss of credibility as it became more radical and less conscious of its ties to liberal reform-oriented whites. In the cases of both of these organizations, Robnett fails to understand that female emancipation was not a primary goal for the men or the women of the SCLC or SNCC and that any expectation that such purposes should appear in relation to their respective leaderships is unreasonable. Consequently, any discussion of "reconceptualizing leadership" in regard to gender identity is not in line with the established facts.

While it is clear that female activists, both black and white, were learning from their experiences in civil rights organizations (Evans 1979), it is just as clear that a second cohesive women's movement had not been formulated during the heyday of the SCLC or the SNCC and would not be until the early 1970s. With these facts in mind, it is clear that Robnett is not, in effect, supplementing our knowledge of social movements but is attempting to alter the historical record by insisting that gender consciousness was intentionally marginalized in the exclusion of women from formal positions of organizational power. It would have been more interesting and more informative to answer the questions, *Why* the SCLC did not consider women for formal leadership? Were there covert strategies or tactics in use that required men to appear in control of the movement? Were men in the SCLC leadership concerned about their public posture in relation to black men in general? Did the nonviolent character of the movement affect the tactical strategies or leadership needs of these organizations? Simply to say that black male sexism was the reason why women were excluded from formal leadership is an oversimplification that leaves the reader without any insights as to how this research is generalizable or even useful in understanding the circumstances at work within each of these organizations.

Overall, although Robnett includes works such as David Garrow's *Bearing the Cross* (1986), Aldon Morris's *The Origins of the Civil Rights Movement*, (1984), and Coretta Scott King's *Life with Martin Luther King, Jr.* (1969) in her references, she apparently drew very little from them in terms of established facts regarding the organizational purposes and goals of groups like SCLC or SNCC. This is a crucial historical error on her part and throws the use value of this article into question. As a result, Robnett accomplishes very little but to further encourage and strengthen those who would attempt to erode the historical image of the SCLC in general, and Dr. Martin Luther King, Jr., in particular, in regard to their relations with women in the movement. She also leaves the door open for gross misinterpretations of the conditions facing women within CORE, which she does not fully address but never excludes from her general argument concerning institutionalized sexism in Civil Rights movement organizations. Therefore, the merit of this piece is minimized and cannot effectively inform scholars of the Civil Rights movement in regard to organizational strategies or the repute, scruples, and temperaments of its participants.

DAVID KEYS

*University of Missouri—Columbia*

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#### FORMAL TITLES AND BRIDGE LEADERS: REPLY TO KEYS

I would like to thank David Keys for his compelling comments. It is clear that he was disturbed by the findings and implications of my study. Many of his criticisms, however, are unfounded, and I will deal with each in turn. First, Keys states that I am "attempting to alter the historical record" (p. 1697 above). While I did not provide most of my evidence on CORE,

it was not for the reasons suggested by Keys. Given the enormous amount of data collected for this study, the evidence presented in the article represents only a portion of my findings. A more comprehensive and detailed analysis is forthcoming in my book, *How Long? How Long? African American Women in the Struggle for Civil Rights* (Oxford University Press, 1997). For now, I will address Keys's criticisms as succinctly as possible.

While Keys notes the number of women who participated in CORE at the city or national level, he omits many of the salient historical facts that significantly alter the conclusion that women were as well represented as men in formal leadership positions. Keys seems to miss my point that power was not synonymous with titles. While the distribution of titles reflected the exclusion of women from the national level of power, it did not render women excluded from leadership. Moreover, when women did have titles, they often had less power since such titles or the work associated with them adhered more closely to gender-based norms. For example, Keys lists Catherine Raymond as CORE's national treasurer but does not mention that she was also Executive Secretary George Houser's secretary. She clearly did not have the power that Keys assumes from simply looking at her title. Another example is Gladys Harrington, chairperson of the New York CORE chapter, who was at odds with the national headquarters and its officers precisely because James Farmer, national director of CORE, and Marvin Rich, executive director of CORE, asserted power over the affiliates. Nor does Keys mention that Gladys Harrington's position as chair was rigorously challenged by Blyden Jackson, a black male who was supported by Norman Hill, an individual very much a part of Farmer's inner circle. Moreover, Harrington's position was challenged by Marshall England, another black male, who was supported by Roy Innis, a proponent of black male leadership, who would later become CORE's national director.

As for Keys's list, which mostly consists of chairpersons in CORE at the state level, this in no way suggests that women had national power. While the wives of James Farmer, Norman Hill, and Marvin Rich (i.e., Lula Farmer, who worked at the national level as the bookkeeper, Velma Hill, and Evelyn Rich) occupied positions on the National Action Committee, which governed CORE, their positions as independent powers were even questioned by Harrington, who pushed for an amendment to bar family members from participation. As for Bernice Fisher, her active involvement in CORE was during its early years, primarily when there was a critical need for a secretary-treasurer, who would work without a staff or office. This was not unlike the position of Ella Baker as the acting executive director of the SCLC. As for Ruth Turner, she worked as a leader in the Cleveland chapter of CORE, not unlike, as Keys points out, many other women. Yet, even though she developed CORE's black power

philosophy, Ruth Turner was not voted national chair. Floyd McKissick was, with her subsequent appointment as his assistant in 1966. Her position in CORE was not unlike that of Diane Nash's, who coauthored the philosophy of SNCC but who never served as SNCC's chair. Turner, in keeping with many women bridge leaders, advocated a grassroots mobilization strategy, one which Ella Baker put into place in the SCLC and SNCC. And, Mary Hamilton organized fearful local communities in the South, not unlike many of the bridge leaders discussed in the article. Keys's argument that women were in regional (really more often local) leadership positions is correct, but this does not change the fact that at the national level, they did not hold formal positions with power. Their lack of national power often manifested itself in conflicts with CORE's national-level leaders. Even a simple reading of August Meier and Elliot Rudwicks's *CORE* (1973) would have given Keys most of this information.

Keys further criticizes the article for ignoring the class dimension, the ministerial role, and the related exclusion of uneducated/nonminister men, a position with which I do not disagree. I discuss this on page 1688; black male ministers were clearly "the legitimate formal leaders." Acknowledgment of this point does not undermine my argument, rather it further supports my contention that women were excluded from these positions. Moreover, I do state that gender was but one of the exclusionary constructs and that educational attainment was another. I also argue that men, too, were often positioned as bridge leaders. The point is not that bridge leaders were exclusively women, rather that it was the highest level of participation for them. Therefore, many knowledgeable and educated women found themselves excluded from primary formal leadership positions or even the inner-circle positions that "unsuitable men" often acquired.

It is precisely the fact that women were not privy to the ministerial level of leadership or its inner circle at the national level that led them to seek "free spaces" for their leadership. Moreover, these "free spaces" were often provided by ministers, who in spite of their education or status, refused to support the movement. Therefore, the movement required the efforts of others, the bridge leaders.

Nowhere in the article do I simply state that "black male sexism was the reason why women were excluded from formal leadership" (p. 1697 above). Although most of my interviewees acknowledged that men held most of the primary formal leadership positions, they were quite clear that this was not an issue at the time. Moreover, they conceived of themselves as empowered. I make these points on page 1676 of my article. Furthermore, on pages 1686 and 1687, I make clear that the male formal leaders, particularly Dr. King, needed to project an image of rationality and con-



trol in order to gain and maintain legitimacy with the state. Obviously, King and the SCLC needed to maintain the moral high ground, and this reality became a factor in their decision making. Equally clear is the fact that women, who would never be regarded as "the" primary formal leaders with either the movement organizations or the state, were not as constrained in their decision making. This, I argue, was positive, in that it divided up the powerful leadership strata, placing women and others who were qualified, but excluded, into the grassroots. Whether sexism emanated from within the movement or from outside pressures is hardly the point. We all know that the women's movement did not begin until the 1970s. We also know that black women have not shared all of the gender-based constraints imposed upon white women. Yet, we also know that black women did not share the same power as black men in the movement. And, we know that black women's history has been neglected.

Therefore, I find Keys's belief that my analysis of women's participation in the movement "accomplishes very little" and somehow "further encourage[s] and strengthen[s] those who would attempt to erode the historical image of the SCLC in general and Dr. Martin Luther King, Jr., in particular" (p. 1698 above) baffling. Clearly Dr. King and the SCLC were instrumental to the success of the Civil Rights movement. Dr. King's efforts, courage, and strength were immeasurable influences upon the movement. He gained the respect of not only his followers but of national and international leaders as well. The movement could not have succeeded without him or the many black ministers and male leaders who supported him. It puzzles me that Keys believes an analysis of black women's realities and positions in the Civil Rights movement would threaten the historical record of Dr. King and the SCLC. Rather, an understanding of women's positions serves to strengthen our knowledge about what makes movements succeed. In the case of the Civil Rights movement, an analysis of gender has clarified the fact that not only strong formal black male leaders but equally strong and courageous black women leaders served to create a successful movement.

BELINDA ROBNETT

*University of California, Davis*

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## Review Essay

### SOCIOLOGY WITHOUT SOCIAL STRUCTURE: NEOINSTITUTIONAL THEORY MEETS BRAVE NEW WORLD<sup>1</sup>

*Institutions and Organizations: Theory and Research.* By W. Richard Scott. Thousand Oaks, Calif.: Sage Publications, 1995. Pp. xvi+178. \$39.95 (cloth); \$18.95 (paper).

Paul M. Hirsch  
*Northwestern University*

The trouble with the new institutionalism is that it does not have the guts of institutions in it. (Stinchcombe 1997)

"Stability," said the Controller, "stability. No civilization without social stability." (Huxley, *Brave New World*, 1939)

W. Richard Scott's *Institutions and Organizations* is impressive and provocative. It brings some of organizational sociology's recent experiments with neoinstitutional theory's expansive claims about culture and cognition to a perverse but logical conclusion: a sociology without social structure. In this isomorphic brave new world, *compliance* is the goal shared by all institutional theories. In the preferred cognitive version, deviance seems defined out. Social control just "happens." Freed from the restraints of the values and norms modeled by alternative "realist" social theories, people instead appear empowered to more "freely" select behavioral scripts from a menu of prescreened, time-tested, societally legitimate, and

<sup>1</sup> Workshops and many colleagues—at Northwestern University's Workshop on Organizations and Institutions, Carnegie Mellon's Graduate School of Industrial Administration, and several sessions at the American Sociological Association's annual meetings—have contributed to the ideas presented here. I am especially grateful to Howard Aldrich, Mitchel Abolafia, Joseph Baumann, Kimberly Boal, Daniel Cornfield, Lex Donaldson, Mark Granovetter, Joseph Gusfield, Michael Lounsbury, John Meyer, Marshall Meyer, Mark Mixruchi, Woody Powell, Gerry Salancik, Mark Shanley, Jitendra Singh, Art Stinchcombe, Marc Ventresca, Mary Vogel, and Ed Zajac for their insights and their reactions to some of the positions taken here. While many of these discussions preceded the writing of this text, and some may strongly disagree with my conclusions, the comments and clarifications from all these colleagues are acknowledged with thanks. Direct correspondence to Paul Hirsch, J. L. Kellogg Graduate School of Management, Northwestern University, Evanston, Illinois 60208-2001. E-mail: paulhirsch@nwu.edu.

"taken-for-granted" assumptions. These always-stabilizing cultural rules appear, in turn, to be picked up by those far below and diffused as routines and performance programs, aligned with the larger frameworks and metascripts apparently constructed and mysteriously signaled (without clear or consistent transmission mechanisms) by a time-traveling, institutional version of the Wizard of Oz.

*Institutions and Organizations* is a fascinating, sometimes unwieldy mélange of several books and ideas, with contrasting messages presented in several voices and writing styles. First and foremost, this is an effort by organizational sociology's senior statesman, whose fundamental generosity and years of leadership are unquestioned here, to redefine and transform it into the interesting but also, unfortunately, authoritarian "new institutional" version he has been aligned with since the mid-1980s. It is an important statement for anyone interested in following both institutional and organization theories. In this book, Scott develops a new typology to incorporate many competing definitions of institutions, and his clear preference for one of them, along with a discussion of exemplars for each perspective (chap. 3) and a further coding of the literature along this and several other dimensions (chaps. 4–6). The book's 178 densely packed pages also provide the opportunity to admire the enormous wealth of scholarly works marshaled and reviewed while, at the same time, leaving ample room to disagree with, debate, and revise some of the conclusions and interpretations offered. While not always clear in the book, anyone who knows Dick Scott from meetings and other conversational settings knows he is more open to such alternative perspectives than a literal reading of *Institutions and Organizations* may suggest. This essay, however, is limited to the messages conveyed in the book under review.

The first section of the book is the strongest. It provides two dauntingly well-researched and characteristically insightful overviews of (1) the historical roots of the multidisciplinary concept *institution* and (2) the flowering of organization studies in sociology as a testing ground for Mertonian middle-range theory. The diplomatic and nonpartisan guide to the field that many will recall from Scott's nearly three decades of valuable integrative texts and review articles (see, e.g., Scott 1964, 1992) is present here, but this remains only through the book's two outstanding introductory chapters. The reader should be forewarned that an important shift in agenda and tone follows. After its opening chapters, *Institutions and Organizations* is no longer an extension of these earlier, evaluatively more neutral projects, nor does it present a dispassionate update. Quite the contrary. From here on, underneath the still-gracious and inclusive narrative, Scott presents a partisan and programmatic vision from which a significant body of the field is now excluded. He is a key player in the intramural wars going on between these factions.

The new roadmap of the field in *Institutions and Organizations* thus

presents a *new* Scott persona—that of the warrior-advocate seeking to move and rechannel organizational (also here renamed “institutional”) sociology in a new and different direction. Staking out and popularizing a series of what will be controversial positions, expanding and redirecting which frameworks and perspectives fit best into this subfield (which is the American Sociological Association’s third largest member group), *Institutions and Organizations* invites more challenges than the “fait accompli” tone of this work seems to presume. It should stimulate and provoke more programmatic debates than have Scott’s earlier, more integrative, and less controversial works.

I specifically note five such issues here: (1) The book’s advocacy of a *forced-choice conceptual dichotomy, rather than collaboration and continuum*, between (good) ideational and cultural versus (bad) realist and more materialist formulations; (2) its largely untestable, nonpredictive and *necessarily post hoc, rather than present or future orientation* toward research and social issues; (3) an *unnecessarily reified and functionalist framing of key concepts* that ignores or denies institutional, cultural, and structural conflicts, contests, and ongoing changes in real time, hence discouraging their study; (4) its increasing *disinterest in, verging on dismissal of, substantive actions taken by associations or organizational subunits at the local (community) level* rather than at the more procedural and formally controlling, abstracted, global and centralized (headquarters, national regime) layers of society; and (5) its persistent indifference to or avoidance of the strong *capacity of large and powerful organizations, like multinationals and international financial firms, to influence and “enact” the rules and environments* that frame governmental policies and affect people’s lives, rather than be seen as always simply responding to even larger (however mysterious) cultural forces bigger than these giant organizations.

An expansive neoinstitutionalism seeking to shift the organizational subfield’s focus more toward the macrosociological units of nation-states and the world system will need to encompass more research on policy setting and the interplay of competing interests. It should address, and provide analytical tools to assess, the wider political implications (raised by Tocqueville, among others) of its theoretically stripping the intermediate levels of society of a role in creating, reframing, or challenging the rules and orders they are strictly conceived here as more blindly imitating and following. Such neoinstitutionalism should also attend more, among its variables and problematics, to the important distinction between obtaining compliance through the use of coercive means versus reliance on voluntaristic action. By placing a higher priority on achieving conformity than on that conformity’s content and the means by which it is achieved, the neoinstitutional perspective invites serious questions about both the values it conveys and their broader implications. I will come back to these issues below.

Fortunately, *Institutions and Organizations* also provides a great deal of substance and theoretical value, to which we now turn.

## Institutional Theories before Institutionalism

Much of the challenge of this subject—to the author as well as to the reader—resides in the many varying meanings and usages associated with the concept of institution. (Scott 1995, p. xiv)

Institutionalism purportedly represents a distinctive approach to the study of social, economic, and political phenomena; yet it is often easier to gain agreement about what it is *not* than about what it is. (DiMaggio and Powell 1991, p. 1)

Because no single institutional theory can encompass all sociologists' claims to the concept, Scott notes at the outset of *Institutions and Organizations* the long-standing and wide diversity of definitions and the confusion over what meanings to attribute to the term "institutional." Much like the more recent contest and continuing confusion over the term "social construction," this ambiguity is only exacerbated when use of the same terms can simultaneously reflect and evoke entirely unrelated meanings on the parts of both their employers and recipients.

Scott addresses this dilemma up front by starting out with a brilliant review of the concept of institution: its variety of meanings and its uses and definitions across disciplines and over the last hundred years. This fascinating account of the sociology of knowledge and of the rich ironies found in early institutionalism's reversals of fortune is a scholarly tour de force that shows Scott at his best. We learn here, for example, that the pathbreaking social policy studies made by sociologist William Ogburn in the 1930s were made possible by an institutional economist, Westley Mitchell, who was a founder of the National Bureau of Economic Research. (As contemporary "neoinstitutional economics" no longer houses such interesting or important social research, one wonders which institutional subfield or disciplines would include work like Ogburn's today?) This survey of diverse literatures, yielding such a broad range of interesting information and intellectual connections, is important reading for both students and scholars of sociological theory.

The early institutionalists at the start of the 20th century came from economics and political science as well as from sociology. Although short-lived in these other disciplines, the earliest conceptions of institutions in economics and political science shared a strong interest in the variability and nonuniversalistic aspects of social institutions. While always aware of the rule-creating and rule-enforcing character of the law, constitutions, and habits and customs, Scott notes that the economists Veblen, Commons, and Mitchell "all criticized conventional economic models for their unrealistic assumptions and inattention to historical change" (p. 2). Where do new rules come from? Are they legitimate? Which parties had standing to participate in the negotiations? These are perennial questions from the "dynamics" side of the various institutional schools; they remain the is-

sues underlying many of the debates that still range around the rule-enforcement emphasis of subsequent neoinstitutional theories.

In other disciplines, political scientists Woodrow Wilson, L. A. Willoughby, and others, anticipating some of the interests of what are now called "new" institutionalists, examined and compared the construction and historical development of constitutions, legal codes, charters, and governance structures. In sociology, early institutionalism was fostered by Weber and Durkheim in Europe and independently developed in the United States by social interactionists (Cooley), pragmatists (Dewey), and pioneers of the Chicago school (e.g., Thomas, Hughes).

Most of the early institutionalists (who might agree with, but whose time period must be clearly distinguished from, the "old institutionalism" label used by DiMaggio and Powell [1991] to characterize much contemporary sociology) emphasized the capacity of institutions to change and be redefined, the importance of their symbolic and subjective meaning, their potential for instability, and the likelihood of conflict, negotiations, and contests surrounding their establishment and uninterrupted continuation. In political science (where institutionalism has since experienced a revival) and economics, this openness to the concrete and to process and realism made institutional scholars vulnerable to a successful attack in what Scott calls the perennial and still ongoing battle between studies of the temporal and particular versus more timeless and general models and formulations. In losing out to the more equilibrium-based and deductive framework of neoclassical economics on the one side and the more individualistic and behavioralist formulations of political science on the other, these early institutional frameworks were casualties and warning signs for what I have elsewhere called the continuing contest between "dirty hands and clean models" (Hirsch, Michaels, and Friedman 1987).

After tracing institutionalism's shaky beginnings, Scott turns to sociology's distinction as virtually the only discipline to retain an institutional focus after its exile from these other fields during the 1920s. Here, an institutional perspective has held forth pretty steadily (albeit in different versions; see Abbott 1992), from the beginnings sketched in chapter 1, through the works of, among others, Parsons and, most important, Merton in the 1940s and many of their disciples in the 1950s and early 1960s. These were followed by Janowitz, Zald, Berger and Luckmann, Perrow, and the more ideational turn by Meyer and his associates in the 1970s, through the contributions of Zucker, Granovetter, DiMaggio, Powell, and others from the 1980s through the present.

Because sociology has pursued an interest in institutions and their constituent rules far longer than in the workings of what were, until recently, the less powerful organizations that both administer and disrupt them, Scott next traces the fascinating story of how the still-tenuous connection between studies of institutions and organizations developed in the United States. In chapter 2, he also provides an interesting clue to the origin and recent regeneration of a mutual ambivalence, some continuing fault lines, and the competition between them over problem definitions.

Scott documents the rise of organization studies as the perhaps ironic by-product of the enormous impact that Merton's insistence on conducting empirical studies in the middle range, rather than pursuing more "grand theory," had on the discipline. In his famous manifesto against deriving explanations from preexisting and already worked-out "master conceptual schemes," Merton ([1949] 1967, p. 51) advocated the "realist" alternative of "developing special theories from which to derive hypotheses that can be empirically investigated, and evolving, not suddenly revealing, a progressively more general" comparative theoretical framework that can encompass greater variation and multiple cases.

For researchers with middle-range ambitions, complex organizations provided a cornucopia of research opportunities, sites in and around which theories could be developed and hypotheses constructed and tested. While the scope and number of organizations had mushroomed over the half century during which institutional theories were developing, organizations remained largely unexamined, providing rich grounds for carrying out middle-range research. Organizations offered a vast number of *bounded entities with internal structures and processes for examination, administered by sets of formal rules and informal networks, but also with connections to an outside world with which they exchanged the goods and services produced for needed supplies, financial support, and institutional legitimation*. For Merton, his students, and others, these quickly became ideal research sites for research and theory building.<sup>2</sup>

The new subfield grew quickly. Scott inventories how, in short order, several generations of remarkable Columbia Ph.D.'s and their followers, influenced by Merton's injunction to look at middle-range issues, came to establish much of the corpus of organizational sociology. These include the large group of now-classic studies by, among others, Selznick, Gouldner, Gusfield, Stinchcombe, Lipset, Coleman, Zald, Clark, and Perrow. Other Columbia faculty, notably Amitai Etzioni and C. Wright Mills, added additional and important perspectives on organizations and their increasingly critical role in American society.

Scott also points out here the influential and fortuitous result of the then-growing new field's perhaps-unexpected revisit to, and reconciliation with, grand theory when James Thompson—as *Administrative Science Quarterly's* first editor—commissioned Parsons's (1956a, 1956b) foundational essays on formal organizations. This formulation established the base for much of the open-systems and institutional theory to follow in organizational sociology by setting out and demarcating the *technical, managerial, and institutional* levels of organization. Scott credits Parsons as the first to so define the analytical boundaries separating the production- and efficiency-related aspects of organizations, at the

<sup>2</sup> For a discussion of how Merton's development of middle-range theory, in collaboration with Paul Lazarsfeld, also built and transformed the subfields of public opinion, mass communication, and community studies, see Frenzen, Hirsch, and Zerillo (1994).

technical level, from the more cultural and legitimacy-directed activities found at their institutional level.

It is interesting that Scott further points out, and dryly credits, Parsons for the continuities between this critical formulation of multiple levels and its later utilizations (reframed as "decoupling") and reinvention in "new" institutional theories. As we shall see, these more strongly contrast and dichotomize the different explanatory parts played by (a) the technical, more material level of organizations on the one hand "versus" the (b) now-greater and more important contribution attributed to myth, culture, legitimacy, and ritual on the other. While the latter still fits both Parsons's institutional level and his IL cells (Parsons 1951), his interest in the former, more economic and technical level (also found in his AG cells) is largely rejected and abandoned (Hall 1992).

This important chapter, suggesting how Parsons's and Merton's respective visions of (grand) institutional and (middle-range) organizational theories were earlier bridged by the field carries some important implications for proponents in their more recently renewed battles, with segmented analogues carrying titles like "new," "old," "recent," and "traditional" institutionalisms (Hirsch and Lounsbury 1997).

### Institutional Theories Updated, Newly Codified, and Redirected

"Enough of history! Where does the area of institutional theory stand at this point in time?" (p. 33) With this exclamation, Scott shifts away from his valuable recounting of how organizational sociology arose in order to present the analytical centerpiece of the volume. His new typology offers three "institutional pillars," created to explicitly emphasize the differences ("underlying theoretical fault lines") that separate perspectives on institutions. Here, Scott also clearly rejects the counterstrategy of seeking ways to "view each of these facets as contributing, in interdependent and mutually reinforcing ways, to a powerful social framework" (p. 34). This decision to look for, focus on, and so *emphasize the differences* between perspectives is unfortunate, especially considering the author's demonstrated talents and earlier predilection to bring together, find similarities, and *integrate* divergent perspectives.<sup>1</sup>

For this ambitious new etic typology, Scott creates three nominal categories—the regulative, normative, and cognitive "pillars," into which virtually all of the work he sees pertaining to institutions, across disciplines and subfields, has been coded. These three categories incorporate the legal (regulative), social (normative), and cultural (cognitive) aspects of institutional theories and research. For Scott, these must be kept analyti-

<sup>1</sup> When he discusses and himself takes note of this decision (p. 34), Scott also refers the reader to his chapter, "Institutions and Organizations: Toward a Theoretical Synthesis" (Scott 1994), which largely coincides with and anticipates much in the book under review.



cally and, by implication, operationally distinct. Each pillar is self-contained, embodying its own internally consistent set of isomorphic rules, guidelines, and internal variations for all (micro- through macro-) levels of society. As Scott elaborates: "The models are differentiated such that each identifies a distinctive basis of compliance, mechanism of diffusion, type of logic, cluster of indicators, and foundation for legitimacy claims" (p. 60). Research cutting across these pillars is largely ruled out, discouraging interpillar communication and making the cross-fertilization of ideas unusual and unlikely.

Scott's requirement that these pillars remain analytically independent and separated seriously weakens the power of this otherwise intriguing and creative new typology. Operationally, I doubt all works touching on institutions can (or should) be fitted into the domain of just one of these pillars; or that many of the authors cited (myself included) would agree with the view that their work cannot also productively straddle them.<sup>4</sup>

Each vertical institutional pillar, self-contained and isomorphic, is set up by Scott to address, account for, and explain actions and behavior, in or at the following units and levels of analysis: individual and group, organization, organizational subsystem, sector and industry, organizational population, organizational field, societal, and world system. Into this grand scheme, feeder subfields from across disciplines are cross-classified in the resulting columns and rows of figure 3.1's "institutional pillars and varying levels: illustrative schools" (p. 59). These include economic history, historical institutionalism in political science, neoinstitutionalism in economics, traditional institutional sociology, neoinstitutional sociology, population ecology, evolutionary theory in economics, and ethnomethodology. Encompassing so much takes on the risk of making them unwieldy; as the boundaries and domain of the organizational "subfield" so expand, the lines previously distinguishing organizations, institutions, world systems, and the rest of the discipline also become hazy.

The challenge of so ordering and interpreting such a wide diversity of divergent thinking is, as Scott avers, indeed daunting. He begins this task with an "omnibus definition of institutions: . . . *Institutions consist of cognitive, normative, and regulative structures and activities that provide*

<sup>4</sup> Throughout their exposition, each pillar's exemplars are presented as belonging in and illustrative of that pillar only. While late in the book, Scott speculates that ad hoc combinations may be possible, the only (three) works coded as bridging pillars almost serve as exceptions to the noncrossing rule. After recognizing studies by Peter Hall, Richard Whitley, and Neil Fligstein for accomplishing this, Scott further characterizes them as all institutional histories at the macrosocietal level and then, as if to explain away the possible anomaly of their so bridging pillars, he adds, "Perhaps, at the macrolevel, where complex institutions such as markets are constructed to stabilize relations and behavior, the use of power and sanctions, up to some point, to reinforce these arrangements does not undermine their cognitive and normative supports" (p. 102). By implication, this cross-pillar interaction remains the exception and is not found at levels less macro than the market and the state.

*stability and meaning to social behavior. Institutions are transported by various carriers—cultures, structures, and routines; and they operate at multiple levels of jurisdiction*" (p. 33; emphasis added). For each institutional pillar to work, as we have seen, *compliance* is required. In keeping with a long tradition of sociological theory, this definition of institution analytically hones in on the problem of order, on stability and its retention.

While not necessary to discuss alongside "institution," introducing the corollary concept of *institutionalization* here as well would provide an early recognition and engagement with the more dynamic, process-oriented problems connected with the study of institutions. With the emergence and dissolution (as well as ongoing moves to assure the stability of) the structures and activities constituting present institutions, comes competitions over which candidates will succeed in becoming, and retaining, the mantle of being institutions. While discussions of these dynamics of the institutionalization process do appear in *Institutions and Organizations* (usually as very macro and longitudinal social constructions), they are not central to its main exposition nor easily aligned with the typology within which the book frames institutions and organizations. Scott's decision to treat separately and to defer raising these process issues until well after presenting the orienting "omnibus definition" is consistent with what Powell (1991), Zucker (1988), and Perrow (1985, 1984) have noted as the lower priority accorded political dynamics, social movements, and social change by "neoinstitutional" theory more generally.

Echoing parts of Etzioni's (1961) earlier, comparatively less macro typology of compliance structures—with its columns for "coercive," "utilitarian," and "normative" categories, but also an important provision for hybrid cells combining them—the subtext across Scott's three pillars addresses how the formulations grouped into each pillar assure stability, conformity, and compliance to rules of all kinds. In two pillars, these are assured by the individual or corporate actor's socialization into and internalization of values and norms that provide consciously understood legal, social, or moral rules and guidelines for behavior. In the third, cognitive framework, these assumptions and mechanisms are rejected. Here, people and organizations are conceived as selecting and conforming to behaviors to which they have no substantive or moral commitment. Instead, their conformity to rules is driven unreflectively by a more procedural mechanism: they are followed (no questions asked and without deviation) because, while external to the adopter, the rules are culturally legitimate if others with whom one socially interacts also follow them. This pillar's substitution for commitment is a world of subjective meanings without affect.

*The regulative pillar.*—For theory and research within the regulative pillar, coercion is the driver of compliance. In this Weberian legal order, correct procedures are specified with clear sanctions for noncompliance. The rule of law is clear, monitored, and enforced; utility functions are materialist and easy to construct. In the regulatory pillar's framework of

coercive isomorphism, all sectors of society conform to rules because it is expedient. Rational choice and instrumentalism prevail.

The subfields listed for this pillar in Scott's figure 3.3 (p. 57) are economic history, "historical institutionalism in political science," and neoinstitutionalism in economics. Exemplars coded by Scott as regulative institutionalists in these respective subfields include North and Thomas (economic historians),<sup>5</sup> Schmitter and Skocpol (each, individually, as a "political institutionalist"), and Williamson (a neoinstitutional economist). The regulative pillar's connection to sociological conceptions of institutions, organizations, and rule breaking seems coded as peripheral, as this pillar is the only one for which Scott omits an explicit reference to the discipline in the illustrative summary figure (p. 59; some additional, mainly rational choice-oriented, sociological works coded as "regulative" also occasionally appear in the text).

If there were room in any of the three pillars for studies of *nonconformity* or efforts to change laws, the regulative pillar would seem a logical place to begin. But in the midst of widespread disagreements over laws and social policy (especially at analytical levels *below* the regime and the nation-state), much organizational/institutional research on these disputes seems *precluded* from inclusion in this pillar. "Analysts working within this framework struggle a bit to explain why it is that institutions emerge . . . , but once they are in place, they have no difficulty in explaining why rules are obeyed" (p. 37). The logic of coercive isomorphism, and Scott's conception of this pillar as "a stable system of rules backed by surveillance and sanctioning power" (p. 37), channel potential studies of contests and *uncertainty* over rules or laws away from inclusion in the regulative institutional framework. Ongoing contests over sociological topics—such as assisted suicide, access to abortion, the utility of affirmative action or environmental protection, conflicts over which rules to enforce vary and are subject to change—may not provide rules "stable" enough for the subjects to be coded in the regulative pillar. Prospective studies of such social conflicts still awaiting societal (legal) disposition seem to be left hanging, perhaps to be postponed until *after* the issues in contention have been resolved. Scott may not intend to convey this conclusion; but his selection of exemplars that focus on and emphasize historical analyses at high macro levels leave unclear (1) if these examples qualify as regulative and (2) if not, then which organizational and institutional aspects of such contested rules and laws-in-process would or would not be claimed or welcomed in other parts of the typology.

If the regulative pillar's parameters are thus narrowed, the issue posed

<sup>5</sup> The intellectual journey of Douglass North, from the regulative pillar in which Scott codes his earlier work, through his increasingly greater emphasis on the importance of taking volition more into account when examining large-scale change, is traced and elaborated in Hirsch and Lounsbury (1996). Here, North's broader frameworks are, additionally, contrasted with that of Williamson's, whose work Scott also places in the regulative pillar.

here is no criticism of the research which is coded therein, for Scott's exemplar studies are all interesting, important, and first rate. If this pillar's primary focus is on unambiguous and uncontested rules and laws (with enough time-series data available to frame problems historically), then the more restrictive focus required for good work in this framework should be addressed and explained more directly. (Its converse should also be more clearly noted if known outcome and time-frame requirements are less of a prerequisite for inclusion.) While Scott does cite a few studies showing that laws governing issues such as property rights law emerged from a series of contests and negotiations between competing interests (p. 68), the image that still comes through is that these issues are not central to this pillar's problematics.

Ironically, the regulative pillar is thus narrower than the field of law and social science, which encompasses more of the dynamics surrounding the creation and retention of laws as well as their enforcement. This focus on competing and contending interests is compatible with the realist ontology that Scott defines the regulative pillar as following. Since monitoring is so integral to the successful administration of rules in this pillar (as Scott also observes in noting the necessity for third-party [e.g., government] enforcement of rules), the treatment of this pillar would also be strengthened by including more detailed examinations of the intermediate-level organizational mechanisms enforcing compliance.<sup>6</sup>

While these issues are all touched on by Scott, his treatment significantly understates the dynamics and variability of actions taken in the regulative pillar to develop information on how the rules are monitored,

<sup>6</sup> Game theorists' studying incentives at this intermediate level, e.g., would point to Americans' near-automatic payments of income tax as an excellent case in point. Among the intermediate-level mechanisms at work here are variations in, and political campaigns around, the enforcement budget and practices of one of the largest monitoring organizations in the world, the Internal Revenue Service. Another mechanism assuring compliance, by salaried and most hourly employees, is the automatic deduction of taxes and the reporting of incomes and interest payments by employers and financial institutions. Deeper knowledge of the operations of these important institutional facilitators and mechanisms, including variations associated with their own staffs' incentives, is a critical component for explaining the dynamics of how these organizational aspects of the regulative pillar actually play out (Stinchcombe 1997). In addition to focusing on the mechanisms of *how* rules are enforced and behavior monitored, game theorists have also long emphasized the importance of providing a combination of (not always coercive) incentives to encourage and induce compliance (Baird, Gertner, and Picker 1994; Libecap 1986; Milgrom and Roberts 1992). The dual issues of material self-interest and compliance based on a belief in the system's overall fairness are *both* included in many of these models. Policy-oriented game theorists explicitly address the contribution of perceived fairness and equity as incentives to people's *willingness* to comply. For example, the twin institutional goals of obtaining tax revenue both equitably (through redistribution) and efficiently (through monitoring and equal enforcement) have long occupied game theory economists (such as Nobel laureate William Vickrey [1994]).

enforced, and even renegotiated at intermediate organizational levels. The regulative pillar, in which social control is most explicit and straightforward, cannot ignore or operate in a vacuum so free of politics, process, and value conflicts. Much as Scott cursorily acknowledges Weber's observation that raw power works less effectively than willing compliance, an important insight of game theory is also to take compliance in the regulative pillar as a serious problematic, as more of a variable than a given; in short as more fragile and sociological than the more simple coercion-as-sole-mechanism Scott seems to define for this pillar.

*The normative pillar.*—With sociology's place in the regulative pillar restricted largely to studies framed in rational choice assumptions, we move next to the normative pillar. Given only two remaining forced-choice coding categories, if a study concerned with organizations or institutions is not in the regulative pillar, it will either be fitted into the normative framework or else the last available but far more restrictive "cognitive" pillar. Presented with this choice, we discover a giant residual category: *most sociological work touching on institutions and organizations must be coded as belonging in the normative pillar*. Depending on the issue at hand, Scott variously defines the normative framework as occasionally allied intellectually with both the otherwise unbridgable realist-versus-social constructionist ontologies, and with their related regulative and cognitive pillars. The normative pillar catches studies that may fail the more restrictive conceptual "purity" tests for inclusion in each of these other pillars' more narrow definitions.

The "social realist ontology" conceives the everyday "real world" as tangible, down to earth, and objectively "out there." In March and Olsen's (1989) terms, compliance in the regulative pillar (with which Scott identifies realism most closely) follows the logic of instrumentality, in contrast to the normative pillar's comparatively broader logic of appropriateness. While the former is not complicated, since the correct rule to follow is always clear and mandatory, the logic of appropriateness permits a greater social orientation. In the normative framework, there is thus more volition, with the more social, as well as purely material and instrumental, bases influencing the resulting decisions.

With the normative pillar defined as the exclusive home for the disciplinary concepts of socialization, norms, values, and social context, we find the broadest overall framework for sociological work. We have already seen that, for Scott, the normative pillar also overlaps in some instances with ontologies and concepts more strictly coded in the other pillars. In addition, the normative framework is also the only institutional category to welcome broadly designed studies of intentional, self-conscious actions and competing interests; power and conflict; and the dynamics of change, social action, and policy issues. By broadly designed, I mean this pillar incorporates aspects of the concept "institution" (e.g., language changes) that overlap with, and would be otherwise assigned to, different pillars exclusively.

As sociology (as well as organizational studies) is generally taught and

understood, these wide-ranging concepts and topics are among those in the core of the discipline. Nearly all of them appear to be coded and channeled by this typology into the "normative" pillar and to be homeless and unwelcome in both of the others. *What brings together these concepts and topics in the normative category is their common provision for including both culture and social structure in the same studies and building in the capacity for individual and collective actors to act, and pursue independent goals* (see Hirsch and Lounsbury, 1997).

With such a wide array of issues and topics, the number and range of leading sociologists coded into the now-bulging normative pillar is enormous. In fact, unlike the depiction of the other two institutional pillars, each featuring some sociology along with models from economics and political science, *sociology is the only discipline Scott finds producing the works coded in, and constituting, the "normative" pillar*. Merton and (much of) Parsons are reunited in the illustrative samples provided. Also combined and coded here as exemplars of normative institutionalism (who might not agree with each other about much else) is the following diverse group: Burawoy, March and Olsen, Singh, Hughes, Krasner, Durkheim, Stinchcombe, Salancik, DiMaggio (sometimes), Mezas, Roy, and, at one point (p. 38), possibly Berger and Luckmann.

In a wondrously rhetorical slight of hand, the larger corpus of concepts and research, which these represent and which encompass most of the discipline, are next further combined and collapsed under the label, "*traditional [sic] sociological institutionalism*" (emphasis added). With so much of the discipline piled into the normative institutional pillar, what features exempt sociological work in the two other categories from this rhetorically negative framing and attribution? As we have seen, little from sociology is coded into the regulative framework. To qualify for inclusion in the remaining "cognitive" pillar, the work must avoid "realism" and be coded as addressing symbols or as apparently belonging in one of the following four "schools": evolutionary theory in economics, population ecology, ethnomethodology, or, at the most ideational macro level, neoinstitutional sociology.<sup>7</sup>

A common (normative?) interpretation of scholarship labeled "traditional" is that it is unoriginal and should be avoided, so it may surprise readers, and many of the exemplars not coded into one of the specialized "schools" in the "new institutional" and more "recent" cognitive pillar, to learn that these works are so out of date. One of my own studies actually is included as an exemplar for the more "politically correct" cognitive pillar. However, I see this particular study (Hirsch 1986), like others Scott seems to reductively code into a single category, as spanning at least two

<sup>7</sup> It is interesting that the terms *organisation* and *organisational* also progressively disappear after Scott introduces these adverbs, which also serve to substitute the terms *traditional institutional sociology* and *neoinstitutional sociology* for the normative and cognitive pillars, respectively, in later sections of the book.

of them (normative and cognitive). For now, I will just claim dual citizenship, even though Scott provides little reassurance that his coding scheme provides for this status.

Because Scott also frequently characterizes studies in the normative pillar as "early" (while also acknowledging that his literature review is skewed toward selections in the alternative, neoinstitutional cognitive category), it needs to be emphasized how many more innovative, important, serious, and creative organizational and institutional studies are *recent* and currently flowing into the normative pillar, than "early" implies. Too many such books and articles, in areas ranging from industry and market formations, historical contests and social policy studies, through industrial relations, organizational downsizings, and interorganizational networks, are neither discussed nor cited in *Organizations and Institutions*.<sup>6</sup> The issues addressed in these more "normative" contributions are also close, in both spirit and intent, to the work of many of the early institutionalists introduced in chapters 1 and 2.

*The cognitive pillar.*—The cognitive pillar is the last institutional perspective in Scott's new typology. In its "social constructionist ontology," actions are legitimated by their accordance with a range of appropriate external cultural scripts and assumptions, rather than the conscious adherence to internalized values, social obligations, formal laws, or "quasi-moral norms," all of which better characterize the realist ontology. The cognitive pillar replaces the preceding pillars' logics of instrumentality and appropriateness with its own logic of "orthodoxy." Here, behavior is driven by understandings ("social constructions") that although perhaps once contested, over time become so ingrained in and synonymous with the actor's surrounding culture and own subjective identity that they now comprise the set of taken-for-granted assumptions that constitute "normalcy," to which no one can imagine alternatives. These now-"preconscious" premises and heuristics for actors' rules and scripts (narrowing the thought process and making the choice of correct answers more obvious) also enable a much more routine, automatic, and change-resistant decision-making process (Zucker 1977).

Within this framework of mimetic isomorphism, culturally legitimate constructions are widely understood, imitated, and carried out. Change is conceived to occur slowly, over long time periods (DiMaggio and Pow-

<sup>6</sup> For example, among the more notable works in this set of missing citations are critical and noteworthy contributions by Abolafia and Kilduff (1988), Baker and Faulkner (1993), Granovetter (1992), Vicki Smith (1990), and Harrison White (1992). And, at the more macro, policy-relevant level of this pillar, at which cognitive "neoinstitutional" works are disproportionately cited while the normative pillar is underrepresented, Selznick (1992), Selznick and Nonet (1978), Etzioni (1988, 1993), Etzioni and Lawrence (1991), and Perrow (1984), e.g., have all published and continue to write important, institutional, and policy-relevant works; their publication dates are as "recent" as many of the cited studies in the cognitive category, for which this label seems reserved.

ell 1991).<sup>9</sup> Though some alternative scripts and solutions may become socially constructed within these parameters, they must conform to and be legitimated by the wider culture before the potentially displaced routines, rules, premises, and holds on behavior can give way. The decision to adopt the unfamiliar or the unorthodox will be taken only if (a) those with whom one socially interacts are also doing so and share the same social construction, and (b) there already are culturally accepted working models of the (otherwise too deviant) behavior operating and available for imitation.

In Scott's focus on population ecology, ethnomethodology, semiotics, and evolutionary economics as the cognitive pillar's exemplar schools, he correctly credits them as major contributions. Collectively, they have reminded organizational sociology and the larger institutional field to accord more attention to the impressive staying power of existing industries, organization forms, structures, customs, and other long-lasting social arrangements. (Their stability and retention is Scott's primary interest here, more than population ecology's additional foci on birth and death rates, and, less frequently, variation and selection [cf. Singh 1994].) As exemplars in the cognitive pillar, Scott includes Glenn Carroll and Michael Hannan, Frank Dobbin, (some of) Paul DiMaggio, and earlier contributions at more micro levels by Burton Clark and Donald Zimmerman. To these, he also adds the more macro and ideational, culture-, and symbol-oriented school of "neoinstitutional sociology," noting the work of John Meyer as exemplary for its tracking of the diffusion and convergence of new constitutional forms across both state and world systems.

Three critical features Scott sees distinguishing the cognitive from the other two institutional pillars are (1) its choice of unit(s) of analysis, (2) the questions asked (vs. those neither asked nor seen as problematic), and (3) the lengthy time intervals deemed necessary to have passed before a study should be undertaken.

A *remarkable* example of framing (and discounting) questions to study at different units of analysis occurs in the following illustrative contrast. Here, Scott lays out what organizational researchers in the normative and cognitive pillars, respectively, should find most problematic in studying schools or hospitals. Studies in the normative pillar will inquire into why the mortality rate of surgeries is lower in some hospitals than in others, why some high schools do better at graduating students and sending them on to college than others, and (I would add) how the hospital or school is organized now and how it came to be that way. For the researcher being trained in the cognitive pillar, such empirical and socially relevant questions seem dismissed as dull and mundane or, at best,

<sup>9</sup> DiMaggio and Powell's (1991) influential essay about "new" institutionalism is an invaluable guide for background on these theories' underpinnings and philosophical basis. Scott's text sometimes appears to assume the reader's prior familiarity. See also DiMaggio (1997) for another excellent and more recent review. A close reading of their essay can fill in some of this information.



of second-order relevance. Skeptical about measuring performance in general, "the cognitive view insists that . . . as students of organizations, our task becomes *not simply to explain why one hospital is more effective than another, or why some schools exhibit more conflict than others, but to explain why some organizations are constituted as hospitals and others as schools. Where do these organizational templates come from, and how are they reproduced and transformed?*" (p. 44; emphasis added). Finding answers to these two latter questions requires covering (a) a much longer time period and (b) also *downgrades* the importance of investigating the more immediate empirical and policy-relevant research questions of why some schools and hospitals perform better than others—regardless of how they came to be named and assigned their respective tasks.

This illustration serves as a good example of the cognitive pillar's directing attention away from problems we can study and learn about today. Examining the histories and cultural characteristics of institutions appears to be mistakenly framed here as mutually exclusive from investigating why some of the organizations in them do a much better job than the others (notwithstanding Scott's earlier-quoted salute to the task having been merely "expanded"). Should violence in schools or malpractice in hospitals be dismissed as simply "part of the natural order" and not be studied as institutional? Are such topics to be shunted off to seek a place in the other institutional pillars so we can better redirect attention to more "serious" long-term questions? Both types of questions are legitimate problems for study. Indeed, even if the problematics of each could be so neatly coded into the separate institutional pillars, they may turn out to be interrelated: neither should be treated as a separate world in itself, set up as superior to the other, or framed as completely independent. While Scott's typology of three pillars provides useful analytical distinctions between these ideal types, the field must beware of their reification. The long-range and constitutive issues addressed in the cognitive pillar are obviously important, warrant serious study, and remain highly regarded in the discipline; but to frame them as a separate ontology, fully independent of their own consequences, is too limiting and can be self-defeating.

This shifting the focus of study, from the normative and regulative pillars' more substantive and structural foci to the cognitive framework's emphasis on ideal and cultural formulations, recurs throughout *Institutions and Organizations*. In addition to rejecting the more "realistic" assumptions and shorter time periods often studied by works in these other frameworks, studies in the cognitive pillar are also less likely to look at, or define as interesting, problems of organizational resistance, that is, adaptations, efforts, and strategies taken to overcome and reverse negative signals or feedback from their environments. For example, the contribution of works that could also be coded into the normative pillar for emphasizing the interests and contests involved in influencing regulators, bringing innovations to market, or pushing and pulling for political changes, would still show up, but only for providing valuable examples

of organizations carrying out approved cultural rituals—rather than for also actively seeking to manipulate situations, to carry out entrepreneurial visions, to influence the pace at which laws already passed are (or are not) being implemented, or to press for the passage of new ones. These aspects of social structure are all passed over and reframed as more symbolic and less dynamic.

*Organisational and political sociology.*—In his coding of studies' contents and his framing of the topics and issues addressed, Scott thus consistently highlights cultural and symbolic aspects while deemphasizing what may be an equal or even greater focus on more structural and substantive elements. The value of their emphasis on structural issues like resources, competition, and power is downplayed and reinterpreted as displaying the appropriate rituals and imagery for their cultural framing. Attention, in these examples, may be limited disproportionately to how the relevant language and symbols changed or, once established, to how quickly an innovation diffused and set a new standard; in both cases, their relation to the structural elements in the same study is minimized (Tolbert and Zucker 1995; Kraatz and Zajac 1996).<sup>10</sup>

A related theoretical transformation is a shift away from viewing organizations as organic entities and minisocial systems, with their own independent histories, decision-making authorities, cultures, and routines. Under the normative framework, organizations retain some analytical autonomy as tangible, midlevel units of analysis, whose employees and executives may exercise power and discretion to control uncertainties in their environments (Hirsch and Shanley 1996; Hirsch 1993). In the cognitive framework, there is much less provision for independent actions to develop or occur at this intermediate level. The diffusion or convergence of externally generated forms, symbols and activities, housed in or transferred among organizations, becomes the topic for empirical studies to focus upon.

Within the cognitive pillar's framework, topics and research sites at the intermediate level—ranging from voluntary groups like the United Fund, church congregations, and grassroots political organizations, to small and large businesses—all disappear as independent and culturally significant units to study. The organization itself is conceived as more of a passive collection point, to administer activities and to house and transmit new cultural accounts, following the legitimate rules and agendas institutionalized at a higher level. Rather than actively negotiate or co-opt elements of their environment (either singly or when combined into trade associations), organizations in this pillar are, instead, penetrated by

<sup>10</sup> A good example of this anomaly occurs in Scott's coding of my own study (Hirsch 1986) of complementary cultural and structural elements of the corporate takeover movement. Focusing on just the former makes it eligible to be coded as cognitive (as it is), while focusing on just the structural elements could frame it as normative. The explicit point of my article was that focusing on just one or the other is not sufficient and that, rather, both vary and need to be examined together.

an institutionalized culture, to which their decisions, activities, and routines all comply. The structural contingency view of organizations as actors striving to reduce uncertainty is replaced by a formulation closer to Fama's (1980) and agency theory's more economic framings of organizations as bundles of contracts and substitutable personnel from top to bottom.

In this disconnect between how each of these pillars defines what an organization is and what it can do, the cognitive category's units for analysis move both up and down a hierarchy of levels, away from organizations, per se, to study (1) social and linguistic interactions between individuals (from ethnomethodology) and (2) more societal, sectoral (field), and industrywide examinations of which cultural framings and organizational forms are the legitimate social constructions (from population ecology and "neoinstitutional" sociology). It is from these constructions that the appropriate rules for behavior, in turn, trickle down to be conformed to in organizations and other intermediate-level units. The cognitive pillar's illustrative schools have no reason to focus on organizations because the framework precludes them from exerting independent influence on what goes on inside or outside their (legal) boundaries. When actors conform to preconscious and external scripts that they have no part in writing, and their organizations have no distinctive (beyond legal) boundaries within which to act independently, there is nothing "organizational," per se, left to examine (Donaldson 1995). *Organizations as units, like most other intermediate units in society operating between interacting individuals and their external societally framed cognitive scripts, become treated (and delegitimated here) as no longer a serious source of ideas, activities, or social change.*

Taking these intermediate units out of the sociological loop raises some very serious, even "early institutional" political issues that is surprising to see unaddressed in *Institutions and Organizations*. Following Tocqueville's analysis of the American political system's stability, Kornhauser (1950) noted and warned that a prime contributor to preventing overcentralized, top strata and powerful elites from unilaterally constructing and dictating all the rules is a middle, intermediate-level stratum of healthy organizations and associations, developing and pursuing a wide diversity of forms and policies, and free from requirements to habitually comply with orders coming down from on high. Here, local is seen as an important countervailing force to global, rather than written off as needing to be brought more in line with and to reproduce external definitions.

Unfortunately, Scott's presentation and advocacy of the assumptions underlying the cognitive pillar has the unintended consequence of inviting questions about its political and social implications, in not only its organizational, but other dimensions as well. With the cognitive pillar's effective abandonment of social contracts built on shared values, norms, socialization, intentionality, affect, obligations, and, more generally, an awareness or concern with the consequences of one's actions, it remains a mystery how people and organizations handle the unexpected, the non-

routine, the conflicts whose resolution would otherwise be guided by the provision of an internalized set of moral or ethical values and norms (Selznick 1996). Unquestioning and preconscious compliance with assertions of historically legitimated, institutional mandates to kill one's former neighbors should not be the analytical equivalent of conforming to a taken-for-granted custom such as putting the garbage out every Tuesday in an American suburb.

These kinds of broad political questions need to be seriously addressed and clarified before the cognitive pillar and neoinstitutionalism diffuse too far in our field; when those explications occur, I can remove the reference to Huxley's *Brave New World* as important to this discussion. More questions about their political implications and other unanticipated consequences are raised and discussed in recent debates over the relationship between the constructs of legitimacy and density dependence (Zucker 1989; Baum and Powell 1995; and Hannan and Carroll 1995).

### Continuities and Complementarities in Institutional Frameworks

Like many excellent books, *Institutions and Organizations* raises many questions in addition to providing interesting answers. In codifying and explicating recent developments in institutional theory, this book is very provocative, providing contributions that I applaud and arguing for positions with which I have taken issue. While the distance between some of these opposing perspectives will narrow over time, a large unsettled question is whether the pillars in Scott's new typology differ only by matters of degree or—and this is more serious—whether they are conceptually different, as unresolvable “kinds.” Can the three pillars be complementary perspectives, with all candidates granted equal standing in the broader institutional field? Or do their different treatments of “social realism” versus “social construction” constitute barriers that may not be crossed? Where the cognitive pillar is treated as a more stand-alone institutional theory, I have argued that it is incomplete and that, rather than going its separate way, it is the openness of the normative framework to the insights and contributions of works in the cognitive pillar that should be better reciprocated.

One of the ironies of this, I believe, exaggerated dispute, is that *there is no critical test available to empirically resolve whether, indeed, compliance is based on following external cultural scripts or adhering to internalized norms or both at varying times*. Psychology cannot scientifically resolve which of these general models best explains the internal workings of individual decisions and behaviors that are outwardly identical. Without testable variation, the underlying causal path remains only a speculation. If the underlying psychological assumptions we adopt have no more empirical grounding than their alternatives, then the power granted competing “ontologies” relying on one or another of these assumptions may be unwarranted: neither realist nor social constructionist assumptions about information processing or motivation have marshaled enough evidence

to warrant becoming the foundation for a stand-alone institutional pillar that is demonstrably superior to its alternatives.

The strong contribution of Scott's typology is its success in laying out complementary perspectives, each of which can inform and learn from the others, and collaborate to further develop an inclusive and more integrated institutional framework. There are points in the book where Scott leads in this direction. He forges an intellectual alliance between the normative and cognitive pillars, for example, to fight off the rational actor framing of the regulative pillar; Scott also cites, as exemplary, studies in the normative pillar (such as Selznick's early works) to support his view that institutional theory has become more friendly to studies of social change. In conclusion, I see an excellent conceptual model here for encouraging investigators to locate additional complementarities and alliances across these institutional pillars.

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## Book Reviews

*Trust in Modern Societies: The Search for the Bases of Social Order.* By Barbara A. Misztal. Cambridge, Mass.: Polity Press, 1996. Pp. vii+296. \$54.95.

Chris Snijders  
*Utrecht University*

Barbara Misztal sets out to argue that certain changes in contemporary societies make the construction of trust more urgent and that the nature of trust necessary to sustain social order and the quality of social relationships is changing (pp. 8–10). Since trust can mean many things, the author first presents an extensive and thorough discussion on the diverse meanings attached to the word by other scholars and its relation to similar concepts such as “faith” and “confidence.” Misztal furthermore notices shortcomings in the self-evident nature of trust that classical sociologists such as Durkheim, Simmel, and Weber seem to propose and discerns three different kinds of trust connected with three different kinds of social order. *Stable order* is the kind of order in which trust is apparent as a routine background to everyday interaction. Here, trust consists of the formation of habits, reputations, and collective memory (trust as *habitus*). People are able to live more pleasantly given justifiable trust in their social environment. In *cohesive order*, trust is based on familiarity, bonds of friendship, and common faith and values as experienced in bonds with family, friends, and society (trust as *passion*). Finally, for *collaborative order* one needs trust to cope with the freedom of others and foster cooperation (trust as *policy*). Trust in other people’s reciprocity is an aid to overcome the detrimental effect of not being able to rely on others.

Misztal fills the last three of the book’s six chapters with an elaboration of her typology, mainly by positioning other work on trust and social order in this typology, thereby clarifying its meaning and impact. Taken together these chapters form convincing evidence that trust can facilitate social order in different ways and that the function of trust can be studied in the light of its role in one of the three kinds of social order. This typology and the place of classical and contemporary research on trust and social order in this typology is, I think, Misztal’s main achievement and one not to be underrated. The coverage of the literature on trust is excellent and ranges from the above-mentioned classics of sociology to recent contributions in anthropology, political science, and, of course, sociology.

The broad coverage of the literature is at the same time its weakness. The majority of the book actually consists of giving the literature its place

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in this new typology of social order, which easily distracts the reader from the author's reasoning. As the author argues (p. 9) it is one of her aims "to collect evidence in support of the thesis concerned with the growing significance of trust relationships." Setting aside the fact that one should not only look for supporting but also for other evidence, I think the book would have benefited from shifting its emphasis away from being a pure literature review and back to the reasoning underlying Misztal's main claim. The general argument is that the fact that trust gets increasingly important has something to do with globalization, an increasing demand for legitimacy of the state and increasing individualism. I am convinced the author will argue that I missed some important points by summarizing it this way, but this is almost inevitable given that the book is written as an array of linked references, quite densely filled with direct one- or two-sentence quotations from these references, and contains relatively little direct argumentation.

To summarize, I think the typology of social order (and the functions of trust in it) as set forward by the author is a useful one that can prevent a lot of common misunderstandings with regard to trust. The coverage and positioning of the literature on trust is outstanding, but unraveling the main thrust of the author's arguments about the increasing importance of trust is rather difficult.

Readers interested in an examination of which scholars occupy the sociological stage with respect to trust or in how the classical sociologists can be reinterpreted with respect to their position on social order should definitely obtain a copy of this book. Readers with a relatively up-to-date knowledge on the literature, who are willing to believe at the onset that trust is a complex, multifaceted topic that gets increasingly important, might find it enough just to scan the bibliography.

*The Cooperator's Dilemma.* By Mark Irving Lichbach. Ann Arbor: University of Michigan Press, 1996. Pp. xi+309. \$39.50.

Edgar Kiser  
*University of Washington*

Nothing better illustrates the increasing convergence of sociological and economic theory than studies of collective action. Not that long ago, almost all economists and game theorists stressed the ubiquity of free riding in the absence of selective incentives, while most sociologists argued that cultural and structural factors were the sole determinants of collective action. Although these types of arguments still persist in both disciplines, many game theorists are now trying to model the social contexts of collective action, and many sociologists are taking the problem of free riding seriously instead of assuming it away.

Mark Lichbach's new book not only documents this trend, it illustrates

the value of combining sociological and game theoretic insights. *The Co-operator's Dilemma* provides a comprehensive review of theories of collective action that arose in response to Mancur Olson's seminal work. Lichbach knows both the sociological and economic literatures well and is thus able to combine discussions of the Nash equilibrium in a noncooperative game with analyses of arguments stressing values or power.

The book is organized around a typology of four proposed solutions to the collective action problem. Market, or "invisible hand," solutions try to specify the conditions under which uncoordinated exchange relations between individuals in a "state of nature" will result in collective action. This set of solutions (Lichbach discusses 12 types in this category) focuses on exogenous changes in the costs and benefits of participation, the nature of the public good desired, the productivity of tactics, and the characteristics of actors (such as their propensity to take risks).

Contract approaches focus on how individuals construct rules and institutions that create and maintain voluntary collective agreements without third-party enforcement (i.e., states). This category includes various forms of self-government, "tit-for-tat" solutions, and mutual exchange agreements. This solution begins to incorporate more sociological factors, mainly in the form of existing social networks that facilitate the information flows necessary to monitor compliance.

The third and fourth solutions are more purely sociological, although game theorists have begun to explore both. Community approaches assume the prior existence of communal organizations with their shared sets of beliefs and values. This is the classic "cultural" solution to the collective action problem, and it works primarily through changing the microfoundations of action from pure rational self-interest to a value-based altruism toward the community. Hierarchy approaches assume the existence of third-party enforcement in the form of institutions that coerce individuals to contribute to public goods. This solution includes most power-based sociological arguments about collective action.

Lichbach evaluates each of these solutions in terms of their logical completeness and logical consistency. He argues that all of the proposed solutions are incomplete because each of them presupposes the existence of at least one of the other solutions. For example, he argues that contract solutions require markets to "permit parties to arrive at the terms of a contract" (p. 25); market solutions require community to create the trust needed to conduct market exchanges; community solutions require hierarchy because common values must be authoritatively enforced; and hierarchy requires contract "because in the very long run only mutually agreed-upon coercion will be accepted" (p. 25). While Lichbach is certainly right that there is some interdependence between each of these types of solutions, his argument that they always *require* other solutions is overstated. Contracts have often been made in the absence of markets, many markets are not supported by shared values, some communal values have persisted for centuries without hierarchical third-party enforce-

ment, and some autocratic hierarchies have lasted for centuries without mutual contractual agreements between rulers and ruled.

Lichbach argues that many of the solutions to the collective action problem are not logically consistent, since they jettison the collective action research program's core assumption of rational choice microfoundations (of course, this is not a core assumption for all who study collective action; Lichbach clearly situates himself in the Olsonian tradition). Lichbach argues that the collective action research program should not entirely abandon assumptions of rationality and self-interest (as many community solutions have), but they should not always retain the most narrow version of these assumptions either. His quite reasonable proposal is that rational choice microfoundations be used as an initial baseline and that they should be broadened only when rational action alone cannot account for collective action. This is an increasingly common position among rational choice theorists, but its lineage can be traced to a sociologist, Max Weber.

Lichbach's book illustrates the inevitable broadening of rational choice models as they have moved from economics into political science and sociology, at both the micro and macro levels of analysis. It should be a valuable resource for anyone interested in surveying the range of solutions to the problem of collective action.

*Detraditionalization: Critical Reflections on Authority and Identity.* Edited by Paul Heelas, Scott Lash, and Paul Morris. Cambridge, Mass.: Blackwell Publishers, 1996. Pp. ix+347. \$59.00 (cloth); \$21.95 (paper).

David Ingram  
*Loyola University of Chicago*

The decline of traditional authority and *gemeinschaft*, in the wake of what, since Weber, has been customarily denoted as social and cultural rationalization, has remained a popular theme among sociologists ever since Marx and Tönnies first broached the subject in the 19th century. As this anthology amply attests, rapid changes in the structure of capitalism, communication media, and international relations have provided a new generation of postmodern sociologists with additional food for thought on the subject. Here, in a single volume, an impressive cast of contributors range over such issues as postmodern morality, social systems complexity, cybernetic subjectivity, global space-time compression, multicultural immigration, and posttraditional communitarianism.

The volume's 15 essays are grouped under four headings. The essays grouped under the first heading, "Losing the Traditional," basically subscribe to the *radical* view that, as one of the contributor's (John Thompson) puts it, "with the development of modern societies, tradition gradually declines in significance and eventually ceases to play a meaningful

role in the lives of most individuals" (p. 28). Thus, Ulrich Beck and Elisabeth Beck-Gernsheim argue in chapter 2 that what is historically new about modern society is the emergence of radical individualism, in which institutions like marriage no longer confront subjects as an objective force limiting their free creativity. Individualism is also correlated with detraditionalization in the essays by Zygmunt Bauman, Thomas Luckmann, and Niklas Luhmann, in which the "break" separating traditional, stratified society from functionally differentiated society is seen as generating contingency in morals, preferences, and responsibilities, on the one side, and a corresponding yearning for inviolate values, on the other.

The second group of essays takes issue with the "triumphalist" view of detraditionalization emphasized above. The contributions in this section challenge the very notion of periodizing a radical break between traditional and modern society, arguing either that so-called traditional societies exhibit an abundance of individualism (Barbara Adam, John B. Thompson) or that so-called modern societies retain residues of tradition (Timothy Luke, Colin Campbell). Although most of the contributors to this *coexistence* thesis concede that detraditionalization has occurred, they insist that traditions rely on individual critical reappropriations for their continuous efficacy over time and space, and (conversely) that individuals rely on background habits for their freedom of thought. Luke, in particular, does a credible job of showing not only how what we call the modern has become traditional but also how (following David Harvey's well-known views about the postmodern condition) the compression of space and time due to new techniques of mass production, mass consumption, and mass communication has quickened the pace of life in highly unsettling ways.

The essays in part 3 discuss specific modern traditions, such as nationalism, humanism, universalism, and pluralism, and their impact on such notions as community, identity, and difference. Richard Sennett starts things off by exploring the tension between the 19th-century ideology of national identity—whose ethnocentrism resonates in the communitarian refrain voiced by today's advocates of a politics of cultural identity—and the experience of belonging to a universal community, which is the lot of the foreigner. Paul Heelas expands upon the notion of humanity as a universal community, arguing along Durkheimian lines that such a notion can serve as a solidifying morality against the relativism of more exclusivist notions of community and identity. This view also informs Scott Lash's critique of deconstructive philosophies of difference (Heidegger, Levinas, and Derrida), which fail to account for modern traditions of intersubjectivity and solidarity. Finally, in marked contrast to the previous contributors, Paul Morris appeals to the work of Jean-Luc Nancy in elaborating a deconstructive account of community that eschews traditional communitarian notions of identity, solidarity, continuity, and homogeneity. Especially intriguing is his insistence on the complementarity of egalitarian, localized communities of *descent* and hierarchical, universalistic communities of *assent*. The latter build on the former (as did early

Christianity on Judaism) while infusing them with singular and utopian meanings.

The last section, "Dissolving Detraditionalization," contains two essays, both inspired by the work of Michel Foucault, that take issue with the notion that the so-called "autonomous subject" of modernity is either autonomous or individual. Mark Poster, for example, argues that the modern subject is constituted by the effects of power relations that are configured in normed discourses and practices. Furthermore, he adds that a necessary correlate of the "autonomous individual" is a "superpanoptic," disciplinary society that conditions "subjects" as uniform (normal) yet de-centered loci of habitual practices. Developing this position further, Nikolas Rose concludes that the distinction between traditional and posttraditional loses its significance once it is conceded that persons in all societies are constituted by habitually ingrained norms of a heterogeneous and by no means continuous nature.

In sum, although there have been numerous anthologies on traditional, modern, and postmodern societies, few if any of them have devoted themselves as thoroughly to examining the validity of the conceptual distinctions implicit in this periodization as this book has. Thus it is essential reading for any scholar interested in what it means to be modern today.

*Democracy's Discontent: America in Search of a Public Philosophy.* By Michael J. Sandel. Cambridge, Mass.: Harvard University Press, 1996. Pp. xi+417. \$24.95.

Steven Tipton  
Emory University

If every sociology implies an ethic, as philosophers insist, it is no less true that every ethic implies a sociology. In making the latter point to political philosophers, Michael Sandel has performed a valuable service, starting with his criticism in *Liberalism and the Limits of Justice* (Cambridge University Press, 1982) of the abstraction of the socially "unencumbered self" at the center of John Rawls's *Theory of Justice* (Harvard University Press, 1971). In response, both philosophical defenders and critics of contemporary liberalism have grown more conscious of the sociology of the modern welfare state that their ethics typically imply, and more able to argue over its problematic premises and practices. The political salience of this argument has mushroomed in an era that spans the end of Lyndon Johnson's Great Society and the rise of current efforts to "reform" welfare for the children of the poor while protecting Social Security and Medicare for the parents of the middle class.

At the heart of democracy's discontent in our time, begins this book, lie two fears—the loss of self-government and the erosion of community. American political institutions embody ideals of justice and the good life defined in terms of rights and obligations, freedom and citizenship, de-

mocracy and law. So Sandel sets out to "identify the public philosophy implicit in our practices and institutions and to show how tensions in the philosophy show up in the practice" (p. ix). This may help us diagnose, if not resolve, our political predicament, lodged as it is in the moral contrariety of the liberal and republican traditions of our public culture, not only in the gap between our ideals and our institutions.

The liberal tradition conceives persons as free and independent selves, unencumbered by moral or civic ties they have not chosen. Freedom consists in the very capacity of such persons to choose their own values and ends. The rival republican tradition conceives freedom as the fruit of sharing in self-government with fellow citizens whose public-spirited character is cultivated by these very practices of deliberating together over common goods and sharing responsibility for the destiny of the political community.

In the first third of the book, Sandel shows how central features of "procedural liberalism" have come to inform the theory and practice of American constitutional law and family law in recent decades. Fair procedures take priority over particular moral ends posed as public goods. Individual rights function as moral trump cards, played to assure the state's neutrality among competing conceptions of the good life, in order to respect persons as free selves capable of choosing their own ends. Problems in theory show up in practice. Our "procedural republic" offers a kind of toleration undercut by its attempts at moral neutrality that often rely on implicit answers to the controversies they seek to bracket, for example, abortion. Treating persons as freely choosing, independent individuals may fail to respect persons encumbered by convictions or life circumstances at odds with this liberal self-image, whether they be Sabbath observers, Holocaust survivors, feminists fighting pornography, homosexuals denied privacy, or homemaker-mothers impoverished by divorce.

These practical difficulties, says Sandel, reflect liberalism's priority of the right to the good and of the self to its ends. Sandel doubts the capacity of liberal toleration to "realize the higher pluralism of persons and communities who appreciate and affirm the distinctive goods their different lives express" (p. 116). He doubts, too, that the liberal self-image of our times is adequate to self-government in the modern welfare state, for democratic self-government depends on consciously cultivated, public-spirited forms of republican citizenship at odds with individual autonomy. Moreover, the modern welfare state consists of a vast network of mutual dependencies, obligations, and administrative rules at odds with ideals of an unencumbered self at the hub of a social network of voluntary associations, free contracts, and consensual agreements. The expansion of government and the dominance of large-scale corporations have banished the voluntarist, autonomous self from contract law and from a political-economic order "increasingly governed by vast structures of power" (p. 118), Sandel observes. Meanwhile, ironically, this liberal self has come to prevail in our constitutional law. There, it justifies individual

autonomy in those matters of religion, speech, and sexual morality that now mark our personal lives, as if to console us for the sense of independent agency we have lost in public life.

The welfare state rings true to liberalism, grants Sandel, in its promise to individuals of legal and civil rights and social and economic entitlements. But public provision of these rights and entitlements depends on citizens sharing "a strong sense of mutual responsibility and moral engagement" (p. 119) in the common life of communities constituted by social practices that bind individual identities into social members obliged to one another and devoted to common conceptions of the good life. Without cultivating community in this constitutive sense, Sandel contends, the case for public provision in the procedural republic suffers a minimalism similar to the case for toleration.

The latter two-thirds of the book shift from constitutional to political debates. Stretching from Jefferson and Hamilton to Carter and Reagan, this section traces a shifting dialogue between two logics of moral argument in posing the key questions of public life. How can citizens become capable of self-government, asks the republican, who then seeks the social conditions and political arrangements needed to promote the civic virtue self-government requires and the liberty it breeds. The liberal first asks how government should treat its citizens, then seeks the principles and procedures of justice needed to treat persons fairly and equally as they pursue their various ends and interests. Republicanism predominates earlier in American history, liberalism later. But the two can be found counterposed all the way through, from deliberation over Virginia's Port Bill of 1784 to 20th-century debates over the New Deal, the Great Society, and Reagan's New Federalism. Noting expansion in the role of government following the Civil War and then World Wars I and II, Sandel seeks to show how the public philosophy of a procedural republic has come to distinguish our increasingly activist state since the New Deal and to frame the increasingly problematic terms of its justification (p. 124). The procedural republic, he judges, "cannot secure the liberty it promises because it cannot inspire the moral and civic engagement self-government requires" (p. 323).

What is to be done? The hope of our time, Sandel urges, rests with those "who can summon the conviction and restraint to make sense of our condition and repair the civic life on which democracy depends" (p. 351) in the corrective light of republican ideals. Is it possible to revitalize republican ideals of freedom, given the scale and complexity of modern society? Is it desirable, given their history of political exclusion and coercion in the hands of would-be moral aristocrats? Against these objections, Sandel follows Tocqueville's tack in favor of a democratic republic that is more clamorous than consensual, one that disperses state power and multiplies sites of civic assembly and formation. Forgoing fundamentalist moral absolutism on one side and protean moral drift on the other, Sandel's republican citizens of the future must be able to "think and act as multiply-situated selves" in democratic politics played out in social set-

tings that range from local neighborhoods to global political institutions. These citizens among us must be able to "negotiate our way among the sometimes overlapping, sometimes conflicting obligations that claim us, and to live with the tension to which multiple loyalties give rise" (p. 350). They must weave the various strands of their identity, culture, and institutional experience into a coherent whole in the form of moral narratives that order our diverse lives, interpret the common life we share, and anchor our political community even as they enable us to argue over its character, purposes, and ends.

This is certainly no small order. The reader may be forgiven for asking how it is to be achieved and the author for offering in reply only exemplary bits and pieces of a daunting puzzle: the Civil Rights movement, community development corporations, community organizing on the model of the Industrial Areas Foundation; recent political rhetorics of civic virtue and social responsibility to balance equal opportunity and individual rights; global prospects for diffusing the sovereignty of national states into a multiplicity of more and less extensive communities and political bodies (p. 345).

Like comparable works before it, including *Habits of the Heart* (University of California Press, 1985) and Michael Walzer's *Spheres of Justice* (Harvard University Press, 1982), *Democracy's Discontent* lacks a blueprint for repairing civic life in a democratic America. It also lacks a close analysis of those forms of economic inequality and social exclusion that bias our polity in the course of dividing Americans by class, race, gender, and generation. Yet Sandel senses the heavy toll these divisions exact from the coherence of public discourse and action in our time, and he sketches a civic rationale for public provision and investment to reverse them. Read in tandem with studies such as Verba, Schlozman, and Brady's *Voice and Equality* (Harvard University Press, 1995), Wilson's *When Work Disappears* (Knopf, 1996), and Fischer et al.'s *Inequality by Design* (Princeton University Press, 1996), Sandel's work sheds welcome light on the powerfully contradictory moral rationale that justifies our peculiar kind of political economy. In doing so, it underscores the need for the middle-class majority of our electorate not simply to check their interests and open their hearts, but to change their minds about our cultural commonsense, if we are to change for the better the way our public institutions work to secure justice, the general well-being, and the blessings of liberty.



*Conspicuous Criticism: Tradition, the Individual, and Culture in American Social Thought, from Veblen to Mills.* By Christopher Shannon. Baltimore: Johns Hopkins University Press, 1996. Pp. xvi+216.

Hans Joas  
Free University of Berlin

The recent upsurge of interest in American pragmatism and the widespread rediscovery of pre-Parsonian American social theory have increased the quantity and quality of scholarly output in these fields. Many of these contributions are clearly works of love, written to defend and revitalize older, but neglected traditions.

The author of the present contribution makes it exceedingly clear that he does not want to be counted among these lines of work. For him "critical, humanistic social science, so often arrayed against the market, has just as often been at the vanguard of extending the logic of commodification to the most intimate aspects of people's lives" (p. xi).

Such a viewpoint is not completely new to those familiar with Marcuse's "repressive tolerance" and the historiography of Foucault and his followers. But this is not the position from which this author argues. He describes his own perspective as "the Roman Catholic insistence that reason, belief, and even unbelief make sense only in the context of some received tradition of inquiry" (p. xiv). His heroes are the late Christopher Lasch and Alasdair MacIntyre. For him the rise of the work ethic in Reformation Protestantism amounted almost to a new Fall of Man; any critique of consumerism or the decline of communities based on the "producer" values of this work ethic is doomed to failure.

Scientific discourse does not evaluate perspectives, but results. The book contains critical interpretations and reexaminations of six crucial contributions in American social science and cultural criticism: the work of Thorstein Veblen, particularly his *Instinct of Workmanship*; the Middletown study of Robert Lynd and Helen Lynd; John Dewey's diagnosis of time in *Individualism Old and New*; Ruth Benedict's *Patterns of Culture*; Robert Lynd's *Knowledge for What?* and C. Wright Mills's work, particularly *The Sociological Imagination*. Shannon's chapters on these works are written in a deliberately provocative, sometimes brilliant, often flamboyant style. The scholarly quality of these chapters is very uneven. The first chapter (on Veblen) seems to me by far the best. The author gives a circumspect and serious, if not innovative, interpretation of Veblen's work, focusing on the notion of workmanship. This makes Veblen's views on the history of mankind and the American present look much more coherent and less idiosyncratic than they appear in other interpretations.

The chapter on Dewey, on the other hand, is clearly behind contemporary Dewey scholarship. The author introduces his interpretation by sweeping and superficial criticisms of Tocqueville's emphasis on voluntary associations and the expressive individualism of the Transcendental-

ists (see, e.g., p. 66). The crucial point of his critique of Dewey is surprising indeed: "Dewey's account of the lost individual proceeds from the classical liberal insistence of the priority of instrumental, man-thing relations to substantive relations between men" (p. 70). Is the author not familiar with Dewey's early neo-Hegelianism and its assumptions about human intersubjectivity? How would he deal with Dewey's use of George Herbert Mead's ideas about mutual role taking and the genesis of the self?

The chapter on Mills attempts to demonstrate that this radical democratic critic of the welfare-warfare state himself had a considerable affinity with technocracy. The author goes to great lengths (see p. 168) to support this claim by citing Mills's famous advice for the sociologist to keep a journal of all scholarly and private life experience to draw upon for research and writing. Mills himself compared the importance of a journal for a sociologist with its importance for a creative writer. For Shannon, such a journal is a kind of postmodern Puritan diary (p. 170) as well as a bureaucratic file ("the epitome of the bureaucratic organization of modern life" (p. 168) and "ultimately, the file is the factory that produces the commodified perspectives which flood the free marketplace of ideas" (p. 175). When self-exploration, bureaucratic administration, and commercialization are all the same, all sociological distinctions collapse.

When—to use Robert Bellah's terms—utilitarian *and* expressive individualism, Republicanism *and* Protestantism, are all considered alike in their deficiencies, we learn too little about any of them. In his desire to keep clear of the internal tensions of modernity, the author simply treats all cats as grey in the dark night of modernity. He concludes his book (pp. 186–88) with a polemic about abortion and a plea for "the great surviving traditions of the postmodern West: orthodox Judaism, Roman Catholicism, the Orthodox churches, and Islam." He is aware that his attempt to further his goals through this book on the history of the social sciences leads him to a paradox: by offering a causal account of intellectual-historical developments, Shannon, as he recognizes (p. xi), would subtly reproduce the worldview he tries to avoid. His alternative is to "offer not so much a causal account as an account of causality" (p. xi), whatever this may exactly be. The book falls not only as a contribution to intellectual history, but also—if I may respectfully say so—as a contribution to modern Catholic thinking. Contemporary Catholicism cannot avoid the pitfalls of modern culture, however much a quiet conservative extremism may long for a complete revision of modern history.

*Paradoxes of Modernity: Culture and Conduct in the Theory of Max Weber.* By Wolfgang Schluchter. Translated by Neil Solomon. Stanford, Calif.: Stanford University Press, 1996. Pp. 389. \$49.50.

Stephen Kalberg  
*Boston University*

Wolfgang Schluchter continues his massive exegesis of Max Weber's works in *Paradoxes of Modernity*. Part 1 "Truth, Power, and Ethics: Max Weber's Political-Philosophical Profile," argues that Weber must be understood as a philosophical thinker. It first focuses upon his concern to include ethical values in all political action. A chronology of Weber's political activities during World War I is offered and a detailed analysis of the content and background of the "Science as a Vocation" lecture. Schluchter calls attention to Weber's broader questions: how do we live ethically and what is the role of science and the nature of "vocational duty" in the modern world? He contends that Weber's politics were principled and grounded in values rather than "power-obsessed."

Schluchter seeks in chapter 2 "to uncover a typology of ethics that in part guides [Weber's] research" and, moreover, constitutes "Weber's own value position" (p. 50). This chapter interweaves Weber's biography and the Heidelberg social milieu with Weber's early inaugural lecture, his sociology of religion, and his writings on the Russian Revolution. Schluchter finds strong evidence for Weber's advocacy of ethical action and rejection of moral agnosticism and relativism as well as the success-oriented, adjustment-to-life, and eudaemonistic ethics. However, he incorrectly states that Weber views an "orientation to values, *especially to ethical values*, . . . as a general feature of human beings" (p. 121; my emphasis).

Written with a deep sense of dedication, this chapter reveals the very core of Weber's appeal to Schluchter. In arguing his case for a distinction in Weber's writings between a formal and substantive ethic of responsibility and a formal and substantive ethic of conviction, as well as between an ethic of reflexive principle and an ethic of principles, and, finally, for Weber's embracing of a "criticistic formal ethic of responsibility," Schluchter asserts that we can find in Weber an ethical system appropriate to our modern epoch, as Kant offered for an earlier era.

The multiple dichotomous concepts and schematic typologies in this chapter appear to me frequently strained, not least owing to the disconnectedness of Schluchter's finer distinctions from Weber's empirical sociology. As Schluchter states, "One has to be willing to speculate regarding Weber's position" (p. 89). Moreover, the Weberian "value theory" reconstructed here banishes, as unfortunate, all internal inconsistency and incompleteness. The tension between concept and empirical reality, the dynamic conflict across concepts, the heroic (and often failed) effort to address a social world perceived as exceedingly complex, and the ubiquity of power, domination, and conflict—all these central features of We-

ber's sociology are pushed aside. Instead, a static and highly abstract ethical system is offered. Weber's confrontations and struggles do not play a role but only that which so often eluded Weber: the firm outcome.

Part 2, "Religion, Economy, and Politics: Max Weber's Historical-Sociological Profile," reconstructs Weber's sparse and fragmented comments on Islam (chap. 3, "Hindrances to Modernity") and his multiple writings on Western Christianity (chap. 4, "The Emergence of Modernity"). Far from treating these "world religions" alone, both chapters emphasize "ideal *and* material" constellations. In doing so, they seek to "convey a relatively coherent picture of the Western trajectory from a Weberian point of view" (p. 3). Schluchter sees "chains of circumstances" and rejects forcefully the evolutionary positions of Hennis and Tenbruck.

In the context of a number of comparisons to Calvinism, chapter 3 first defines the religious ethic of Islam as "world mastery between world conquest and world adjustment" (p. 121). Its "feudal or petty bourgeois or booty capitalist" (p. 133) economic ethic was devoid of a thrust toward an "economically rational mode of conduct" (p. 134). In addition, Islamic feudalism erected strong obstacles to modern capitalism, the city never attained corporate independence, and the "stereotyped 'jurists' law" of Islam "prevented a logical systematization of law in terms of formal juridical concepts" (p. 164) as well as any subjection of domination to legal constraints. Schluchter's continuous comparisons to the medieval West allow a clear demarcation of the various ways in which Islamic civilization lacked the "structural heterogeneity and pluralism" (pp. 160, 164) characteristic of the Occident. He brilliantly takes the "many pieces . . . [or] historical prerequisites" and "[puts] the puzzle together" in order to explain "why rational capitalism [never] 'succeeded' . . . in Islamic civilization" (p. 166).

He then addresses directly the very different "individual constellation" that accounts for the Western trajectory. While the *Papal revolution* of the 11th and 12th centuries called forth a "relatively rational bureaucratic institution" independent of political domination and paved the way for "legislation by rational enactment," the *feudal revolution* "created Western knighthood, with its inner-worldly unified mode of conduct and the idea of the contractual character of political power" (p. 218), and an *urban revolution* "contributed to the development of the secular concept of the corporation and the 'birth' of a particular urban citizenry" (p. 218). Each of these spheres contained their own dynamic conflicts; their relative independence implied a structural heterogeneity and competition across "principles of organization and legitimacy" (p. 222) singular to the West. In addition, and not least, a "rational economic ethic" crystallized with ascetic Protestantism.

With the exception of the treatment of the ways in which a rational economic ethic arose from Calvinism (which remains quite abbreviated) and the near-total omission of the central concepts of *Verstehen* and meaningful action, this reconstruction is a powerful one that moves quali-

tatively beyond Schluchter's earlier attempts. It is unique in the literature.

This volume has been translated with great care by Neil Solomon. Although the juxtaposition in one study of "political-philosophical" and "historical-sociological" analyses prohibits clear thematic unity, Schluchter's refusal to simplify Weber's project and his sovereignty over the full range of Weber's texts renders this volume indispensable to all today interested in Max Weber. Historians of sociology in particular will find Schluchter's frequent discussions of historical and intellectual contexts quite helpful. Moreover, the dual foci of Schluchter's writings are clearly evident: his concern to reconstruct Weber's "theory of values" and analysis of the rise of the West.

In my view, Schluchter's interpretation omits fully the very core of Weber's achievement *as a sociologist*. He reads Weber either as a philosopher or as an historian utilizing sociological concepts and pays little attention to Weber as a sociological theorist and rigorous comparative-historical sociologist who undertakes substantive research by reference to an array of demarcated causal strategies and procedures. The societal orders (*Lebensordnungen*) and orientational models of *Economy and Society*, for example, which guide all of Weber's empirical work, appear prominently in Schluchter's reconstructions, yet only in an ad hoc fashion. He correctly insists that Weber rejects all closed causal models, yet each of his chapters moves too far in the other direction: each starts from scratch. While every chapter unravels masterfully a different deep reservoir of Weber's knowledge, each takes a relevant summary statement by Weber as its organizational framework rather than his *mode of analysis*, or practiced methodology. Repeatedly, in this regard, Schluchter refers alone to Weber's most global organizing mechanisms, such as "ideas and interests" or "chain of circumstances." Would sociologists read this classic today were he only an historical sociologist and theorist of ethical values?

*Between Facts and Norms: Contributions to a Discourse Theory of Law and Democracy.* By Jürgen Habermas. Translated by William Rehg. Cambridge, Mass.: MIT Press, 1996. Pp. xliii+631. \$40.00.

Joachim J. Savelsberg  
*University of Minnesota*

With this English translation, *Faktisität und Geltung* becomes available to a much larger readership. This is good, as Habermas tackles fundamental problems of modern constitutional states: from endangered social solidarity to a loss of direction and self-confidence in politics. In a Herculean effort, Habermas attempts to provide the intellectual tools toward understanding and solving them. In an age of completely secularized politics, he argues, the rule of law cannot be maintained without radical de-

mocracy and a new and common understanding of what constitutes justified interests and equal treatment.

As William Rehg, masterful translator, states, this book constitutes the "culminating effort" (p. ix) in a 30-year project. It resorts to philosophy of law, legal theory, and sociology. Given "considerable demands on its readers" (p. x), Rehg provides a length summary (pp. ix–xxxvii), and the author, in a 1994 postscript, takes "the role of an interpreter and attempt[s] to recapitulate the core idea that informs the whole book as he sees it" (p. 447).

In the first two (of nine) chapters, Habermas introduces the category of modern law in the context of his theory of communicative action. After neither historicism nor philosophical anthropology were able to establish practical reason, after Nietzsche denied it and systems theory erased it, Habermas pleads for communicative reason as an alternative, ascribed neither to macrosubjects nor individuals but to the socially integrative force of noncoercive processes of reaching understanding. Discourse theory, indebted to pragmatist thinking, leads to a proceduralist paradigm of law. Communicative action uses the rationality potential of language for social integration, and modern law fills gaps in social orders whose integrative capacities have been overtaxed (law no longer colonizes).

Habermas, while building on Kant, challenges Kant's subordination of law to morality for pluralist societies that lack normative consensus. This critique also applies to Rawlsian moves toward social contract traditions of rational natural law. Luhmann's systems theory on the other hand completely separates law from morality. Against both sides, Habermas builds on Weber and Parsons, and, through their critique, establishes the socially integrative meaning of legal communication.

Chapters 3 and 4 are dedicated to the philosophy of law, the former to clarify the relationship between law and morality, the latter that between law and politics. Again, Habermas claims middle ground between the liberal tradition à la Locke and civic republicanism from Plato to Rousseau: both private and public autonomy are basic for the functioning of democracy. In dealing with the role of state authority, Habermas links informal discursive sources of democracy with formal decision-making institutions. Law transforms communicative power into administrative power.

Habermas applies his philosophical design to current issues of legal theory in Germany and the United States, focusing on the tension between conformity to statutes and precedent versus rightness in terms of moral standards in judicial decision making and on the role of constitutional courts (chaps. 5–6). He proceeds to address sociological challenges to the application of normative theory. Chapter 7 outlines a model of deliberative democracy, and chapter 8 considers challenges of social power and systemic complexity. Chapter 9 provides a defense of a proceduralist paradigm of law in an attempt to bring legal and social theory together.

In his discussion of political sociology, Habermas rather bluntly sum-

marizes empirical findings as though instrumentalization of the political process and the role of expert knowledge in administrative-political control did indeed severely damage legitimate democratic rule. His examples for participation in deliberative democracy are unfortunately limited to groups with "good" causes. Theoretically, Habermas argues mostly with rational choice and systems theorists. The absence of other, especially network, literature as a reference point is astonishing. Habermas holds against "empiricist" approaches that they do not incorporate the normative foundation of a theory of democracy, a deficiency that his discourse-theory-based model of deliberative democracy is supposed to overcome.

The book is of burning interest to all who are concerned with basic challenges to modern law and democracy. Readers who master the challenge will gain enormously. Yet, many will not be convinced of the incorporation of normative judgments into an analytical theory. And, while Habermas does confront his theory with institutional reality, the complete lack of reference to any articles in the leading German or American sociology journals is a disturbing indicator. Sociologists know that even most carefully deduced, logically consistent, and convincing theories often flounder in the light of empirical evidence. Further, assumptions regarding the exclusively strategic character of economic and bureaucratic action or the complete lack of moral consensus in pluralist societies (even at a high level of abstraction), reveal risks of mistaking ideal types for empirical reality. And what about *empirical* consequences of participatory versus bureaucratized modes of decision making and *actual* implications of technocratization for legitimacy? Incorporating more insights provided by theoretically guided empirical social science would help toward theoretical and operational specification. Such intermediate steps are needed to bridge the gap between abstract deductive theory and practical solutions to problems of legal order and democracy. If Habermas does not do it, maybe others will. His challenge certainly deserves it.

*Michel de Certeau: Interpretation and Its Other.* By Jeremy Ahearne. Stanford, Calif.: Stanford University Press, 1995. Pp. ix+227. \$45.00 (cloth); \$16.95 (paper).

Jack Katz  
*University of California, Los Angeles*

I can only read Michel de Certeau with the interests of a research sociologist, who must ground observations about society in descriptions of people acting with reference to others in particular times and places. From that perspective, it is easy to dismiss him. His texts, full of statements of abstractions affecting abstractions, frustrate a research sociologist's desire to visualize what confirming or disconfirming evidence would look like. He has religious preoccupations with Catholic mysticism that show up in how he wrote and what he argues. And he is not the rage anywhere.

His work dates from the 1970s and now, about 10 years after his death and the English publication of *The Practice of Everyday Life* (University of California, 1984), the attention paid him runs primarily in small corners of cultural studies, French thought, and the history of ideas.

That is unfortunate, and Jeremy Ahearne's thorough and carefully crafted little book helps show why, even though it is not directed at sociologists. De Certeau wrote primarily about historiography, about various episodes of the interpretation and domination of folk cultures and folk spiritualism over several centuries in France, and about contemporary practices of everyday life. Throughout, he focuses on political, intellectual, and cultural efforts to define, and, in one sense or another, to suppress, phenomena that exist outside of a politically and economically rationalized social order. He is fascinated with a 16th-century text written about Brazil's Tupinambou people by a Frenchman who was stranded with them for three months. He dwells on the prosecutions of nuns possessed by the devil in 17th-century Loudun. He examines the Abbé Grégoire's efforts to "annihilate" regional patois. With Dominique Julia and Jacques Revel, he sees censorship in the Second Empire as the beginnings of a "castrating cult" that sought to undermine popular culture by idealizing it. And he considers the cunning, often devious, ways that people read, shop, and get around in cities today.

Lest this focus be dismissed as familiar, Ahearne makes clear that, in distinction from Foucault, de Certeau wants to bring out the survival, usually in a very distorted form, of what the disciplinary apparatus has tried to suppress. Thus the folk anthropological description of the Tupinambou not only set them up for Western manipulation, it also recognized and preserved their "otherness" in an "eroticism" of abiding fascination. The possessed of Loudun resisted their inquisitors with confessions that were so extensive, they became devices for maintaining the impenetrability of their spiritual world. The very effort to suppress regional affiliations has given surprisingly persistent life to efforts that resist national homogenization. And the rationalized maps and professionalized advertising of contemporary life is treated by a city's residents as the background for shortcuts, "poachings," and endlessly innovative bricolage work.

Reading de Certeau is an experience of alternating repulsion and fascination. I do not think that is accidental. De Certeau recreates for the reader both the relationship he had to his materials and the relationships of overt revulsion/suppression and secret attraction/nourishment that he tries to document in various historical settings. Just as one is ready to throw one of his texts aside in disgust, some damnably indispensable line of inquiry suddenly breaks through. Thus, in reading his treatment of 17th-century witch trials, it is easy to long for the eminently accessible prose of Kai Erikson's (*Wayward Puritans* [Wiley, 1966]) treatment of a Puritan prosecution, but then one starts to appreciate that something worth special note was going on when one of the possessed managed to trouble the exorcists by claiming that she had forgotten her name, per-



haps lost it in the wash, or by confessing without pause, "I am Behemoth. I am Dog's Dick. I am Iscaron." Why, after all, did these localized outbreaks of religious fervor become so important to the state? De Certeau suggests that the possessed of Loudun were challenging what was becoming essential to the project of nation and power building, a certain version of personal identity and subjectivity, one that insists on a single, singular, and publicly known definition of self.

But he will not take us any farther than this tantalizing suggestion. It was de Certeau's choice to hover around the openings to the new lines of empirical inquiry he was designating. Whenever he stepped through to begin describing social life in an easily recognizable form, he would quickly step back to scout the boundaries between the conventionally visible and that which must persist secretly.

For social research, the most immediate payoff of his work is its beginning of a social phenomenology of everyday life. Here he leaves touches of substantive contribution, for example, in the appreciation of the archipelago pattern in the informal mapmaking efforts of city dwellers and in noting the synecdochic character of perception as one passes through a city's streets. Raised to his broader level of social theorizing, de Certeau might reflect on why it is so hard to keep in mind that such cartoonlike maps are not just funny *New Yorker* covers but the real thing and that the real joke is the assumption that official maps are not fictions.

In cultural sociology, de Certeau would shift attentions to the tactical ways that people move between what are formally designated and usually studied as different domains (the marketplace, elections, work settings, sports arenas, family life). In their interinstitutional, intertextual movements, people constantly create a kind of silent social world, or, more accurately, a social world that is well known and indirectly addressed in folk terms but that remains beyond conventional attentions, including those of sociological research. With his emphasis on people's everyday tactics, de Certeau cuts against the overly abstract and passive character of much of his writing and points us clearly toward a rich research path.

*Max Weber and Democratic Politics.* By Peter Breiner. Ithaca, N.Y.: Cornell University Press, 1996. Pp. xvi+237. \$39.95.

Robert J. Antonio  
*University of Kansas*

Post-World War II readings of Weber identified him with liberal democracy. In 1959, however, Wolfgang J. Mommsen began an intense debate over Weber and democracy with his controversial treatise about Weber's nationalism and about how his ideas of bureaucratic domination, rational-legality, and plebiscitary leadership paved the way for Carl Schmitt's protofascism. But not until the 1970s did numerous English language works probe the different sides and ambiguities of Weber's political thought.

Because few of these studies have addressed closely the issue of Weber and direct democracy, however, Peter Breiner's exploration of the topic fills a distinctive niche in the Weber discourse.

Although holding that Weber's ideas of bureaucracy, legitimacy, and leadership truncate the possibilities of modern democracy, Breiner also contends that he offered vital resources for vindicating the ideal of direct democracy. In his view, Weber provided essential tools for "*sociologically informed* political theorizing and political practice" (p. 78; emphasis mine), contributing substantially to a broad stream of thought stressing "political prudence" or "political knowledge as situation-bound advice on the means, ends, and logics of action" faced regularly by political actors (p. 2). Emphasizing the "consequentialist" strand of this tradition (e.g., Thucydides, Machiavelli, and Gramsci), Breiner argues that Weber recognized the "paradoxical" relations between political aspirations and practices. Because political actions transform the very context in which they are first framed, unintended consequences are unavoidable, and all political prescriptions must be treated as tentative. By analyzing normative ends in their sociohistorical contexts and embracing uncertainty, Weber's practical judgment escapes the formalism of purely normative arguments, transcendental claims about unchanging truths, and totalistic theories of history.

Breiner implies that Weber's perspectivist emphasis on the formative impact of values on knowledge, ideal-type method of analyzing the potential consequences of different means-ends schemes in divergent historical contexts, and keen sensibility about everpresent, unexpected factors constitute a peak in the prudential tradition. In his view, Weber counters the tendency of elitist and radical theories of democracy to split ends from means and intrinsic from instrumental concerns. Thus, Breiner sheds entirely different light on Weber's purposive rationality than the critiques of his instrumentalism (e.g., by Marcuse, Habermas, Alexander). To illustrate Weber's prudence, Breiner elaborates his critique of marginal utility theory (i.e., stressing its ahistorical presuppositions, lack of attention to context, and underappreciation of unintended consequences). This chapter not only demonstrates Weber's broader approach but explains nicely his view of neoclassical political economy and still instructive vision of an alternative social economics.

By contrast to Weber's contextual, multicausal, contingent critique of economics, Breiner argues, he invoked objectivistic, singular, irreversible processes and conditions against political positions he opposed. Breiner contends that Weber's claims about bureaucratic rationalization and passive citizenry constrict democratic possibilities to mass plebiscitary politics and lead directly to the conclusion that participatory democracy is an irresponsible pipe dream. However, Breiner also employs Weber's prudence to criticize Rousseauian radical democrats, implying that they too need to accept the inherent uncertainty of political action and responsibility for its unintended as well as intended consequences. Breiner believes that participatory institutions are feasible if they are seen as a radi-

calizing addition rather than as a substitute for administration and representative politics. But to fit instrumental means to political ends and avert idealistic flights from reality, even this more modest vision of radical democracy must come to terms with the paradoxical split between aspirations and consequences.

Two critical points are in order. First, Breiner draws heavily from Weber's methodology, ideal-types, and ethics of politics. Yet, Weber details the tensions, countervailing forces, and unintended consequences that create free spaces, openings, and possibilities for substantive democratization in his historical discussions. Although concurring with the general thrust of Breiner's critique, I believe that his mode of argument exaggerates somewhat the coherence and narrowness of Weber's views about democracy. Second, his reading of Weber and view of radical democracy have affinities for John Dewey's instrumentalism, experimentalism, and uncertainty and the idea of active publics enlivening representative politics. Dewey took seriously the doubts of pessimistic liberals, like Walter Lippmann and Weber, about the masses' capability and willingness to bear the weight of political responsibility. Their questions are no less pressing today. Although Dewey provided no final answers, he stressed the enormity of the cultural project entailed in radical democratic hopes about creating participatory institutions within mass democracy. Breiner's argument would have been stronger if he addressed this non-Rousseauian branch of radical democratic theory and other side of prudence.

This short review understates the complexity and subtlety of Breiner's dense, nuanced argument. His book is a serious contribution that should be read by Weber scholars and others concerned with democratic theory. Moreover, Breiner's position will be of interest to those troubled by the abstract tendencies of contemporary theory. His vision of Weber's sociologically informed political prudence offers an alternative method to quasi-transcendental and deontological theories (e.g., Habermas and Rawls) that privilege normative over instrumental matters and to modernist and postmodernist approaches that substitute textual criticism and exegesis for historical engagement. This theme alone makes Breiner's work worth reading.

*Muslim Politics.* By Dale F. Eickelman and James Piscatori. Princeton, N.J.: Princeton University Press, 1996. Pp. xi+240. \$39.50 (cloth); \$13.95 (paper).

Lisa Wedeen  
*Wesleyan University*

This book challenges the still-current notion that political movements inspired by contemporary experiences of Islam are monolithic and immutable. The authors apply the term "Muslim politics" to a variety of regional and transnational contexts within which contests over political institu-

tions and over systems of signification take place. These movements are specifically Muslim because they "relate to a widely shared, although not doctrinally defined, tradition of ideas and practice," which Muslims in "different contexts identify as 'Islamic' to support their claims and counterclaims" (p. 4). The authors explain the appeal of "Muslim politics" to diverse classes, and especially among educated young people, as the result of widespread discontent and uncertainty following economic restructuring.

One of the book's helpful interventions is in the chapter on "Muslim Politics: A Changing Political Geography," in which the authors use the insights of anthropologist Arjun Appadurai to rethink the effects of technological innovations and global transformations on attachments to and identifications with territory. Ethnicity and religious activism have become, to use Appadurai's word, "deterritorialized," so that Muslim activists from different nation-states might identify as much, if not more, with each other than with fellow citizens (p. 136). But as the authors also point out, "deterritorialization has its limits" (p. 153). Nonstate Islamic organizations may help to advertise issues to a transnational Muslim community, but the increasing permeability of national frontiers in the age of globalized communications and migrations does not seem, in most cases, to undermine the primacy of the nation-state. Many Islamic activists use transnational linkages to challenge the limits of state authority, but they tend to imagine remaking their world within the confines of existing territorial arrangements.

The book also makes accessible the important conclusions of prominent theorists of Islamic law and history who argue against persistent popular readings of Islamic religion and politics as fundamentally inseparable. Instead of conflating Islamic doctrine with actual practices, chapter 3, "Sacred Authority in Contemporary Muslim Societies," makes a historical argument, maintaining that divisions between religious and secular authority were already evident not long after the Prophet Muhammad's death. The authors' sensitivity to the context-dependent relationship between political and religious concerns will also help a general audience grasp the ways in which political groups might interpret, appropriate, and manipulate religious language and symbolism.

The authors make a valuable attempt to show that politics is not merely about material interests but also about contests over the symbolic world, over the signification of signs and symbols and the representation of events, conditions, and people. The book thus counters the tendency of conventional studies in political science to view politics as basically a matter of material interests and the groups articulating them. For students of symbolic politics, however, such observations, although always welcome, are unlikely to be perceived as startling or new. And for many political scientists, the absence of a thorough, systematic analysis of how symbols operate, how they mediate, structure, define, sustain, and continually reassert political obedience, for example, probably leaves the authors' assertions of symbolism's centrality to political life unconvincing.

The book provides a wealth of information and is ambitious in its scope, in the number of examples selected for study, in the eclectic review of literature across academic disciplines, and in the promises of conceptual clarity it makes. The book's efforts to question rigid dichotomies, such as between public and private, are successful if not novel, but its goal of clarifying concepts such as tradition, authority, resistance, and ethnicity remains unrealized. The discussion of ethnicity, for instance, may work as a helpful corrective to formulations of identity that posit ethnicities as fixed, natural, and essential, but such descriptions are in no way specific to Islam, nor is it clear from the arguments presented here how constructions of ethnicity and of Islam interact to produce specific constellations of political power. Despite these shortcomings and a sometimes convoluted style, the book is a positive addition to a growing body of scholarship seeking to interrogate popular assumptions about Muslims.

*Comparing Policy Networks: Labor Politics in the U.S., Germany, and Japan.* By David Knoke, Franz Urban Pappi, Jeffrey Broadbent, and Yutaka Tsujinaka. New York: Cambridge University Press, 1996. Pp. xiv+288. \$54.95 (cloth); \$17.95 (paper).

Roger V. Gould  
*University of Chicago*

Inasmuch as a chief strength of network imagery is its abstraction, it would have been natural to expect a steady flow of serious comparative studies employing network methodology as their principal tool. Oddly, many practitioners, having noticed how amenable most data are to translation into network terms, have instead devoted their energies to representing idiosyncratic, not to say trivial, social systems as networks: Ham radio clubs, association football matches, and communities of macaques are about as likely to attract the network analyst's attention as are labor markets, political patronage systems, and international trade flows.

Fortunately, the authors of *Comparing Policy Networks* have taken it upon themselves to demonstrate the power of abstract structural imagery in studying a topic that is both significant for social science and conducive to comparative research: national-level arenas for labor policy decision making in advanced capitalist societies. The book's premise is simple: Given the many differences among nation-states in the types of political actors that matter, the rules they must follow, and the way they choose to operate, a coherent analytical framework capable of encompassing a variety of national "policy domains" is indispensable for making comparisons. Knoke et al.'s main aim in this book is to show that what they term "the organizational state perspective," an approach that centers on network representations of interest overlap, communication, and coordination among highly visible public and private organizations, is a strong candidate for cross-national comparative research in political economy.

The perspective, originally presented in an earlier study of three policy domains in the United States (Edward O. Laumann and David Knoke, *The Organizational State* [University of Wisconsin Press, 1987]), relies principally on multidimensional scaling of information and political support relations among organizational actors. The goal is to render the chaos of lobbying, logrolling, and arm-twisting among policy actors comprehensible by mapping thousands of organizational contacts into two- and three-dimensional space.

In a series of detailed and data-laden chapters, the authors demonstrate that it is indeed possible to portray three national polities in commensurable terms—that is, in terms abstract enough to accommodate all three cases, but not so broad that differences disappear. Overall, the authors find that their data and analyses confirm the conventional wisdom that Japan and Germany are more corporatist than the United States, in the sense that, in the first two nations, peak labor organizations seem to communicate with employer organizations through the mediation of public authorities, whereas in the United States, communication on labor issues is more polarized and contentious.

The authors draw many other, more fine-grained conclusions from their data, but there is a recurring problem in deciding what they mean. The problem is that existing views of state-society relations are imprecise to begin with and in any case have not been framed in the terms used in this study. Knoke et al. do some of the work of translating conventional wisdom into “organizational state” terms, but they do this loosely. For example, the general perception in the literature that Japan’s polity is more dependent than that of the United States or Germany on informal communication motivates the authors’ prediction that communication networks there will be both denser *and* more centralized than in the other two countries. Yet one could easily imagine arguing that a polity more dependent on informal, behind-the-scenes contact would exhibit less centralized networks (indeed, at the extremes, high centralization and high density are logically incompatible). This slack between theoretical propositions and their operational counterparts makes it difficult to know when to be surprised by the findings and when not to; and, given that there are some surprises, whether to view them as evidence against conventional wisdom, or evidence that the data collection and analysis procedures the authors employ need refinement. This difficulty is endemic to research areas in which alternative methodologies compete; to the degree that one’s confidence in a method is measured in terms of established knowledge, one is unlikely to accept divergent findings as new knowledge.

The book’s last two empirical chapters, the first of which tests a model of policy-making inspired by Coleman’s rational-actor framework, and the second of which applies blockmodel analysis to the three information-exchange networks, are specifically intended to move past the issue of descriptive validity to questions about how policy issues are decided, and—even more generally—how “the underlying power structures of the three labor policy domains” (p. 201) differ. There is some evidence that

taking account of communication patterns helps to predict policy outcomes, but the improvement in fit over a model that merely counts actor preferences is slight enough that even the authors are wary about making strong claims. Similarly, the blockmodels of communication patterns are as sensitive to noise as they are to meaningful patterns, making the reduced-form images they generate very difficult to interpret. The findings that inspire confidence are thus, again, those about which there will be little debate. Social scientists interested in the state may be disappointed after wading through this chapter to read that the central conclusion is "public actors [i.e., state authorities] play a very important role in all three domains" (p. 208).

Cross-national studies on this scale, using a unified research instrument carefully designed to take account of cultural and linguistic differences among respondents, are difficult and correspondingly rare. For this reason alone, *Comparing Policy Networks* is a noteworthy piece of work. Further exploration of the data, using conceptual language of the sort the authors advocate, will very likely make a major contribution to the political economy literature.

*Suburbs under Siege: Race, Space and Audacious Judges.* By Charles M. Haar. Princeton, N.J.: Princeton University Press, 1996. Pp. xiv+266.

*Our Town: Race, Housing, and the Soul of Suburbia.* By David L. Kirp, John P. Dwyer, and Larry A. Rosenthal. New Brunswick, N.J.: Rutgers University Press, 1995. Pp. x+267.

John R. Logan  
*State University of New York at Albany*

Exclusionary zoning in the suburbs illustrates the determined use of public policy to influence future growth, a key element of nonecological theories of community development. It has also proven to be a hotly contested battleground. Arrayed on one side are residents and officials of a select set of suburbs that restrict or exclude construction of apartments or even single-family houses on the usual quarter- or third-acre plot (though they may well promote industrial parks or shopping malls, and they may also tolerate high growth rates for houses on larger lots). On the other are a motley array of builders (seeking profits from higher density projects) and fair housing organizations (promoting opportunities for minorities). Representatives of central cities and minority membership organizations play a more ambivalent role, typically condemning exclusionary practices but also aware that suburban exclusion reinforces their own political base in urban centers.

The state of New Jersey has offered a stunning window on this battle since 1971, when local advocates began a challenge against the small semirural town of Mount Laurel. Now, 20 years after the New Jersey

Supreme Court issued its first rulings in the Mount Laurel case (in 1976), two books offer a chronicle and evaluation of the process that established a legal mandate for the economic integration of suburbia. They will be of special interest to sociologists in the fields of urbanization, race relations, and law, though the substantive issues raised here transcend these specialty areas.

Each book focuses on a different aspect of the story. Charles Haar (*Suburbs under Siege*) is a legal scholar and longtime advocate for judicial intervention against exclusionary practices. He depicts the Mount Laurel saga as evidence that judicial activism can be successful. A courageous, skillful court can and should stand up against recalcitrant localities and a reluctant legislature to protect poor and minority citizens who have not mobilized effectively on their own behalf. As a result of the New Jersey court's intervention, he argues, 15,400 affordable housing units were built in the suburbs, with the prospect of at least 54,000 more due to rezoning. "Behavior has changed in suburban municipalities" (p. 131). Haar's view is based on his own experience as a special master (i.e., a specially selected mediator for individual cases), interviews with judges, lawyers, and others, and a close reading of the long series of court decisions. He carefully analyzes these decisions in terms of the strategic choices made by their authors: how to retain jurisdiction within the state courts and to defend against later legislative challenges, how to facilitate enforcement, when and to what degree to back down. The second Mount Laurel decision (1983) took unusually strong steps. It mandated the use of statewide planning standards to fix numerical targets for providing lower income housing in every municipality; it offered individual builders specific remedies that encouraged them to sue local governments; and it established a special judicial system to accelerate adjudication. Haar's interviews with the three judges designated to implement this scheme (and many of the special masters that they selected) provide a unique and detailed description of this implementation process.

*Our Town*, by Kirp, Dwyer, and Rosenthal, focuses less on the legal decisions and more on the political process within which they were forged. This account looks more closely at the local situation from which the Mount Laurel case emerged, including the tensions within the Springville Action Council that first proposed low-income housing in the town, the roles played by local Democratic and Republican politicians, and the backgrounds of the Legal Services attorneys who filed suit. It also digs more deeply into statewide politics. At this level, contradicting Haar's thesis, these authors depict the second Mount Laurel decision as overreaching the real power of the courts. The consequence of aggressive action, they argue, was an intense reaction by the legislature. The state's Fair Housing Act (1985) accepted the constitutional principle that suburbs must be responsive to regional housing needs. But it imposed new upper limits on the number of affordable units that a municipality could be required to provide, it transferred authority for implementing the law from the courts to a new state agency, and it allowed suburbs to "buy



off" up to half their fair share of affordable housing by negotiating to provide the housing in central cities instead. This was "a blunt message to the high court: get out of exclusionary zoning" (p. 137). When the law was challenged by housing advocates, it was unclear how the court would respond to this watering down of its mandates. The legislative reconfirmation of Chief Justice Wilentz himself hung in the balance. As a result, in *Mount Laurel III* (1986) "the Supreme Court capitulated" (p. 137). Compared to Haar's praise of judicial activism, Kirp, Dwyer and Rosenthal believe that ground-breaking decisions can be sustained only in unusual conditions, when powerful allies can be enlisted. In this case, the federal courts, the governor, and the legislature were opponents, and neither central city politicians nor statewide minority groups offered much support.

Whether the glass of judicial activism is half full or half empty depends, then, on one's vantage point. Haar convincingly states the moral case for the obligation of the courts to protect minority rights and identifies strategies for more effective intervention. Kirp, Dwyer, and Rosenthal offer a broader understanding of the political context of the law. On a question so central to our times, and in the context of national struggles over affirmative action, legislative redistricting, and other hot issues, both books are important additions to our thinking.

*Divided by Color: Racial Politics and Democratic Ideals.* By Donald R. Kinder and Lynn M. Sanders. Chicago: University of Chicago Press, 1996. Pp. xi+391. \$27.50.

David Harris  
*University of Michigan*

There is little doubt that black and white Americans hold dissimilar views on policy issues. What is less certain is why this disparity exists. The search for such an understanding drives *Divided by Color*. The book begins by documenting the gap in black-white public opinion. It focuses on support for six racial policies: (1) government action to ensure fair treatment in jobs, (2) government efforts to ensure school integration, (3) increased federal funding for programs that assist blacks, (4) government initiatives to improve the economic and social position of blacks, (5) hiring and promotion preferences for blacks, and (6) reserved positions for blacks at colleges and universities. Racial differences across these items are staggering. On a 10-point scale of support for racial programs, 63% of blacks but just 9% of whites fall into the three most liberal categories of the scale. This discrepancy is much larger than gender differences on gender issues or class differences on class issues.

What explains the disparity? Kinder and Sanders consider three explanations. First, they test whether self-interest accounts for racial differences. Self-interested citizens are those individuals who "pursue material

benefits and harms in politics, and [who] have in mind only themselves and their immediate families" (p. 52). The data shows little evidence that self-interest is an important factor. However, when the focus is broadened to group interest, the effects are stronger. Blacks and whites are not as concerned with how a policy will impact their own families as they are with how it will affect their racial group. Despite these findings, Kinder and Sanders remain skeptical about the role of interests because their data reveal that whites' racial sentiments are an important predictor of their perception of the threat blacks pose. As such, interests are likely a proxy for racial attitudes rather than evidence that whites are concerned about real threats and advantages.

Second, Kinder and Sanders examine the role of group animosities. In response to the decline of biological racism and simultaneous rise of racial code words, they offer racial resentment as a new measure of whites' distaste for blacks. Racial resentment, a concept that differs little from symbolic racism, "features indignation as a central emotional theme" and is "the conjunction of whites' feelings toward blacks and their support for American values" (p. 293). Racial resentment is measured by six items that are designed to subtly ascertain whites' views about blacks. The resulting composite indicator is internally consistent, stable, correlated with racial stereotypes, and distinct from biological racism. Most important, it is a strong predictor of white support for racial policies. When whites judge programs designed to help blacks, they apparently take it as an opportunity to express racial antipathies.

A third potential determinant of public opinion is principles. This argument holds that there is a black-white gap not because of self-interest, group interest, or racial resentment but rather because of differential subscription to core American values. Specifically, Kinder and Sanders evaluate the impact of beliefs about equality, economic individualism, and limited government. Of the three, only economic individualism consistently shows no effect. Beliefs in equality and limited government both significantly predict support for racial policies, though each is a less important factor than racial resentment.

Finally, Kinder and Sanders examine the role elites and the media play in framing issues. They divide their sample into panels and present each with modified versions of the original public opinion questions. This exercise shows that how an issue is framed dramatically affects whether racial resentment, self-interest, or principle is activated. As an additional test of framing, Kinder and Sanders take advantage of the fact that Willie Horton, a black convicted murderer and rapist, became an image in the 1988 presidential campaigns while they were collecting their data. This natural experiment confirms the importance of frames. Whites interviewed before the Horton ads appeared on television were less sensitive to racial resentment than were whites queried later. The lesson of these experiments is that elites and the press have power to affect public opinion and, indirectly, policy by adopting one frame over another.

Kinder and Sanders present a thorough, thoughtful, and compelling

explanation of why black and white Americans view the political landscape so differently. While there is no doubt that this book should be read by all who attempt to understand the role of race in our society, it is not without problems. First, the discussion has a tendency to overinterpret the effects of racial resentment and downplay the importance of other factors. While racial resentment clearly plays the largest role in determining public opinion, belief in limited government and equality regularly exhibit effects that are at least half as large as those for racial resentment. Second, it is not clear that the racial resentment scale measures racial resentment. An alternative interpretation is that it indicates whether people are racial structuralists (i.e., they believe that blacks face greater obstacles to success). Unlike racial resentment, racial structuralism is a theoretically valid explanation for white *and* black public opinion. Also, because it is composed of prejudices and information, one's belief in racial structuralism is susceptible to interventions. Whether racial structuralism or racial resentment better explains the black-white gap in public opinion should be the focus of future research and discussion, as the two frames have differing implications for race relations and policy.

*The French Melting Pot: Immigration, Citizenship, and National Identity.* By Gérard Noiriel. Translated by Geoffroy de Laforcade. Minneapolis: University of Minnesota Press, 1996. Pp. xxix+325. \$22.95 (paper).

Jeremy Hein

*University of Wisconsin—Eau Claire*

In *The French Melting Pot*, Gérard Noiriel does for immigrants in France what Eugene Weber did for French regionalism: establish the existence and longevity of diversity in a country portrayed as the paradigm of the assimilationist nation-state. The original publication of *The French Melting Pot* in 1988 was therefore an event for France akin to the publication of *Beyond the Melting Pot* (N. Glazer and D. Moynihan [MIT Press, 1963]) for the United States. Glazer and Moynihan announced that the United States was not an amalgamation of cultures but a segmented hierarchy composed of competing religious, ethnic, and racial groups. Noiriel does not go this far in his characterization of France. But in the French context, his thesis is equally profound and, for some, almost heretical. Noiriel argues that France is not a monolithic national culture that absorbs newcomers but an evolving blend of peoples.

Citing the well-publicized statistic that about one in five inhabitants of France has an immigrant parent or grandparent, Noiriel asks why contemporary foreigners (who account for only about 7% of the population) are thought to be such a novel experience for the country. This anomaly serves as the springboard for an even deeper question: how could a nation with 100 years of immigration history have a national identity myth devoid of immigrants? The main culprit is the social sci-

ences. By failing to study immigrants, French historians have perpetuated a collective amnesia about the role of immigration in the formation of modern France. The late Fernand Braudel comes in for particular criticism. Braudel's magnum opus, *The Identity of France* (HarperCollins, 1990), portrays French national identity as essentially complete by the late 1600s. This *longue durée* approach to history marginalizes the more than 2 million immigrants who settled in France between the 1880s and 1930s.

French sociologists also share some blame. Durkheim regarded immigration as too plebeian a topic for true scientific inquiry. Nonetheless, Noiriel credits French sociology as the first social science to seriously research immigration, but that did not occur until the 1960s. Once begun, competitive research centers balkanized the study of immigrants, diminishing the impact of findings on other social scientists and public opinion.

After explaining the absence of immigrants from French collective memory, Noiriel spends several chapters reinterpreting the French experience with immigration largely through a rhetorical comparison with the United States. Focusing on the Belgians, Italians, Poles, and Spaniards who arrived during the early 1900s, Noiriel shows that these immigrants contributed to French culture, politics, demographic growth, and agricultural and industrialization productivity. He does not dispute that the descendants of these immigrants became French, but he persuasively insists that the French nation-state never assimilates immigrants; *au contraire*, it is the immigrants who assimilate the ways of France.

American social scientists will find Noiriel's comparative allusions of interest if for no other reason than that he turns the tables. Rather than the United States being the model melting pot, he suggests that France has much to teach the United States. This shift in perspective is refreshing, but the comparison is more heuristic than analytic. Noiriel draws a general distinction between countries populated by immigration and those where immigration occurred after the development of a centralized state and industrialization. But he goes on to suggest that the cultural integration of European immigrants proceeded in similar ways in France and the United States. References to the relevant American literature, however, are too superficial to substantiate this argument (e.g., there are no citations of articles in the *Journal of American Ethnic History*).

While the U.S.-France comparison remains conjectural, *The French Melting Pot* succeeds brilliantly in putting contemporary French discord over immigrants in a historical context: immigrants, their contributions, their social problems, and nativist reactions have a history of more than 100 years. The current national identity "crisis" is due to amnesia of this past and a national myth that excludes immigrants. Lest American readers think Noiriel has had the last word in the debate, it is important to note other perspectives. Some cite the collapse of national institutions that once integrated newcomers as the source of the crisis. Others (including Braudel) argue that the institutionalization of Islam via North African immigrants and funding from the Middle East has in fact introduced

a new pluralism to France (civilization in Braudel's terms). This debate is far from over, and those in it will have to question long-held assumptions about the French nation-state as a result of Noiriel's book.

*The Racialisation of British Policing.* By Simon Holdaway. New York: St. Martin's Press, 1996. Pp. ix+226. \$49.95.

Vernetta D. Young  
*Howard University*

There have been numerous studies on the relations between the police and minorities, namely African-Americans and Hispanics, in the United States. These studies have examined public opinion about the police, police behavior and discrimination, citizen complaints against the police, and employment practices. This book covers each of these issues. Holdaway, an ex-police officer, sets out to provide an "introduction to the sociological study of relations between police and minority ethnic groups" (p. vii) in Great Britain. Although Afro-Caribbeans and Asians are identified as the primary minority groups, information is presented on Chinese, Vietnamese, Chileans, and Jewish people. Still, most of the discussion centers on Afro-Caribbeans.

Holdaway, in the chapter "Thinking about 'Race,'" argues that it is more appropriate to talk about racialized relations than race relations. For Holdaway, the meaning of race is negotiated as a result of human actions. Structural variables, such as immigration, occupation, income, and gender, as well as social context determine the salience of race. He argues that racialized relations in England can only be explained by looking at the history of immigration and the exclusionary practices used to maintain inequality. These factors influence how minorities experience crime and victimization and set the stage for the discussion of the racialization of policing. Holdaway identifies a number of key areas of policing or "sites of racialisation": victimization, racial attacks and harassment, the race and crime debate, riots, race relations within the police, and reform of the police.

In chapter 2, Holdaway provides a historical context for racialized relations between the police and minority groups in Britain. He reports that during the 1940s and 1950s Caribbean and Asian migrants, without work skills or capital, moved into urban areas that already exhibited high crime rates. Faced with inequality in employment and housing and with social exclusion, they were forced to remain in these high crime areas.

Holdaway reports that Afro-Caribbeans and Asians perceive crime as a significant problem and have higher rates of victimization than whites. The police, however, provide inadequate attention to the concerns of minority groups largely due to stereotypical characterizations of these groups and the areas in which they live. He argues that notions of "race" are ingrained in police policies, practices, ideas, beliefs, and actions. Hol-

daway seems to be offering an apology for the failure of the police to understand and address the complex nature of these relations. He further notes that for minorities these experiences are compounded because they are not permitted to escape the situation due to structural and social constraints.

The occurrence of racial attacks and harassment is used to further emphasize the consequences of the exclusion and marginalization of minority ethnic groups—the racialization. According to Holdaway, these acts pose an immediate physical and mental harm, but just as important, they pose a “challenge to the right of ethnic minorities to live in Britain” (p. 46). Many incidents are not reported because citizens feel they will not be taken seriously and that the police will not protect them. The resulting fear, insecurity, and intimidation becomes part of everyday life and adds to the burden of racialized discrimination. Again, not only are minority group members defined by their race but these definitions have consequences for their safety and well-being and influence the responses of both offenders and the police, who are sworn to protect.

The remaining chapters illustrate the importance of understanding the contextual meaning of race. Holdaway looks closely at the rank-and-file occupational culture of the police. He reports that race is “intertwined with other features” (p. 76) of the social world of the rank and file. What may be considered routine police stops by other groups may be viewed differently by black and Asian people. Holdaway suggests that in these instances the police are influenced by factors like area of residence and housing type, which “seem to mediate the impact of racial factors” (p. 88).

Holdaway concludes the book by discussing the measures taken by police organizations to improve the quality of service provided to minority groups. He acknowledges a number of different approaches. He also notes that minority police officers have entered the profession and have been confronted with these same problems. Their efforts to organize and deal with these issues are encouraged.

More important, Holdaway emphasizes the need to recognize how social criteria are used to racialize relations. To improve the delivery of services to minorities, these notions, the social processes that led to these ideas about crime and its relationship to minorities, and the organizational and cultural factors that lead to racialized outcomes must be understood (p. 104). This book is appropriate as supplemental reading in an upper level course in issues related to policing or by those interested in cross-cultural comparative policing.

*Policing under Fire: Ethnic Conflict and Police-Community Relations in Northern Ireland.* By Ronald Weitzer. Albany: State University of New York Press, 1996. Pp. xiv+350. \$19.95 (paper).

Denis O'Hearn  
*Queens University, Belfast*

This volume addresses two audiences: sociologists of policing and students of Ireland. In the first pursuit, Weitzer provides a descriptive model of "divided society policing": systematic bias in favor of the dominant social group, politicized policing that strongly identifies with the regime, dominant-group monopoly of top police positions, dual policing for counterinsurgency and "ordinary law enforcement," special powers against the subordinate population, absence of effective police accountability, and polarized community-police relations. He proposes that community-police relations in divided societies are shaped at the neighborhood level by police effectiveness at fighting ordinary crime, the intensity of counterinsurgency policing, the legitimacy of the state in the neighborhood, and national-level controversies over policing that spill over into local political discourse.

Finally, in his implicit dynamic model, divided societies with "divided society policing" evolve into "modern" societies with "liberal" policing. Policymakers in "modern" societies such as Britain are more genuine about reform than those in "traditional" societies. Although Weitzer (mercifully) avoids describing Irish divisions as "tribal," this implies a battle between progressive British reforms and traditional Irish backwardness.

Weitzer tests his models with a thorough case study of policing since the northern Irish state was created in 1922. He distinguishes two periods: overt sectarian Protestant rule before 1968 and "reform" thereafter. His comprehensive use of varied data sources gives his analysis more credibility than some previous studies of policing in northeastern Ireland that uncritically used social attitude surveys or interviews of policemen themselves. Weitzer is knowledgeable about his case. His desire to present a balanced analysis leaves him unwilling to whitewash police harassment, violence, bias against Catholics, and so forth. He also has interesting insights into the contradictory nature of police relations with Protestants.

Despite this, Weitzer's functionalism and trust in British liberalism limit his analysis. At the outset (pp. 3-4), he contrasts functionalist theories, which assume that policing provides protection and order, with conflict theories, which view police as repressive state organs that protect dominant groups from subordinate groups seeking change. While conflict approaches may explain communally divided societies, he argues, functionalist approaches suit "liberal democracies," where policing represents broad community interests. Thus, while the pre-1970s sectarian Protestant statelet was biased against the subordinate Catholics, Weitzer implies, post-1960s policing is reformist in *intent* because it is administered through direct rule by modern "pluralist" Britain.

This leaves Weitzer needing to explain why the Royal Ulster Constabulary (RUC), despite its goodwilled reformist intentions, is still unacceptable to Catholics. One answer is that special wartime conditions require counterinsurgency policing. Although the British and the RUC are sincerely trying to reform policing, they cannot succeed as long as the RUC are responsible for counterinsurgency instead of just "ordinary policing."

This fails to consider whether Western policing is less benign than functionalist models presume. Ethnic or class biases may affect policing in Western as well as peripheral societies. Moreover, even if states such as Britain were internally pluralist, they might condone or even encourage less progressive policing in their colonies, hinterlands, or ghettos.

Weitzer admits that the nature of policing cannot be divorced from politics. Ethnically divided political regimes limit the degree to which polarized community-police relations can improve. But this conflicts with his central thesis, that liberalization of counterinsurgency policing could make policing more acceptable to the subordinate communities.

Thus, by focusing on counterinsurgency reforms, he fails to address adequately whether the police (and the state in general) must be *transformed* rather than reformed. He does not even discuss proposals to replace the RUC with community-oriented police services, although he is rightly dismissive of conventional "community policing." Instead, he concentrates on making the RUC more acceptable by liberalizing its counterinsurgency methods in favor of more "ordinary policing."

To his credit, Weitzer accepts that policing is a political problem and that, ultimately, police acceptability demands political solutions. But his interim solution underestimates the degree to which "ordinary" policing inevitably has counterinsurgency aspects in states like Northern Ireland.

One wonders whether this book would have been more agnostic of RUC and British state intentions in Ireland had it been written after the events of July 1996 when the British state apparently capitulated to a threat of wholesale police insubordination if RUC officers were forced to confront their violent Protestant neighbors and friends who wanted to march through Catholic neighborhoods. The police subsequently attacked peaceful Catholic protesters with relish and, under the direction of their British chief constable, held whole Catholic neighborhoods under house arrest for days at a time. Even mainstream Catholics and normally prounionist southern Irish politicians reluctantly concluded that little had changed in the basic sectarian nature of policing and the northern state since the 1960s, despite nearly 30 years of putative reforms. A basic premise of Weitzer's book, that the RUC *can* reform itself and achieve broader community support short of a political solution, may have been overtaken by events.



*Bad Business: Professional Crime in Modern Britain.* By Dick Hobbs. Oxford: Oxford University Press, 1995. Pp. ix+140.

Neil Websdale  
Northern Arizona University

As a working class Brit now working in the United States, I thoroughly enjoyed *Bad Business: Professional Crime in Modern Britain* by Dick Hobbs. Hobbs's well-written book takes readers on a rich ethnographic tour of the lives of professional criminals. He engages professional criminals on their own terms and in their own language in pubs, clubs, offices, and their homes. Like other books in this genre, such as Wright and Decker's *Burglars on the Job: Street Life and Residential Breakins* (Northeastern University Press, 1994), Hobbs presents professional crime through the eyes of those who commit it, as they commit it.

Hobbs argues that it is impossible to distinguish neatly between professional crime and everyday business activities in the marketplace. Professional crime is subject to the same market forces as legitimate business. Consequently we see "bad business" changing with structural alterations in the marketplace, such as the deregulation of the London Stock Exchange in 1986 and the rise of increasingly sophisticated electronic communications. Professional crime may be "bad business" but it is still nonetheless "business." Hobbs's central argument then is that "bad business" is best appreciated against the historical ebb and flow of rational economic forces. He does a fine job of contextualizing the historical drift of professional criminals from cracking safes and robbing banks toward myriad economic activities, some legitimate, some illegal, but all designed to accumulate wealth.

The voices of professional criminals assume center stage in Hobbs's ethnography. The narrative is sprinkled with the linguistic skills of working-class criminals. Banks are "tasty" (i.e., appealing to rob), inmates "flob" (spit) on prison guards, some people are "wankers" (i.e., literally, "masturbators" but colloquially "idiots"), commodities "fall off of lorries [trucks]" (i.e., they are stolen, or perhaps more appropriately, redistributed), men talk of "birds" (women), and at times talk a load of "bollocks" (i.e., literally, "testicles," colloquially "bullshit") and all are pursued by "Old Bill" (police). Long interview excerpts weave a rich web of firsthand experiences that alert readers to the motives, justifications, and linguistic codes of "bad business" people.

We learn of the master craftsman, Dick Pooley, whose safecracking exploits depend upon an intimate knowledge of explosives "in the way that any craftsman would with the tools of his craft" (p. 15). Dick, and his like, are now virtually extinct along with other "icons of proletarian identity" (p. 18) such as the shipyards, coal mines, and steelworks. In his account of a father and son team, Danny and Chris, we see the struggle between generations of men and the way in which generational tensions around the development of masculinity are mediated by the need to make

money and to engage in professional crime. In the tale of Danny and Chris, as in other tales, Hobbs echoes the work of Messerschmidt's *Masculinities and Crime* (Rowman and Littlefield, 1993) on the way in which crime constitutes a vehicle for performing gender. We meet Fat Laurie, whose bowel movement is carefully timed to coincide with the opening of a public house called "The Dog." At The Dog "pine disinfectant battles with stale tobacco smoke and the phantoms of last night's beer," as Fat Laurie takes a "shit and a Scotch" (p. 52). As "men of violence," Gary and Eric are much more than the dupes of Thatcher's right-wing ideology. Rather, Hobbs elucidates, they are young people with limited occupational horizons but considerable initiative who drift from dishing out violence to dealing amphetamines.

Through all these excerpts, and many more, we appreciate the agency of professional criminals amid the shifting structural conditions of modern Britain. Changing technologies limit the ease with which banks can be defrauded, safes are safer, the police have greater powers of surveillance, and skilled meaningful labor is increasingly hard to come by. These historical changes mean that a cohesive community of professional criminals that endures across generations is more myth than reality. Citing Zygmunt Bauman (*Intimations of Postmodernity* [Routledge, 1992]), Hobbs points out the professional criminal community is a "retrospective unity" rather than a transhistorical reality. This "unity" is consistent with the characters themselves who are what Hobbs calls "fantasists."

*Bad Business* will be of immense interest to sociologists and criminologists. It is readable, theoretically eclectic, and richly insightful. If I have a criticism it is that the author could have spent more time talking about the intricacies of his approach. His approach is fairly straightforward, but more methodological reflexivity might cast light on the homologies between life as a professional criminal and social life in general.

*Recriminalising Delinquency: Violent Juvenile Crime and Juvenile Justice Reform.* By Simon L. Singer. New York: Cambridge University Press, 1996. Pp. vii+230. \$54.95.

Troy L. Armstrong  
*California State University, Sacramento*

In 1978, the New York State Legislature voted, by overwhelming margin, to pass the juvenile offender law, lowering the eligible age of criminal responsibility to 13 years old for murder and 14 years old for other violent offenses. Enactment of this statute shifted the initial locus of legal decision making about juvenile offenders who have been charged with specific violent felonies from juvenile justice officials to the criminal court and culminated a lengthy period of debate over the wisdom of introducing legal procedures to allow waiver, the prosecution in criminal court of designated juveniles under the legally recognized age of majority. At

the same time, New York had long resisted efforts to establish legal procedures for waiver; it was one of the few states that had earlier set the low general age of 16 years old for criminal responsibility. In this historical and legal context, a stringent legislative waiver statute was enacted.

The juvenile offender legislation was widely viewed by the public as being motivated largely by political considerations that promoted a strong stance against a perceived emerging epidemic of violent youth crime. This law appeared to be promulgated as a direct result of several heinous and highly publicized murders perpetrated in a callous, predatory fashion by a 15-year-old chronic juvenile offender, who, due to his age, could not be transferred to adult court. This relatively convenient explanation leaves much to be desired, given what is currently known about the complexity and range of factors identified as playing some role in juvenile justice reform. Using this as an impetus for inquiry, Simon I. Singer, who has previously written extensively on the topic of waiver, constructs a far more expansive and challenging argument examining a wide-ranging set of circumstances and a variety of multidimensional factors postulated as contributing in varying degrees to the creation, passage, and impact of the 1978 juvenile offender law.

Written as a case study in juvenile justice reform, *Recriminalizing Delinquency* provides a detailed and insightful account of a controversial legislative enactment, its background, content, intent, and impact. Certainly, the publication of this volume is very timely. The perception held by the public and elected officials about the escalating crisis in youth violence has resulted in steps being taken in most states across the United States to launch major initiatives to revise juvenile codes and to broaden correctional statutes to shift policy and law concerning serious juvenile offenders into a far tougher stance. At the heart of these changes have been endeavors to enact new, harsher waiver statutes. This volume addresses one example of such an effort and has made a major contribution to the emerging literature on waiver.

Singer structures his argument and account of the New York events by organizing descriptive materials and analyses into a sequence of three sections focusing upon the origins, implementation, and effects of the juvenile offender law. Discussion of origins explores antecedent activities and evolving perceptions/attitudes that contributed to recriminalizing juvenile offenders, while examination of implementation focuses largely upon the relative importance of those identified principles that appear to have significance for assigning criminal responsibility. In addressing impact, Singer attempts to determine whether any general deterrent effect has been achieved by the juvenile offender law. Both the implementation and impact sections benefit from the inclusion of very thorough multivariate analyses, which provide valuable insight into the respective influence of a substantial array of potentially relevant variables. In addition, a final substantive chapter describes the attitudes and perceptions about the incarceration of youth who had been sentenced as adults and committed to a secure facility under the law.

To demonstrate that understanding of the dynamics of juvenile justice reform must be grounded in a complexly configured framework, Singer proposes a strategy for explanation that incorporates consideration of a wide set of organizational constructs and principles. At the heart of his argument is a series of assertions and assumptions—derived largely from organizational theory—about the inherent structural and processual nature of this particular bureaucratic arena. From Singer's perspective, key to a proper assessment and analysis of planned change in juvenile justice is the fact of its being a loosely coupled system in which there is both a tendency for confusion in decision making across subsystem boundaries and little coordination among component parts. Within this framework, he repeatedly asserts that recriminalization (e.g., introduction of legislative waiver) is one optional direction in which a juvenile justice system can be moved as it creates another legal subsystem for responding to deviant behavior. Singer argues that the kind of toughened measures that he examines in the book do little to demolish the existing juvenile justice system but rather serve to preserve a rationale and need for its continued existence.

My only two reservations with Singer's analyses and conclusions focus upon the deliberate uncertainty that he has introduced into his central argument and the level of interactive complexity he has deemed necessary in his discussion of contributing factors. With regard to the former, by admitting that he does not possess any direct measure of tightly and loosely coupled systems of justice, Singer generates a sense of uncertainty in his analysis that results in an inconclusiveness in his findings. With regard to the latter, the set of organizational concerns and interests identified as being potentially relevant to the sudden emergence of waiver legislation in New York State, as well as the interactive complexity of these factors, left this reader slightly bewildered. Yet, through this ambitious design, Singer has clearly set an agenda for subsequent research that should provide an opportunity for new and important insights to be obtained about the juvenile justice reform process.

*Contested Meanings: The Construction of Alcohol Problems.* By Joseph R. Gusfield. Madison: University of Wisconsin Press, 1996. Pp. ix+374. \$56.50 (cloth); \$19.95 (paper).

Joel Best

*Southern Illinois University at Carbondale*

Like athletes, sociologists can become known for characteristic moves, favorite analytic turns. Joseph Gusfield, for instance, likes to dissect taken-for-granted assumptions, taking what everyone seems to accept without question, and exposing it as a construction of unexamined assertions, favored for its convenience to established interests. This analytic turn can be found throughout *Contested Meanings*; the very title warns

us that knowledge is debatable. This volume collects 13 Gusfield essays about alcohol problems; although eight of the papers, dating from 1967 to 1991, are reprinted, most first appeared in specialized collections and will be new to many readers. Throughout, Gusfield seems to delight in challenging the obvious, in questioning what everyone knows about alcohol problems and policies.

Gusfield argues that, since the 1930s, alcohol problems have been defined by—owned by—“the alcoholism movement,” the coalition of academics, treatment professionals, government agencies, and Alcoholics Anonymous that has promoted the notion of alcoholism. The movement views alcohol problems, such as drunken driving, public drunkenness, and fetal alcohol syndrome, as located in particular individuals (alcoholics or at least “problem drinkers”), therefore it promotes policies designed to identify these individuals and help them “recover” by not drinking. Thus, those arrested for driving under the influence (DUI) are ordered to attend classes on drinking designed to help them recognize that they “have a problem.” Because such policies often have little effect, a “new temperance movement” has emerged, dedicated to limiting access to alcohol, reducing alcohol consumption, and convincing categories of individuals (e.g., pregnant women) to reduce or stop drinking. While these claims are so commonplace as to seem unexceptional, Gusfield dismantles them and makes the familiar strange.

He reminds us that drinking is both a widespread practice and something of a learned skill. Most adults drink, and most drinking does not cause much harm. While it may be true that the people who drink the most are more likely to get in trouble, many people drink heavily without incident, and at least some problems occur because drinkers are inexperienced. There are plenty of individuals who drink regularly but view themselves as competent drinkers who can “handle it”; they may, for example, drink and drive, yet argue that they take extra care, know their limits, and so on. Such individuals are likely to resist the alcoholism movement’s claims that they are problem drinkers, even when they experience arrest or some other difficulty. Gusfield explores this world of ordinary drinking in three chapters reporting on ethnographic fieldwork; two of these describe drinking in bars and the drinkers’ subsequent decisions to drive, while the third examines probation hearings and classes for drivers with DUI arrests. He finds drinkers who believe they can take care of themselves, bartenders who keep a protective eye on regular customers, and offenders who resist claims that their DUI arrests reveal anything more than their bad luck in being stopped when they had high blood-alcohol counts. Where the alcoholism movement sees problem drinkers in denial, Gusfield is willing to ask how drinkers construct their own activities.

In his analysis, drinking becomes a situated activity: people drink, not because they are driven by internal compulsions, but because they enjoy it, because they value the sense of playful release. People drive after they drink for various reasons—notably because driving is often the most

practicable way to get to and from bars—and they drink in bars because the law often limits the public places where drinking can occur. Does it make sense, Gusfield asks, to define drinking problems solely in terms of people's freedom to choose to drink, while ignoring the limits on their choices for transportation or locations for drinking? Why do we treat drinking and driving as a drinking problem rather than a driving problem?

The ownership of alcohol problems is in the hands of experts and agencies who delimit our sense of what is problematic and thereby promote particular sorts of solutions. If alcohol problems are moral problems, caused by problem drinkers' irresponsibility, then experts must either convince problem drinkers not to drink or, by limiting access to alcohol, make drinking more difficult. Because such policies ignore the widespread acceptance of drinking, they find little support among ordinary drinkers. Gusfield notes the long history—and limited effectiveness—of alcohol education programs, suggesting that their enduring "is one of the clearest arguments for the persistence of magic in modern societies" (p. 285). He continually challenges the effectiveness of broad policies based on narrow definitions, arguing instead for considering of piecemeal approaches that identify and address key elements in the situations where alcohol use seems most likely to lead to trouble.

While those interested in alcohol problems will, of course, find this book provocative, the questions Gusfield raises are relevant to other social problems, too. Alcohol is not the only product associated with leisure and defined as problematic, for which reduced access is promoted as a solution. There are parallel claims, calls to restrict or ban guns, tobacco, rap music, pornography, illicit drugs, war toys, and so on. It can be argued that, like alcohol, these products sometimes lead to serious trouble, but the great majority of the time they do not, and, also like alcohol, they inspire vigorous debates between their critics and their consumer-defenders. *Contested Meanings* suggests that, in studying these debates, sociologists should be slow to buy into unexamined assumptions, that the construction of meaning should be studied, not just taken for granted.

*Alcoholics Anonymous as a Mutual-Help Movement: A Study in Eight Societies.* By Klaus Makela et al. Madison: University of Wisconsin Press, 1996. Pp. xii+310. \$46.00 (cloth); \$14.95 (paper).

Paul M. Roman  
*University of Georgia*

The 1935 beginnings of Alcoholics Anonymous (AA) involved an unexpected meeting and bonding between two upper-middle-class male WASPs in Akron, Ohio, a prototypical heartland American city. Their perceptions of releasing each other from the grips of alcohol addiction through mutual confession of shared weaknesses and shared humanity

strongly motivated their search for other alcoholics, mostly like themselves. Their efforts spiraled into an amazing organizational structure, centered around a therapy combining folk, medical, and spiritual elements.

Arising from mainstream American culture, AA has achieved symbolic dominance over America's popular imagery of routes into and out of alcoholism. How far has AA diffused beyond the narrow boundaries of its beginnings? An important response is this collaboration, framed around excitement about AA's international diffusion, and providing multifaceted descriptions of the fellowship in the United States, Finland, Sweden, Iceland, Poland, Austria, Switzerland, and Mexico. The study attempts an organizational analysis, demarcated from extensive sociological scholarship on individual AA experiences.

The 18 authors apparently concurred on a descriptive framework for presenting their findings that unfortunately discourages analytic insights. Many observations are fragmented and shallow, even trivial for those uninterested in the specifics of AA structure and process. Sidestepped are opportunities for sociological analyses of the interplay among AA process and structure and the host sociocultural environments. For example, a start at an in-depth description shows that AA in Poland is intertwined with the Roman Catholic Church, an incredible contrast with AA's traditionally strident independence from extraorganizational connections. But I was disappointed when the description trailed off with the pallid comment that the church is a dominant presence in Poland.

Without macroanalyses of structural and cultural supports, the question of the true extent of AA's diffusion remains. AA's early leaders were sensitive to the contextual limits of its WASP-ish beginnings, and through the 1960s its General Service Board worried about successful outreach of the fellowship to minorities and women. Today, AA's success beyond white middle-class males seems widely accepted, and the albeit modest documentation of this achievement is cited in this volume. For some, international diffusion data complete the paradigm that AA can work anywhere. Yet it must be recognized that the "ways of knowing" about AA's successful diffusion are extraordinarily limited: its guarantees of anonymity and ephemeral definitions of membership preclude valid organizational demography.

Despite extensive AA diffusion, these authors observe that many if not most persons who first attend an AA meeting do not return. These invisible masses for whom AA does not "click" presumably share a belief that they have a significant drinking problem. Yet we know little about them, their lack of attraction to AA, and the ultimate resolution of their troubles. Cross-cultural observations of this attrition might temper this book's opening assertion that "AA is one of the great success stories of our century" (p. 3).

A different methodological problem is determining the significance of AA in a nation's collective definitions surrounding alcohol problems. This study affirms that AA is "there" in these eight nations. But the signifi-

cance of its presence is contingent upon its place among other cultural definitions and strategies used to cope with alcohol problems. These issues are avoided almost completely.

To wit, a striking set of data demonstrates dramatic growth of AA activity in Latin America. Despite the national expertise across the writing team, little effort is made to explain this development or why growth is slower elsewhere. Some unique Mexican AA practices are described, but there is no linkage offered between the Latin American sociocultural environment and the upward trajectory of AA activity. Moreover, given the emergence of widespread disruptive drinking in nations in early stages of economic development, insights from AA's growth in Latin America could address future human misery elsewhere in the world.

Further needs for comparative analysis are highlighted by central AA beliefs that abstinence is essential for recovery, and that striving for abstinence is a condition of continued participation in AA. By contrast, especially in Western Europe, an alternative perspective holds that regaining control over drinking may be a reasonable solution for uncontrolled drinking. Abstinent "recovering alcoholics" in U.S. culture comprise a social category, have substantial opportunities for interaction with each other, and enjoy a modicum of social dignity. Such constructions of meanings and of opportunities for social integration may not transcend cultural boundaries; the meaning of AA membership in a non-U.S. culture is encapsulated within that culture's complex of definitions. The potentially huge implications of such differences are not systematically addressed here.

This book offers a plethora of detail about AA in these eight nations, but meager fare for those seeking sociological knowledge about comparative organizational systems. Future research needs are well articulated, and, in its defense, the volume may be characterized as an invitation for further inquiry. Its impact may be enhanced by a forthcoming companion collection of national case studies of AA.

*Drug War Politics: The Price of Denial.* By Eva Bertram, Morris Blachman, Kenneth Sharpe, and Peter Andreas. Berkeley and Los Angeles: University of California Press, 1996. Pp. xiv+347. \$48.00 (cloth); \$17.95 (paper).

James A. Inciardi  
*University of Delaware*

Americans have experienced a number of alternative approaches to drug control for generations. One legacy has been limited success while another has been an occasional spate of books on the topic. In fact, criticizing American drug policy seems to have become a cause célèbre for both academics and policy analysts—particularly those who have done little work in the drug field. *Drug War Politics* represents the latest entry to the literature in this regard.



The authors state that the book is not just another book about the failure of U.S. drug policy but rather the politics of the drug war—the politics of denial—and the struggle for drug policy reform. The “politics of denial” refers to the reasons why the drug war’s fatal flaws and collateral damage are for the most part ignored.

Part 1 of the book was written to expose the pattern of denial and examines some of the reasons why many Americans fail to see the ways in which the drug war serves to create and exacerbate the problems of drug abuse, addiction, and crime. Part 2 looks at the politics of denial and asks questions about the nature of these politics and why they persist. Part 3 discusses alternatives to the current drug war, such as legalizing drugs (which they agree is also a flawed approach) and implementing a public health paradigm.

On the positive side, the book is extremely well written and contains material that will certainly be of interest to many readers who have had limited exposure to the great drug debate. Of special interest in this regard is the history of the drug wars, from the early struggles during the first three decades of this century through the entrenchment of the “punitive paradigm” in midcentury, followed by the “presidential drug wars” and the emergence of the “narco-enforcement complex.”

Unfortunately, however, for those of us who have been following the drug wars over the years, there is nothing new in this book. There are at least a dozen or so other monographs and perhaps a hundred or more scholarly papers that have been published in the last three years that have said essentially the same things. All too often we have heard that the supply-reduction strategy of drug control (which includes interdiction, enforcement, asset forfeiture, and foreign assistance initiatives designed to keep drugs out of the United States and drug dealers off the streets) just does not work—which it does not. At the same time, we have been hearing that the demand reduction strategy (which includes prevention, education, and treatment) has been short-changed over the years in the federal drug war budgets—which it has.

When scholars from one or more fields step out of their areas of expertise into others, there is always the risk of making both minor and major errors. The authors of *Drug War Politics* are not from the drug field: one is a policy analyst and organizational consultant, two are professors of government and political science, the last is a specialist in foreign policy studies, and all have fallen into this trap. One of the more obvious is their belief in what I have often referred to as the “enslavement theory of addiction.” Enslavement theory holds that because of drug prohibition and because of the high price of heroin, cocaine, and other illegal substances on the drug black market, otherwise law-abiding citizens are forced into criminal careers in order to support their expensive drug habits. The theory sounds quite logical, and it has been repeated many times over the years. The problem with enslavement theory is that in most cases it is just not so. For the great majority of drug-involved offenders, drug use did not precede their careers in crime. Although drug use tends to

intensify their law-breaking behaviors, their criminal patterns were well established before their drug initiation. There are dozens of empirical studies documenting this phenomenon, and the authors neglected to mention them.

As a final point, the authors argue quite forcefully that the use of illegal drugs should be treated not as a crime but as a public health problem. This certainly makes sense because, after all, the wars on drugs have for the most part failed. However, this is certainly not a new idea. Although the "public-health paradigm" in one form or another has been called different things by different people—the medical approach, the disease model, demand reduction, and the harm reduction paradigm—it has been suggested for generations and parts of it actually have been implemented as part of the "war on drugs." In short, although this is a well-written book, it was disappointing because of its failure to offer any new insight on America's great drug war.

*Fringe and Fortune: The Role of Critics in High and Popular Art.* By Wesley Monroe Shrum, Jr. Princeton, N.J.: Princeton University Press, 1996. Pp. xvi+284. \$49.50 (cloth); \$17.95 (paper).

Judith R. Blau

*University of North Carolina at Chapel Hill*

It was Henry James's view that criticism was an aesthetic discourse, in contrast to reviewing, which he considered to be practical and journalistic (*Literary Reviews and Essays on American, English, and French Literature*, [Twayne, 1957]). Yet James would have missed the subtle clarity with which Shrum makes the distinction between critics and reviewers. Shrum's theoretical point is that critical standards that help to distance audiences from their experiences and direct pleasure are central to a sociological understanding of high art and not relevant for the enjoyment of popular culture. Very specifically, high culture is to criticism as popular culture is to reviewing. Audiences of high culture are engaged, through critical mediation, in the evaluation of art products, whereas popular culture is enjoyed directly, without intervention or an interpretive mediator.

*Fringe and Fortune* arrived on my desk the week after returning from the Avignon Theater Off Festival, which was the perfect time to read a book on the Edinburgh Fringe Festival. The genre can be described in the terms of a public festival—dense crowds, street theater, and performances in semiofficial as well as in official venues. There is a zany mixture of carnival and authenticity, sublimity and silliness, satires on sublimity and satires on silliness, political happenings, and multimedia events. The genre, as I concluded, and Shrum confirms, can be superofficially described as a crossover between P. T. Barnum, Studs Terkel, and funky Aristophanes.

Therefore, my own casual observations in Avignon, supported by much of Shrum's lively ethnographic description, led me to expect an analysis of a theater festival in terms of a conception of the public sphere or of public culture. Shrum confirms that such a conception is useful but also articulates a theoretical perspective to clarify how aesthetic values apply to some but not all of the theatrical endeavors at the Fringe Festival. His conclusions are based on a clear and insightful overview of the theory and the history of aesthetics as well as his sociological investigation, consisting of interviews—with cab drivers, theatergoers, pub customers, street people, producers, ticket vendors, and actors—as well as an appropriately scaled quantitative analysis of audience composition and of reviews.

The Fringe Festival is an especially ambitious undertaking; in 1988, the year in which Shrum did his research, there were 913 advertised programs, with about 100 to 500 performances a day, and it extended over a two-week period. He distinguishes between events that are defined by high-art standards (theater, opera, mime, dance, orchestra, recitals, poetry) and those that are broadly popular and entertaining (defined by the categories of comedy, musical, revue, cabaret, folk, jazz, rock, and events for children). He is careful to point out that his job as critic for a magazine, the *List*, gave him access to places and people that he would not otherwise have had. Being a critic also provided him with a way of being a spectator of his own professional reactions and those of other journalist critics.

Thus, the thesis and the corroborating evidence indicate that it is critical discourse that maintains the line between high and low. Critics define and maintain legitimacy and standards within the realm of high art, but in the realm of popular art, personal tastes prevail. The book includes an excellent and stunning review of relevant theories in aesthetics and the sociology of art and of culture.

Against his own thesis, he poses two alternative ones: "convergency theory" (Herbert Gans and my own work), which posits a diminishing difference between audiences and between products with regard to high and popular art, and "cultural capital theory" (Pierre Bourdieu and Paul DiMaggio), which posits that status interests help to maintain the differentiation of high and popular. Shrum provides some support for the "omnivore thesis" (Richard A. Peterson), namely that educated people have wide-ranging interests and indulge in many kinds of art activities. However, Shrum's arguments are based as much on audiences' tastes as on the importance of the tastemakers. Shrum's analysis poses further unanswered queries as well: In their mediation role, what gives critics the warrant to exercise judgment? To the extent that criticism is not grounded in clear aesthetic conventions, who has the right to be a critic? Why is an Olympic gymnastic performance not high art if it must be explained by an expert to television audiences? Is a Mozart piece popular or high art when it is rearranged for a film? Is not jazz the test case that

a genre can cross the "great divide"? Quality—what is intrinsic only to high culture—is constructed through discourse as critics repeatedly evaluate works, and it is our repeated readings of critics that inform our judgments. *Fringe and Fortune* has a charm and wit that novelist James would have liked.

*A Space on the Side of the Road: Cultural Poetics in an "Other" America.* By Kathleen Stewart. Princeton, N.J.: Princeton University Press, 1996. Pp. xii+243. \$49.50 (cloth); \$15.95 (paper).

Karen McCarthy Brown  
*Drew University*

Kathleen Stewart has written a subtle and rich ethnography of southwestern West Virginia. From everyday language—what the locals call "just talk"—Stewart conjures a dynamic, conflictual portrait of life in the "hollers" and coal camps. Here is a rendering that evokes liquid cultural patterns never allowed to solidify.

*A Space on the Side of the Road* reproduces and analyzes the words of individuals occupying a gap in space/time left when progress deserted the coal camps of West Virginia. The author suggests that telling the story of America and progress from the perspective of the Other "tweak[s] the anxieties and desires that motivate the master narratives of center and margin, self and other" (p. 6). In the United States, so-called Appalachia has surely become one such Other, an object of "both dread and desire" (p. 118). Appalachia has come to embody both old-fashioned authenticity and "a degraded state of nature" (p. 119). But this is not a book about stereotypes. It is something much more interesting—a book about the folk of Egeria, Odd, and 'Miga and the ways they configure their world. Stewart's compassionate intelligence shines through her text, as does her considerable knowledge of and ease within the hollers and coal towns where she lived and moved during two years of field research (1980–82).

The author invites her reader to "imagine the kind of place where, when something happens, people make sense of it not by constructing an explanation of what happened but by offering accounts of its impacts, traces, and signs" (p. 57). Thus a litany of violence and loss may be provoked by a simple request for directions.

All right, now, go on down Miss Banks's place past that *big ol' bridge* where that *McKinney* boy went over and it looks like you might go in *after'm*. . . . Keep on 'til you see the *Black Eagle* post office. That's where the *snake* handlers *stay* at and Bud said *he's skeered* to deliver the *mail* down there where he might put his hand in a mailbox and there's a *snake* in there. . . . *All Right*, now, *keep on, keep on*, pretty soon you come to the place where they shot up that *boy*. *What* was that boy's name? You know that one *kill* his *wife*. (pp. 56–57)

Stewart tracks the narrative alchemy whereby tales from a world "got down" (when told just right) produce new hope and new energy to endure daily trials. She notes the "nervous" scanning of the environment—nature, the neighbors, even heaven itself—to look for signs of trouble around the corner. And she names the double bind of a sociality that thrives on an excess of information about others, while at the same time placing a high value on privacy and condemning those who stick their noses into another's business.

*A Space on the Side of the Road* is also a theoretical book of considerable merit. Stewart situates her work in the center of the "new ethnography," a diverse collection of recent, experimental, ethnographies responsive to a variety of postmodernist influences. The ethnographer, she believes, cannot resolve the subject/object dilemma through current confessional practices, such as situating oneself in the narrative. Neither can the researcher gather up voices and put them in her text, pretending that they are speaking for themselves. "There is no textual solution," Stewart says with refreshing candor. The only thing to do, she suggests, is write ethnographic narratives that track the nervous signals of culture in everyday sociality.

Facing a question that pops up frequently these days among anthropologists, "What is a culture anyway?" Stewart gives no more comfort than she gave to ethnographers. But unlike some critics, Stewart does not suggest that we drop the word *culture* from our working vocabulary any more than she suggests that we stop writing ethnographies. We need not give up on the concept of culture simply because it cannot be reduced to fixed sets of values and beliefs. Culture simply is not "something that can be gotten right," not ever. According to Stewart, culture can only be evoked through "multilayered narratives of the poetic in the everyday life of things" (p. 210).

*A Place on the Side of the Road* is without a doubt one of the best examples of the new ethnography, but it is not an unproblematic work. Particularly in the first half of the book, the reader strains to accommodate the jolt that comes with frequent shifts between the rhythmic, understated, and wickedly innovative narratives of Stewart's West Virginia sources and her own densely packed analytic prose. It is one of the great ironies of our time that postmodernism, and I intend the term to refer to the broad intellectual movement with the best chance of bringing new voices into the arena of academic discourse, has adopted a mode of discourse that too often masks its democratic impulse.

*Why the Wealthy Give: The Culture of Elite Philanthropy.* By Francie Ostrower. Princeton, N.J.: Princeton University Press. 1995. Pp. xi+250. \$29.95.

Paul G. Schervish  
*Boston College*

My research shows that at least 40% of all personal charitable contributions are made by households in the top 3.5% of the income distribution. For this reason alone, Ostrower's examination of the meaning and practice of philanthropy among 88 elite New York City donors is informative for students of philanthropy, culture, and American institutions. The leading question of the book is given by the title—to excavate the culture of elite philanthropy. It is difficult to pin down an analytical definition of elite philanthropy. Ostrower's functional one is that elite philanthropy is the array of social relationships of power and prestige by which the wealthy "take philanthropy . . . and adapt it into an entire way of life that serves as a vehicle for the cultural and social life of their class" (p. 6). Philanthropy, in short, is one of the most comprehensive indicators and effective producers of the cultural status, social cohesion, and personal identity of the wealthy.

While emphasizing that the wealthy never abandon their class trait to do what they want, where they want, and in the way they want, Ostrower correctly identifies wide variation in specific personal motivations and activities the elite manifest in accomplishing their underlying purposes of power and privilege. She provides important insights about the centrality of the associational ties that elite cultural, medical, and educational institutions use to attract and reward contributors. As in all studies of the elite, we hear much about board memberships, social networks, and the strategies of boundary maintenance aimed at determining who is to be welcomed into the stratosphere of elite philanthropy. We learn that wealth holders of all political stripes concur that they should be able dispose of their wealth however they wish, that they know better than Uncle Sam about how to spend their legacies, and that philanthropic bequests are an honorable alternative to estate taxation. The wealthy agree that philanthropy should never take on the massive social welfare or basic research agendas that are properly the responsibility of government. We learn too that the elite are more likely to follow class rather than religious or ethnic patterns in giving, that they seldom get directly involved with the ultimate beneficiaries of their largesse, and that women are less likely than their male counterparts to hold positions of authority.

The wealthy are thoroughly committed to a positive view of philanthropy and cannot be shaken from it. Employing a wonderfully astute interview technique, Ostrower proposed to her respondents a series of negative criticisms of elite philanthropy, namely that philanthropy induces dependency among the needy, encourages fraud, abets social snobbery, and legitimizes an undemocratic exercise of power. The wealthy

dismissed most of these putative criticisms outright; the rest they reject after only brief consideration. The wealthy readily acknowledge their command over a disproportionate amount of resources. But this only fortifies their social mission: they are obligated to make a public difference, and they are doing so through their philanthropy.

The major shortcoming of the book is that it draws general conclusions about the wealthy population from an unrepresentative and unweighted sample of New York City donors. To her credit, Ostrower warns the reader about her narrow sample, but for some reason she fails to write the bulk of the text with that caution in mind. For example, the first sentence of the conclusion states that "this book has asked why wealthy donors engage in philanthropy" (p. 132). Because her sample does not reflect the wealthy of New York, much less of the nation, her study cannot answer this question. In another example, the book purports to chart the relative distribution of motives, roles, relationships, and functions of philanthropy by the wealthy. However, the sample allows Ostrower only to map the existence of such attributes for some elite New York donors to only 48 of the city's nonprofit organizations. It does not allow her to generalize—as her tables, multivariate analyses, and conclusions appear to do—about the philanthropy of the American wealthy. Perhaps a more accurate title for the book (one closer to earlier titles she used) would be *Why Some of the New York Wealthy Give*. To put the wealthy of New York City in perspective, there are nearly 70,000 households with at least \$1 million in adjusted gross income, and 4.5 million households with a net worth of at least \$1 million. My interviews with millionaires indicate that elite philanthropy is only part of the panorama of wealth and philanthropy engaged in by millions of people. The foundations the wealthy create, the novel programs and community projects they initiate, and the nonelite institutions and organizations they support are all evidence of something more than elite philanthropy. I do not think Ostrower means to say that all philanthropy by the wealthy is elite philanthropy, but this is one meaning of what she says. As a study of why the wealthy give in the United States, the book is flawed. As a study of philanthropy surrounding the elite institutions in New York City, this book has significant merit.

*Intellectuals and Public Life: Between Radicalism and Reform*. Edited by Leon Fink, Stephen T. Leonard, and Donald M. Reid. Ithaca, N.Y.: Cornell University Press, 1996. Pp. xii+327. \$47.50 (cloth); \$19.95 (paper).

Michael D. Kennedy  
*University of Michigan*

Originating in a seminar at the University of North Carolina at Chapel Hill, these 12 essays take the sociology of public intellectuals well beyond available collections for their geographical range and historical-theoretical

engagement. The split between intellectual as expert and intellectual as critic serves as a volume leitmotif.

Leonard begins with a genealogy of the politicized intellectual, embedding it in the old sociology of intellectuals and newer postmodern and Gramscian approaches. Lloyd Kramer follows with an account of Enlightenment intellectuals in Habermasian and Foucauldian modes, using the expert/critic divide as a foil and prop to inspire our recuperation of their recombination.

Intellectual histories are the obvious method to analyze public intellectuals. The engaging social theory and fascinating biography of Régis Debray and W. E. B. DuBois are inspiring because, in words and deed, they have been powerful critics of establishment and movements. Reid and Tom Holt magnify their original brilliance with insightful elaboration. Liberation theology's centering of the poor also inspires, but whom? Roberto Goizueta's concise account of theological innovations does not assess social consequence, and Protestant Evangelism's explosion makes a better claim to being the *consequential* theological innovation in Latin American Christian base communities. A more social history can help assess intellectuality's embeddedness and ideology's consequence.

With good timing, intellectual debate can play a vital role in transforming the state. Mary Furner describes how the debate between statist and associative economists "economized" U.S. turn-of-the-century discourse and facilitated the move away from *laissez faire*'s static individualism. Fink's exploration of the 1912–15 Commission on Industrial Relations begins with the opportune moment but turns to a careful dissection of how personal conflict and contending visions of intellectuals—as agitators of public opinion and as engineers of policy—destroyed opportunity. Ellen DuBois's story of U.S. activist women writing history and making archives, 1880–1940, suggests that we consider "effect" as both immediate and longer term. Movement theorists have long thought in these terms as well as about how opportunities for intellectual consequence are made.

Craig Calhoun shows how in 1989 protesting students drew on long traditions of Chinese intellectual praxis *and* on a global vision of liberalism circulating through indigenous public spheres and international media to define the nation's cultural crisis. But Chinese students went beyond their advisors to push for what could not be expected. The student demand for dialogue with the authorities was a more radical challenge to hierarchy than most Western observers appreciate in their imaginations of democracy for others. Studying intellectuals directly invites culture and politics to meet in the social and encourages us to explore the variety of critique and the authority for its expression.

Geoff Eley shifts the familiar critique of critical intellectuals—the recurrent gap between rank and file conservatism and the doctrine of ideologies—away from state policy and the pronouncements of elites toward the implication of German socialist ideology in the everyday life of its working class. He shows how the socialists' prewar cultural conservatism and Kaiserreich's authoritarian political culture limited counterhegemon-



ony and intellectuality's spread. But he also demonstrates how the socialists created the subculture in which cultural radicalism of a wider organic intelligentsia was produced. Exploding and splitting under Weimar, the group of organic intellectuals faced other challenges too: their condescension for everyday worker culture and new media and fascist movements that captured popular desire better than they.

James Epstein's story of Richard Carlile and the English *zetetic* clubs of the first half of the 19th century reinforces the analytical appeal of such an expanded theory of intellectuality. Carlile's followers were uncredentialed autodidacts but with a commitment to reason that marginalized them from the masses. Another story of distance from the masses with more familiar invocations of intellectual distinction is Jane Burbank's magisterial account of the Russian intelligentsia from Peter the Great through Gorbachev. Her review of debates on the group's historic role shows that while they were not Gramscian "organic" intellectuals, they always managed to claim authority to speak on the nation's behalf.

When the nation, rather than public policy or popular mobilization, is the object of intellectuals' desire, the expert/critical tension looms less large. The nation's lability limits obligations of reason and empiricism. While the popular might resist the intellectual's refashioning and the state demand the presentation of expertise, the nation can be held to be more than what exists and accessible only to peculiar vision. Even without the ear of the state, the support of the movement, or the advantage of the moment, intellectuals often claim authority to define the nation, if not always with immediate consequence. What privileges this articulation, and when is it consequential? The answer begins here. This collection offers the comparative historical sociology of public intellectuals a wonderful foundation for teaching and puts the public consequence of intellectuality to the center of our work.

*The Enchantment of Sociology: A Study of Theology and Culture.* By Kieran Flanagan. New York: St. Martin's Press, 1996. Pp. xiv+226.

Richard K. Fenn  
Princeton Theological Seminary

Flanagan is a senior lecturer in the sociology of religion at the University of Bristol. In this book, he surveys not only the complex relationships between sociology and theology, but he roots them in what he perceives to be the dominant trends of a postmodern social system. A Roman Catholic himself, he is at war with theologians who have become too eager for relevance and with sociologists who fail to discern the truly sacred from its many false appearances. *The Enchantment of Sociology* is part analysis, part diatribe on the order of Julian Benda's Durkheimian attack on utilitarians in *The Treason of the Intellectuals* (Norton, [1928] 1969). Not only sociologists but the clergy and theologians have lost their capacity

to discern the sacred and to pass judgment on false gods. Caught up in a world that they mistakenly believe to be secular, they miss its various forms of sacralization. Secularization, in Flanagan's view, is a myth; at best it characterized "modernity" when "the rules of engagement" between the religious and the secular were clearer and more widely understood.

If sociologists were to be more to Flanagan's liking, they would develop a number of critical faculties and capacities for judgment. They would need, first of all, the capacity to distinguish the sacred from the mundane or secular: a difficult task in a world that blurs the boundaries between them. In his sixth chapter, Flanagan argues that sociologists must recognize that they are living in a postsecular world, where "religious objects are resited on a field of religious indifference" (p. 190). Drawing heavily on an earlier discussion of Pierre Bourdieu, Flanagan stipulates that the cultural "field" is a difficult one in which to wage battle for the sacred. There are too many players who can produce and consume the sacred, naming it to their own satisfaction. It is also difficult to know what are the rules for fighting over what is and is not sacred.

According to Flanagan, sociologists ought to be better at this game than they are, but sociologists are caught up in their own subfields, where they talk about postmodernism without the benefit of any "transcending affinities." Because they are tone-deaf to religion and theologically illiterate, they are in no position to separate the truly sacred from the sham. In a world of overlapping "fields," sociologists have no sense of their own boundaries and are in a "liminal" relationship to theology. Thus they lack the firm ground and clear boundaries that might otherwise enable them to pass judgment on those who turn religious symbols into commodities. A more serious indictment is Flanagan's accusation that sociologists are oblivious to their own petty heresies and thus unable to recognize their own Pelagian or Neoplatonic tendencies. A "reflexive" sociology, he would argue, should be more adept at theological self-criticism.

The tone and some of the content of Flanagan's book echo Durkheim's diatribe against the utilitarians. There is a similar impatience with those who study the practical and the mundane without discerning the universal; those who see only the parts but fail to recognize the whole miss the very groundwork of social life and mistake form for substance. Flanagan's sense of despair frequently reminded me of Fritz Stern's (*The Politics of Cultural Despair: A Study in the Rise of German Ideology* [University of California Press, 1961]) "politics of cultural despair": the ethos that provided a cultural demand for fascism. Flanagan sees a danger in any demands for purity and totality, but he locates this danger in the residues of the Enlightenment rather than in his own Durkheimian tendencies.

It is crucial, Flanagan argues, for sociologists to speak of "the cultivation of virtue, and the subcultures that embody and realise its basis" (p. 120). A Christian sociology may be in order, but in that venture, sociologists will get little help from theologians, who have sold their birthright for a mess of liberal or utilitarian pottage. If Flanagan is right, there also

should have been more protest and judgment from sociologists who have studied the process of secularization. He is particularly scathing with regard to the work of Bryan Wilson. Wilson (*Religion in Sociological Perspective* [Oxford University Press, 1982]) has (rightly) discerned the decline of the sacred in social life but is too dispassionate about it. According to Flanagan, Wilson's response has been "neutered"; Flanagan even accuses Wilson of using sociology as a "professional mask" for his own "death wish to belief" (p. 115).

His colleagues in the sociology of religion might not agree with Flanagan's representation of their work. For instance, not only has Wilson explicitly warned of the negative effects of secularization on social life, he has pointed out that Christianity itself has been a major force promoting secularization and the loss of the social ties and constraints that dignify local communities. Conversely, David Martin (*Tongues of Fire: The Explosion of Protestantism in Latin America* [Blackwell, 1990]) has pointed out that Christianity not only dissolves some forms of traditional obligation but creates new networks of affiliation and obligation to replace the old. Unlike the sociologists whom Flanagan finds illiterate with regard to religion and theology, Martin's work is theologically sensitive and acute. Wilson and Martin are not the only sociologists whose work Flanagan fails to appreciate; Niklas Luhmann also receives unnecessarily abbreviated treatment, and his work on societal differentiation and on the evolution of religious beliefs is ignored. On the other hand, Flanagan has a particularly high regard for Bourdieu, whose work he finds convenient for discussing the ambiguities of symbolic production and consumption and for suggesting the presence of the sacred in an allegedly postmodern and postsecular world.

Toward the end of his reflections, Flanagan reverses his field. Instead of finding a dearth of meaning in the world, he discovers a surplus: an excess of form over content, as he puts it, in his discussion of Georg Simmel. However, there is no way for individuals to sort out all these meanings or to come to terms with them. Modern individuals have lost the disciplines that enable them to relate form and content to their own experience. It has been a function of magic to protect individuals from the surplus of meaning and to reduce that surplus to something that an individual can grasp. This is now a function, however, that Flanagan attributes to sociology, although he implies that sociologists lack the grace to know that they are standing, so to speak, on holy ground. Hence, he would argue, sociology should seek a certain reenchantment.

Magic, however, is the disease as well as the cure, a recipe for disappointment and renewed helplessness. Unfortunately, Flanagan appears to ignore this point in his longing for a return to an enchanted universe. With his yearnings, it is therefore not surprising that he complains of melancholy and malaise and sees signs of hope primarily in religious virtuosos who have recently rediscovered the monastic vocation. Flanagan wants very much indeed to move beyond dry sociological theory toward a genuine and more conclusive engagement with theology. Unfortunately,

he finds theology by now far too sociological to understand its own basis in faith and grace. After reading this challenging and sometimes acute book, I would hope that Flanagan himself would engage more directly in theological pursuits rather than wait for sociology to become reenchaned.

*Religion and Mass Media: Audiences and Adaptations.* Edited by Daniel A. Stout and Judith M. Buddenbaum. Thousand Oaks, Calif.: Sage Publications, 1996. Pp. 294. \$48.00 (cloth); \$22.95 (paper).

Jennifer L. McKinney  
*Purdue University*

There is no question that media and religion are important elements in U.S. society. Researchers in both mass communication and the sociology of religion have developed strong literatures in these fields, yet neither tends to include the other. In *Religion and the Mass Media*, Stout and Buddenbaum provide essays integrating these two areas to examine religious institutions and audiences. The significance of this lies in the fact that previous research in these areas has focused on a content-centered approach. Stout and Buddenbaum explore religion and the media centering on the audience, asking, What do churchgoers themselves have to say about media and religion?

The first three sections of the book encompass the current status of religion and media research, institutional perspectives of media, and audience behavior. Current and past research is summarized first, situating the reader within the interdisciplinary approach to religion and the media. Several explanations for mass media use, including community integration, secularization theory, ideological frameworks, and psychological perspectives are described. The historical contexts of media use by U.S. Christian groups guides the second section, highlighting how these groups have incorporated the emergence of media into institutional teachings, evangelistic efforts, and preference/avoidance strategies of media use. Four empirical studies on the comparative use of media across Christian traditions explain how audience members respond to and then practice their religious directives. These studies demonstrate the relationship between religion and tolerance, differential newspaper use and orientations between groups, and group reactions to potentially threatening media events.

The last two sections of the book include case studies and the future of religion in the information society. The case studies focus on audience member adaptations of mass media in everyday life. Mormons and fundamentalists display within group differences of media use, while Quaker and Mennonite groups utilize various strategies for media use that maintain identity in the face of and even through the media. The use/avoidance of "gospel rap" in the black church is also explored. Each study

underlines the point that, although media use by religious groups varies, media plays a very important role in the everyday lives of religious audiences, in support or in spite of institutional directives. Jorge Reina Schement and Hester C. Stephenson conclude by analyzing how media roles are defined and what this means in the emerging information society. These authors explore major trends that shape awareness, as well as major tensions that dominate the information society, and the subsequent implications these have for the practice of religion.

Stout and Buddenbaum have collected essays that take a pointedly different approach to the study of religion and mass media. However, in achieving "a creative synthesis of ideas between mass communication research and the sociology of religion" (p. 5), they fall a little short. Although the emphasis is on integrating these two disciplines, the editors have selected essays that account for religion and media from a communication perspective. In fact, no sociologists of religion have contributed to this collection. Sociological concepts are included, such as secularization theory and Merton's community integration, but these concepts are simply points of departure. Sociological theory does not overtly drive any of the essays, expanding on, or adding to current sociology of religion.

The editors of this book initially situate the reader in the current state of research in religion and mass media. This is followed by a selection of essays that track the use of media within the traditions of Catholicism, mainline Protestantism, evangelicalism, fundamentalism, and Mormonism. These historical contexts, particularly Quentin J. Schultze's analysis of evangelicals, are excellent reviews of doctrinal and practical media use by these religious organizations. This particular section is a valuable resource for anyone interested in religion, communication, or the integration of these two areas.

A second critique, then, lies in the book's consistency. The historical contexts of the traditions set forth in section 2 should set up and organize the essays in the following sections, giving a solid overall view to these particular religious bodies. The remaining empirical and case studies, however, do not follow explicitly from these five traditions, resulting in the second half of the book becoming less focused. Catholic groups are not included in the following case studies, and although Quaker, Mennonite, and black church groups can be folded into one or a mixture of the previously discussed traditions, an understanding of their media use in a larger historical context would certainly enrich and tighten the focus of the collection.

Despite some of these shortcomings, *Religion and Mass Media* initiates an intriguing dialogue between researchers in communication and the sociology of religion. This book takes a fresh approach toward research, stimulating many new research questions. As the editors note, "Given the importance of religion and the mass media in U.S. society, however, the potential value of conducting research in this area seems well worth the effort" (p. 30), and so it does.

*The Germanization of Early Medieval Christianity: A Sociohistorical Approach to Religious Transformation.* By James C. Russell. New York: Oxford University Press, 1994. Pp. ix+258. \$42.00 (cloth); \$15.95 (paper).

Lutz Kaelber  
*Lyndon State College*

Religions differ in their approaches toward the secular sphere. As Max Weber noted, those religions that combine a dualist conception of the nature of the universe with the notion of a personal divine creator tend to devalue the worldly realm. Their religious teachings encourage believers to look past this world and to set their hopes on the beyond instead. Weber termed this a *world-rejecting* worldview. Nondualist belief systems and religions espousing the idea of impersonal eternal order, in contrast, are more likely to hold favorable views on what goes on in this world; consequently, they are more likely to adopt a *world-accepting* worldview. James Russell's book, which is a revision of the author's doctoral dissertation in historical theology, discusses what happens when these different perspectives collide. It does so in the context of what is commonly called the Christianization of Germanic peoples in early medieval Europe. Russell describes this as a cultural encounter between a predominantly world-accepting Indo-European folk religion and the world-rejecting views of Christian missionaries. His thesis is aptly captured in the title of the book, namely that the Christianization of early medieval Europe was equally a Germanization of early medieval Christianity.

Russell's argument, while valid to a point, is hardly as novel as it might appear to be. It has long been established that the early medieval Christian Church was heavily involved in ideological boundary work, by which it actively recommissioned some previously inadmissible pagan acts in new forms for Christian ends. As historians have noted, Christian missionaries faced ample competition in the form of diviners, soothsayers, and other manipulators of the supernatural. A well-established part of the culture of pre-Christianized peoples, these magicians offered those who sought their services the affirmation of some preternatural control over nature at a time of widespread social and political instability. Rather than taking this cultural world head on, for which it had neither the personnel nor the necessary means of communication, the Church selectively borrowed practices from competing pagan manipulators and promoted these practices as part of a pool of interceding acts considered legitimate for Christian purposes. At the same time, the Church vilified those who continued to perform such practices under a different authority and without its sanction. Places where people revered stones, fountains, or trees were turned into sites for oratories, chapels, and shrines. Even the drawing of lots and certain forms of astrology were cast into Christian molds. In turn, some licit religious artifacts proved to be amenable to manipulation, as Christian formulas turned into spells and the consecrated host was put to magical use. In the realignment of religious wor-

ship and magical manipulations, many, but of course not all, of these practices came to run together. A strong Germanic influence on Christianity, Russell argues, can also be found in the development of a proprietary church system, certain military ideals, notions of sacral kingship, and the *Adelsheiligwesen*.

Most of this has been known for some time, so the value of Russell's book falls or rises with the adequacy of its theoretical underpinning and the accuracy of its historical analysis. Serious problems exist in both regards. Russell is apparently unaware that the very concepts of world rejection and world acceptance have their origin in the writings of Weber, whom he mentions all but twice. He consequently also fails to consider Wolfgang Schluchter's recent theoretical elaborations on this subject. Rather than a coherent argument informed by sociological theory, Russell's approach to religious change seems more like an assembly of disjunct and at times trivial propositions, the empirical usefulness of which remains in doubt. For example, he states that "social structure influences ideological structure and both contribute significantly to a society's general religious orientation" (p. 102). What insight is gained by such a statement, touted as a "fundamental postulate" of a general model of religious transformation? Is it useful to describe, in a different context, the worldview of early medieval Germanic societies as "sociobiological" (p. 212)?

The weakness of the book's theoretical formulations are complemented by a historical part that, to a significant extent, consists of an assortment of long quotations in the text and footnotes. Russell habitually tells the reader what various scholars have asserted, stated, observed, noted, discussed, shown, and summarized—without any critical discussion of the views presented. In doing so he all too often mixes arguments that derive from notable analyses of the subject with those of somewhat dubious scholarly merit. It remains unclear, not only how much this study goes beyond what has already been established in historical scholarship (e.g., Valerie Flint's magisterial *Rise of Magic in Early Medieval Europe* [Princeton University Press, 1991]), but also whether its author has achieved a solid mastery of the relevant literature. For these reasons, I can recommend Russell's book only with grave reservations.

*Social Change and the Middle Classes*. Edited by Tim Butler and Mike Savage. London: University College of London Press, 1995. Pp. xii+388. \$29.95 (paper).

Mark Western  
Australian National University

Whether the object is to theorize how professional and managerial occupations fit into the class structure or to understand middle-class politics or how a diversifying class structure figures in debates about the "death

of class," contemporary class analysis is increasingly concerned with the middle-class(es) in advanced capitalism. *Social Change and the Middle Classes* examines these issues in recent British society. It is primarily a textbook but contains sufficient new material to also represent the current state of play of British social science in this area.

The book is underwritten by a "class formation problematic" (p. 23) in which class analysis is about examining if and how class structural relations give rise to social collectivities. This approach combines a minimalist view of class structure with a maximalist understanding of class formation. The causal significance of class structure in different areas of social life is an open empirical question, not an a priori assumption, but the kinds of collectivities that potentially emerge are multifarious. Thus, chapters take up (among other things) issues of gender and race in class formation, changing professional and managerial careers, the impact of public-sector restructuring on middle-class state employment, middle-class use of domestic servants, links between geographic and worklife mobility, the role of the middle classes in "colonizing" the British countryside, class differences in diet and home ownership, and middle-class politics. Yet despite its scope, the book is extremely well integrated because several questions emerge repeatedly: What does the class formation problematic entail? How should the middle class be conceptualized to best explain class formation? How are class processes changing over time?

The dominant figure in British class analysis is undoubtedly John Goldthorpe, and many chapters engage either his conception of the middle class or his class analytic agenda. Goldthorpe asserts that professional, administrative, and managerial employees form a "service class" distinguished from other employees by the relationship they have with their employers. In contrast to contractual relationships in which nonservice-class employees exchange specific tasks for specific rewards, the service relationship is defined primarily by prospective benefits that service-class members anticipate over their working lives. Expectations about future benefits link the interests of service-class employees to those of their employers and predispose them to political conservatism. Recently, however, Mike Savage, James Barlow, Peter Dickens, and Tony Fielding (*Property, Bureaucracy and Culture: Middle Class Formation in Contemporary Britain* [Routledge, 1992]) have adapted Erik Wright's conceptualization to explain British middle-class formation in terms of the way professionals and managers mobilize property, cultural, and organizational resources. In concrete terms, the difference in the views of Goldthorpe and Savage et al. comes down to a disagreement about whether managers and professionals constitute a single class or different middle-class fractions and the consequent implications this has for class formation. Two early theoretical chapters in *Social Change and the Middle Classes* address these issues, while the empirical chapters on career mobility, regional mobility, diet, and politics also examine how well service-class and asset-based conceptions explain differing aspects of class formation. Con-



cluding chapters by Goldthorpe and by Savage and Butler are especially useful in clarifying how the two perspectives differ.

Other chapters, particularly those dealing with the intersection of class and gender, interrogate the concept of class formation through a critique of Goldthorpe's agenda for class analysis. Goldthorpe has prescribed a research program that takes the structure of class locations as given and investigates mobility patterns and sociopolitical class formation as conditional on this structure. In their chapters, Anne Witz and Rosemary Crompton argue that this characterization misses fundamental questions about the intersection of class and gender relations, particularly those pertaining to the way sex-segregated class and occupational structures arise and the extent to which men's mobility opportunities are predicated on women's exclusion from privileged class locations and the domestic labor of service-class wives. Crompton further asserts that as women increasingly enter middle-class occupations, the service class will fragment into a range of household types that will undermine its putative solidarity.

Crompton's chapter also highlights the third recurring theme of the book—social change. The contemporary reorganization of class processes is a major focus of chapters dealing with public-sector employment, black middle-class formation, middle-class careers, the spatiality of class formation, middle-class politics, and middle-class consumption.

Overall, this book presents British class analysis as theoretically sophisticated, substantively informed, and committed to empirical analysis. It also demonstrates the power of class analysis to inform investigations of social action and social change. U.S. researchers should find it invaluable as a summary of recent British research and a comprehensive treatment of contemporary British society.

*Industrial Constructions: The Sources of German Industrial Power.* By Gary Herrigel. New York: Cambridge University Press, 1996. Pp. x+480. \$54.95.

Sidney Pollard  
*University of Sheffield*

The thesis of this book is that the traditional accounts of Germany's astonishing rise to industrial power from the 1870s to the present, which concentrate on the large units in cartelized industries aided by the large universal banks, are incomplete. For a full understanding, we have to give equal weight to a range of other industries, consisting of medium- and small-sized firms and financed by regional or cooperative banks. The leading *Konzerne* in the former sector, above all in iron and steel, coal mining, chemicals, and electrical engineering, were generally able to stand alone and are here dubbed *autark*. The other set, mainly consumer

goods producers but also some types of engineering, depended on specialization backed by complex horizontal and vertical linkages to obtain components or fulfill mixed orders and is termed "decentralized." The structure was clearly regional; some regions, such as the Ruhr or Silesia, contained only the autarkic type, while others, such as Baden-Württemberg or Saxony, were devoted exclusively to decentralized industries. The political structure of Germany, from the separate governments included in the Kaiserreich to the federalism imposed by the Allies after 1945, lent itself easily to such regional specialization, but the central government always managed to devise policies to benefit both types of region.

No brief review can do justice to the wealth of detail and the broad range of sources with which this thesis is supported, backed as it is by footnote discussions that add 50% to the length of the book. Though the prevalence of smaller firms has not gone unnoticed in the past, beginning with Clapham's classic text (*Economic Development of France and Germany 1815-1914* [Cambridge University Press, (1921) 1963], pp. 287-88), this very important contribution will make it impossible, at least in the English language literature, to neglect in future the role of the nonautarkic industries in the economic rise of Germany.

Yet many misgivings remain, largely because of the absolute, all-or-nothing tendency to squeeze the complexities of German economic history into a rigid schema of this kind. Regions, and even industries, were simply not all of one type or the other. The historical account in consequence is peppered with what at one point is rather engagingly called "rule-proving exception(s)" (p. 232). Moreover, there were many more influences upon Germany's economic development than the internal and external structure of firms and the varying training and skill of the workforce, on which this thesis is almost exclusively based.

For example, the on-going debate on the extent to which an emergent regional factory industry was benefited by a previously existing domestic industry, in which both sides can cite numerous historical examples, is simply brushed aside by the dogma that traditional regions could not tolerate major innovations and were therefore avoided by the new industries. Thus the reader is startled to learn that the modern steel industry settled in the Ruhr district, not merely because of the coal and the waterways linking to the iron and the markets, but because there had been no traditional steel-making industry there. A brief glance at Sheffield, leading center of the old and pioneer of the new, would have shown this view to be untenable. Indeed, the failure to cast any glance abroad, in what is after all a fairly generalized view of industrial history, is a major failing of this book. Again, in the Wilhelmine Reich, the one-sided high-tariff protection for iron and steel as well as for grain imposed harmful cost increases on the finished goods industries, but there was no question of even-handed help for them. In Weimar, the division of interests was not, as stated here, so much autarkic versus decentralized industry as it was successful exporters versus those depending on the shrinking home market. Moreover, the bitter debate on social expenditure and its effect on

state-industry relations, a major issue that split industry and society, does not fit the schema and thus fails to get its due.

Lastly, the attempt in the final pages to draw far-reaching conclusions on Germany's future from the results, and possibly short-term blips, of one or two recent years, would seem to fit more appropriately into a journalistic essay than a serious history. The structural analysis of the earlier chapters, even were it wholly acceptable, can surely provide no legitimate base for ad hoc prophecies.

He who offers sweeping generalizations must expect to be subject to detailed and perhaps even niggling criticism: this does not detract from the value of new perspectives. Backed, as they are in this case, by rich detail and knowledgeable insight, they make an important contribution that no one in the future will be able to ignore.

*Growth in a Traditional Society: The French Countryside, 1450–1815.*  
By Philip T. Hoffman. Princeton, N.J.: Princeton University Press, 1996.  
Pp. xiv+361. \$39.50

Jack A. Goldstone  
*University of California—Davis*

Major controversies in early modern history may now be approaching closure, thanks to the painstaking work of Philip T. Hoffman. Did French agriculture lag behind that of England and the Netherlands? And was this due to differences in communal land practices, or farm size, or taxation? For those seeking answers to these questions, *Growth in a Traditional Society* will be an essential, indeed foundational, work.

Distilling the work of over a decade in the archives, Hoffman presents new and exceptionally detailed data on French agricultural productivity. The main data are drawn from the lease records of Notre Dame, one of the largest landholders in the Parisian Basin. These data are complemented by leases drawn from 19 additional sites around France, which Hoffman follows for three centuries prior to the French Revolution. Not only are Hoffman's data new, so too is his analysis. Hoffman aims at the economists' ultimate measure of agrarian efficiency: total factor productivity (TFP). This is calculated by using the lease data and price data to obtain the total costs of the inputs for each farm (land rents, taxes, animal rentals, wages paid for labor) and comparing those costs with the prices of the outputs (crops and animal products). When price weighted for their shares in production and output, these figures can produce estimates of how much "value added" the farmer provides; in other words, what output he/she gets for a given level of inputs. When these figures are tracked over time, as Hoffman does, we can see the pace of agricultural improvement down the centuries.

Hoffman's results are sometimes reassuring, sometimes startling, but always backed by detailed investigation of the data, and careful consider-

ation of alternative explanations. Although his findings are too rich to summarize fully in a review, three main results stand out.

First, Robert Brenner, who in the famous "Brenner debates" argued that the class structure of landholding in France shackled its agriculture, is simply wrong. Brenner maintained that the tiny sizes and high rents of French peasant farms, the result of landlord domination of a prostrate peasantry, prohibited high productivity. Yet this picture is mistaken on every count. French farms with tiny plots and high rents *did* exist in large numbers in the Paris Basin. But despite their large numbers, they covered only a small part of the total farmland and were not the major source of grain for sale. These small farms were more like the cottage gardens of English day laborers. The French peasants who leased these plots were essentially wage workers on larger commercial farms, who kept a small plot to farm on their own. Thus in years when wages were low, a small garden plot provided them a bit of insurance against dearth. In fact, larger farms, which though less numerous covered a far greater area, paid lower rents, grew crops mainly for market, and—at least in the Paris Basin—were as productive as most regions in England.

French agricultural productivity growth lagged badly *not* in the areas with tiny peasant plots, which only existed where large, efficient commercial farms also provided wage opportunities, but in the areas of medium-size family farms. These farms—which covered most of France outside the Paris Basin—lacked the capital, the market opportunities, and the specialization that led to increased productivity. These family farms throughout France, not the tiny plots of wage workers in regions near Paris, were the reason for the relative backwardness of French agriculture.

Second, the great French rural historians—Marc Bloch, Pierre Goubert, Emmanuel Le Roy Ladurie—were at least half right. They had argued that French agriculture had remained almost unchanged, *immobile*, from the late Middle Ages right up to the Revolution. Hoffman shows this to be largely true as a gross average. But it was far from wholly true of all times and places. Hoffman shows that productivity gains in the Paris Basin were consistently high, with total outputs comparable to those of southern England. Moreover, although other parts of France had very little long-term productivity growth, this overall stasis concealed periods of healthy agricultural growth (mainly in peace and good climate) that were undone at intervals by the predations of war, which destroyed accumulated capital in land improvements and livestock and set back productivity by decades. In other words, the pattern of French productivity growth is like a chessboard: seen from a distance, it appears a grainy brown, but when examined in detail, it has areas of light interspersed with areas of dark.

Third, Hoffman confirms my own estimates and those of Ernest Labrousse regarding the contribution that lagging agricultural productivity growth made to economic pressures in the years leading up to the French Revolution. While recent revisionists have suggested that there was really

little difference between England and France prior to the Industrial Revolution, and that French agriculture experienced its own period of rapid gains in the 17th and 18th centuries, Hoffman shows that such arguments are off base. As Hoffman puts it (pp. 132–35), “The overall performance of French agriculture was disappointing, even under optimistic assumptions. . . . Ultimately, it took them three centuries to accomplish what the English did in two. [In the centuries prior to the Revolution] food supply grew by less than 0.22 percent per year [while] the French population . . . rose at . . . 0.21 to 0.32 percent per year.” Although Paris’s immediate hinterland showed great gains after 1750, this did not change the picture, for “the local food supply perhaps tripled between 1500 and 1789, but the city’s population nearly quadrupled,” and Paris merchants were forced to move ever farther afield, into areas with lower productivity, to seek food for the capital. As a result, “food prices climbed, and real wages . . . failed to keep up. Only landlords had reason to be gleeful, as rents soared under the pressure of the population.”

In sum, French agriculture was capable of great progress, but only where capital, large urban markets, and commercial farms combined. Unfortunately for France, such areas were few. More commonly productivity stagnated, leaving France prone to periods of economic distress when population surged. For presenting this story with unprecedented detail and evidence, and, I should add, clear style and rich argument, we are indebted to Hoffman.

*Time Pioneers: Flexible Working Time and New Lifestyles.* By Karl H. Horning, Anette Gerhard, and Matthias Michailow. Translated by Anthony Williams. Cambridge: Polity Press, 1995. Pp. xi+198. \$54.95.

Graeme Salaman  
*Open University*

This book is about time. It is about a group of people for whom the flexibilization of their working time is intended to secure that they “have more time.” These people are called “time pioneers” (TPs), since they are seen to pioneer a new approach to a contested resource—time itself. The authors see the TPs as spearheading a new response to employers’ demands—not with respect to the traditional trade-off between effort and wages, nor even necessarily over the sheer *quantity* of time worked, but over the flexibility of working hours. TPs do not wish simply to reconfigure their working time to make their work more convenient for other schedules: they wish time for themselves. And they are prepared to pay for it: to trade working hours for their own time. They seek to reconfigure the prevailing time-money exchange by reorganizing everyday life in terms of time.

The analysis is based on the primacy of “lifestyle” as an explanatory concept, which here refers to the capacity for making subjective prioritiz-

ations and interpretations. The focus is thus on choice, associated with individuality and identity. The study claims to address the role of subjectively meaningful time in the constitution of identities—to use time as an analytical category to investigate the conduct of life, the construction of identity and of meaning. Thus the study is not simply of people who wish to shift the boundaries between work and nonwork time, but rather of the possibility that such shifts indicate new social perceptions of time.

The research group was selected by criteria that would identify new forms of time utilization. The researchers specifically set out to find people whose working arrangements deviated from standard working time in a number of practical ways concerning their temporal employment arrangements. However, qualitative criteria were also used to ensure that respondents not only experienced specific employment arrangements but had a definite subjective approach to, placed a high value on, and had a sensitivity toward time. The study's conclusions are based explicitly on data gathered from this carefully selected sample.

Chapter 2 describes the characteristic time attitudes of the TPs in contrast to conventional attitudes and locates these in the context of employers' efforts to (re)organize working time in accordance to their priorities. The study focuses on the ways in which TPs' efforts are contested within the employment relationship. Flexibilization of and reduction in working time frequently result in the intensification of work content, through a number of employer strategies. Other difficulties are also noted, including the impact on relations with colleagues and superiors and on the TPs' capacity to demonstrate and substantiate work performance.

Chapter 3 addresses the work attitudes of the TPs and discusses the difficulties they encountered in dealing with the reactions of their conventional colleagues—reactions that reveal a clear set of interpretative schema about the meaning of work, identity, and time (e.g., that "free time" is determined by working time).

Clearly the TPs' decisions about working time have major implications for their income, and TPs are required not simply to manage reduced incomes but to devise strategies whereby they need less money, establish new household regimes, rely on DIY (do it yourself), networking, and so on. This is explored in chapter 5. The time-money balance is now reconfigured in a way in which expenditure patterns and levels are geared to a prime commitment to low and flexible level of time commitment.

At its extreme this reconceptualization of the time-money exchange becomes more than a strategy for getting by with less: it directly questions the central social mechanisms which posit the convertibility of time and money. Chapter 6 furthers the discussion of the TPs' new lifestyle, with its central rejection of the centrality of work and employment to identity, an associated fundamental revision of consumer patterns and habits, and an increase in self-control over the shaping of individuals' lives. The final chapter raises some further implications of the study.

The book is useful for its analysis of a relatively underresearched phenomenon. Although TPs are not—despite the claim of the authors—a

new phenomenon, they still merit attention particularly at a time when employers, through "downsizing," reengineering, and other initiatives are intensifying employees' contributions to the wage/effort exchange. The book certainly advances our understanding of the content and implications of the TPs' approach, if it does not greatly help us to understand the etiology of this approach. The weaknesses of the book arise from three features. First, the very focus on TPs as a distinctive and discrete cluster of attitudes may, paradoxically, serve to represent the "conventional" approach as more homogeneous and unreflexive than it is in reality. It may be that the TPs' portrayal of the conventional approach reflects their need to define their position as much as it draws any stark and persistent differences between TPs and non-TPs. Second, it would be useful to be able to position this analysis demographically, with respect to locations within the lifecycle. For example, are the young more or less likely to adopt this stance? Third, while there are en passant references to current discussions of issues of identity, the full wealth of the possible explorations of these connections is underdeveloped. Specifically, the relationship between consumption and identity is handled in a restricted manner, whereby TPs' identities are seen to involve a unique and distinctive balance between employment, nonwork, and consumption elements. But this discussion is not grounded in recent debates about the ways in which new forms of organizational practice have influenced issues of identity and in particular have blurred differences between production and consumption identities; thus the analysis of identities is somewhat limited. Finally, while the analysis of contemporary changes in the role of work in identities is timely and important, it is necessary to note that what it means to be a worker or an employee is not and never has been set rigidly but has always been dependent on historical and cultural conditions. Work has no more "real" meaning than time; and it is one of the strengths of this book that it advances our understanding of the historically and socially contingent nature of the role of both of these in identities.

*The Business of Practicing Law: The Work Lives of Solo and Small-Firm Attorneys.* By Carroll Seron. Philadelphia: Temple University Press, 1996. Pp. xiv+224. \$49.95 (cloth); \$22.95 (paper).

George Gonos  
Centenary College

The tension between the traditional norms of professionalism and a rampant new commercialism is being felt across a range of fields, and Carroll Seron's book, delving into the dilemmas this tension has created in the legal field, provides a valuable sociological treatment of the subject. Since the late 1970s, the loosening of rules surrounding legal advertising has spawned a growing number of highly visible firms that operate on the principle of business efficiency. Seron asks whether such entrepreneurial

firms are replacing more traditional legal practices. Through in-depth interviews with 102 solo and small-firm lawyers in the New York regional metropolitan area, she inquires into how legal professionals are constructing their practices and professional identities given the competing value orientations and options now available in the field.

Rather than a linear trend toward more commercialized firms, Seron finds that the legal field, fueled by robust growth in the suburban service economy, sustains a range of approaches to practice coexisting in "expanding but mutually exclusive segments of the market" (p. 105). Three distinct types of practitioners are delineated—traditionalists, experimenters, and entrepreneurs—and Seron's interviews focus on the different strategies each pursues with regard to three key aspects of legal practice—getting business, organizing work, and serving clients.

The continued predominance of traditional firms is a major finding. These firms are deeply rooted in local communities and built on collegial, family-like bonds between partners. (It may be disconcerting to find out that, typically, these firms are founded only on a handshake since, as one attorney says, a written agreement "doesn't really mean anything" [p. 70].) Though they believe lawyers should have the right to advertise, the consensus among traditionalists is that unprofessional advertising is wrong. Their primary means of acquiring clients is the careful cultivation of contacts through extracurricular activities, e.g., joining the "animal" clubs. With respect to office technology, they describe themselves (with pride) as computer illiterate and prefer dictation to the computer terminal. Since their legal work is routine, social skills are considered more important than technical knowledge; hence, the work—dealing with individual clients with "real" problems—retains a craft-like feel.

While the experimenters may dabble with advertising, the entrepreneurs use it extensively. They serve not clients, but consumers who, they believe, want legal services to be "cheap and fast" (p. 108). Here, there may be "not even a veneer of collegiality" (p. 98). Manager-partners design marketing plans and training manuals and develop standardized procedures for handling cases. Clients have extended contact with paralegals and only limited access to attorney-employees (or independent contractors). Computerization allows the entrepreneurs to make a "systematic assessment of the return on their marketing and managerial investments" (p. 87).

The persistence of a "deeply gendered" (p. 68) pattern of organization across both traditional and entrepreneurial firms is a theme sustained throughout the book. Through reports elicited about their working days and the division of labor at home, Seron finds that female attorneys "actually share with their male counterparts a deeply traditional and conservative value orientation about 'proper' gender roles" (p. 17) and that only in rare cases have they attempted to renegotiate the division of household obligations with their husbands. Male attorneys thus have the advantage of making private arrangements that free them to put in the "expanded professional hours" that are pivotal for effective business development.



At the office as well, Seron finds that small-firm attorneys rely on a traditional division of labor between professionals and support staff ("our girls").

Other genuinely intriguing aspects of this study must be left aside to take up a more general concern. Though Seron states that the newer entrepreneurial firms "have gone beyond the somewhat rigid, factorylike model developed by Jacoby and Meyers" in the direction of greater "creative flexibility" (p. 97), such claims are left unsupported by her data. With the legal profession's standardized product, hierarchical decision making, and so on, the reader is struck by just how thoroughly Fordist these operations appear. Similarly, Seron's frequent assertions to the effect that the entrepreneurial firms represent "a model of postindustrial design" (p. 20) seem based on a superficial use of the term postindustrial and a distortion of the original theory to which it refers that typically understood law as something like an objective arbiter of universalist value orientation, not a commodity for sale. Further, by implying that technological change and general economic development have driven the trend toward the commercialization of legal practice, the postindustrial formulation understates the importance of political choice.

Overall, however, Seron's work delivers on its promise to further our understanding of attorneys' work lives in the context of structural change. Considering that its subject area is one in which laments about the demise of professionalism are often substituted for real data, this is a much needed piece of research that will be appreciated by those with interest in the sociology of the professions and, more generally, in work, gender, and inequality.

*Moral Codes and Social Structure in Ancient Greece: A Sociology of Greek Ethics from Homer to the Epicureans and the Stoics.* By Joseph M. Bryant. Albany: State University of New York Press, 1996. Pp. xv+575. \$27.95 (paper).

Joseph Roisman  
Colby College

In this ambitious book, Joseph Bryant writes a historical sociology of the political, social, and intellectual life of the ancient Greeks from the Dark Age to early Hellenistic times (ca. 800–300 B.C.E.). Closely following Weber's and Marx's perspectives on ancient economy and the state, Bryant seeks to combine sociological, historical, literary, and philosophical analyses in discussing Greek institutions and moral codes. Bryant's horizons are commendably wide; the endeavor, however, is only partially successful.

The thesis of the book will be familiar to readers of Weber or of Moses Finley, Weber's most influential follower among ancient historians. Bryant examines the economic, political, military, religious, and family insti-

tutions of the Greeks and links them directly to their values, norms, and ethics. He proceeds to show that any change in the former leads to a response in the latter. The book focuses on the nexus between citizen and polis (or city-state), which Bryant identifies as the essence of the male Greek's experience. He views Greek history as a cyclic process that begins with the individualistic, aristocratic hero of the Homeric epics, culminates in a polis that is a communality of citizens, and concludes with the individualism of the inhabitants of the Hellenistic city. The Homeric warrior monopolized economic and political resources and any claim for honor and military prowess. Greater prosperity and changes in warfare that emphasized collective effort by the heavy infantrymen, or hoplites, led to greater equality in the legal and political rights of the citizens. The process of democratization, though only available to adult male citizens, was reflected in a value system that stressed collective spirit, patriotism, and self-fulfillment through civic service (p. xiii). Limited economic resources pushed the Greeks into acquisitive, predatory conduct both in interpolis warfare and domestic factionalism. These, in turn, exacted a heavy toll on the citizens and undermined the integrative function of the polis's institutions. The gaps between rich and poor, the prevalence of unattached mercenaries, and general depolitization weakened the state and its communal, egalitarian-oriented ethos. In the 4th century, the polis was unable to resist the monolithic power of Macedon and lost the cornerstone of its being, independence.

Within this process, Bryant situates intellectual products that mirrored their age. Thus Homer described the aristocratic ethos and Hesiod the commoner's protest of the dark and archaic ages. Socrates, Plato, and Aristotle tried to save the classical polis by suggesting ways of strengthening communal bonds in the face of threatening circumstances. The Cynics, the Epicureans, and the Stoics gave up on the polis and offered as an alternative individualistic self-fulfillment.

My simplification of the author's leading thesis is dictated by the constraints of a short review. The book offers a wealth of information, and Bryant's descriptions are often lucid and useful for the layperson—when, for example, he describes Socratic or Platonic thinking—but the work suffers from some fundamental problems as well.

Integrating sociology, history, philosophy, and literature is a task full of tension, because each discipline pulls toward its particular methods and theorems. Such methodological difficulty can be overcome when dealing with a relatively limited topic, but not when dealing with the entire political and cultural history of the independent polis. To accommodate his holistic view, Bryant opts for a textbook format, and his account, though often sound, is largely familiar. There are lengthy, irrelevant discussions (especially of philosophy) and a strong inclination toward generalization, polarization of attitudes, economic determinism, and reductionism. For example, he admits that politics governed economy in the polis but explains wars as an economic necessity. Thus, he

excludes the causal role of, say, individual leaders, prestige, friendship, or even the function of war in fostering unity.

Bryant's attempt to identify intellectual products with a particular class view negates the universal appeal of the work. The historical outlook is often class oriented and informed by traditional scholarship. Missing are structuralist and anthropological insights or modern reassessments of the evidence. Such omissions are especially telling in the discussion of the Hellenistic age (chap. 6). Bryant resurrects the view that links the "collapse" of the polis with Hellenistic moral philosophy that focused on individual, rather than communal, Greek experience. But the polis's loss of independence did not mean that it ceased being a very significant focus of political and social life. In fact, the cities took on new responsibilities regarding health care and education that could only have strengthened the bond between polis and citizen (cf. J. K. Davies in *Cambridge Ancient History*, 2d ed. [Cambridge University Press, 1984]: 7.1: 304–20, which is not really utilized here). Bryant does not discuss dominant social institutions, such as friendship (*philia*), or the concept of reciprocity, even though both were important to the moral bond between polis and citizen. Especially regrettable is the lack of distinction between ideology and practice or the uncritical reading of some sources. Communalism was the banner of the polis, but public control of private life was often more wishful thinking than reality. David Cohen and Joshua Ober have shown how amenable political and moral ideologies (themselves often fraught with internal contradictions) were to manipulations by individuals of all walks of life (David Cohen, *Law Sexuality and Society* [Cambridge University Press, 1991]; Josiah Ober, *Mass and Elite in Democratic Athens* [Princeton University Press, 1989]). Bryant takes Demosthenes' rhetorical stereotyping of the pro-Macedonians as wealthy oligarchs as proof that the upper class supported Macedon, while the lower class stood for war and independence. The demos in fact opposed allocating money to the Athenian war efforts. The conflict between Greece and Macedon showed more realpolitik than class politics.

Students of social and moral institutions, especially of state and ethics, will find Bryant an intelligent reader of Marx and Weber. They should be aware, however, of the existence of other valid interpretations of Greek cultural history and institutions.

*Promises to Keep: Decline and Renewal of Marriage in America.* Edited by David Popenoe, Jean Bethke Elshtain, and David Blankenhorn. Lanham, Md.: Rowman and Littlefield Publishers, 1996. Pp. xii+337. \$59.50 (cloth); \$21.95 (paper).

Linda J. Waite  
*University of Chicago*

*Promises to Keep* is an edited volume tightly focused on the single issue clearly stated in its title. The 12 substantive chapters detail changes across various dimensions in the institution of marriage in the contemporary United States, with special attention to the implications of recent trends for children. A final chapter presents a summary of the Council on Families in America's report on marriage. I enjoyed this volume a great deal, found it uniformly well written and sometimes inspired, and—a surprise for a scholar working in precisely this area—came across some ideas and insights that were completely new to me.

The first five chapters document the decline of marriage from various perspectives. In "The Decline of Marriage as the Social Basis of Child-rearing," Barbara Dafoe Whitehead argues that children suffer when adults approach marriage and parenthood as individualistic and expressive pursuits. Increasingly, children's needs for stability conflict with their parents' pursuit of individual happiness. In a chapter on values and attitudes, Norval Glenn explores the paradox that marriage remains a fundamental value for Americans at the same time that the proportion who are married has fallen and reviews reasons for the decline of marriage. Moira Eastman discusses current myths about marriage that have appeared and presents figures that generally contradict them. This chapter fits neatly with the next, by Arland Thornton, which presents comparative and historical perspectives on family and points to the universal importance of family-based social organization throughout the world. A final chapter in this section of the book, by Janet Giele, outlines conservative, liberal, and feminist views of families.

The next section, titled "Religious, Legal and Cultural Dimensions," includes chapters by Don Browning, Milton Regan, Jr., Carl Schneider, and Robert Weiss. In a chapter that I found especially insightful, Browning discusses both the "male problematic"—the tendency of men to mate without making an investment in the children which result—and the "female problematic"—the tendency of women to bond tightly with their children but much less securely with the children's father. Browning discusses marriage as reflected in Christian family theory; Regan and Schneider both focus on family law as it affects marriage. Regan points out that family law has moved from status toward contract, allowing individuals to define what they mean by marriage for themselves and, in the process, undermining the distinctive character of the family. He argues for a new model of status in family law, with a consideration of the shared commitment inherent in marriage. Schneider looks at the fam-

ily as a social institution, supported by the moral discourse in the law. He argues that this moral discourse has diminished and makes suggestions for using the law to reinstitutionalize the family. Weiss discusses the problems inherent in parenting when the adults live in separate households, including the difficulties in forming a parental partnership, alliances between one parent and the child, and parental attachment.

The final section, titled "Rebuilding a Marriage Culture," begins with a chapter by Maggie Gallagher, who sees a shift in power away from the married who wish to remain so toward the unmarried and spouses who wish to leave. She argues that we must shift power back toward marriage to recreate a promarriage culture. David Popenoe presents a series of suggestions for improving marriage and, especially, children's well-being. These include later marriage with a lifetime commitment, withdrawal of the mother from the labor force for at least a year after each birth, and parental provision of most child care. In the penultimate chapter, William Galston reviews the essential functions of the two-parent family and sources of its decline. The last chapter summarizes the report on marriage from the Council on Families in America and presents its extensive recommendations for changes needed to reverse the decline in marriage documented in the report.

This is a book with a point of view, a basic set of assumptions on which the authors of the various chapters build and from which they elaborate. Although the chapters generally make a case for a particular point of view, they are thoughtful and well-informed. It is not possible to provide a detailed series of recommendations for sweeping changes without creating controversy about some of them, and that will certainly be the case here. But I found it a pleasure to read these arguments made by serious and concerned scholars on a topic as central as the health of the institution of marriage. I have come to refer to this book frequently and recommend it highly for researchers, teachers, and policymakers. It will also interest general readers concerned with the health of the family today.

*Disaster Evacuation Behavior: Tourists and Other Transients.* By Thomas E. Drabek. Boulder, Colo.: Institute of Behavioral Science, University of Colorado, 1996. Pp. xvii+354. \$20.00 (paper).

Russell R. Dynes  
*University of Delaware*

There are many kinds of monographs—those that are tendentious and those that are trivial; those whose ideas do not merit more extensive treatment and those that record afterthoughts; those that explain obscure methods and those that only explain the obscure. Consequently, it is a treat to read a monograph that is both theoretically and pragmatically important. Part of a series of useful monographs, it is published by the

Natural Hazards Research and Applications Center at the University of Colorado, Boulder.

The book needs to be placed in the author's previous research. Thomas Drabek, during his long and prolific career in studying disasters, has produced a succession of significant studies from issues of organizational preparation for disaster, to later concerns with search and rescue, family recovery, disaster evacuation, the use of microcomputers in emergency management, and evacuation planning by the tourist industry. In addition to his own research, Drabek performed an important task for others by summarizing the disaster literature in *Human Response to Disaster* (Springer Verlag, 1986). His most current monograph fills some of the gaps that the summary revealed.

Most evacuation studies have assumed a stable residential population who make decisions about evacuations. In reality, all communities have a considerable number of business travelers and homeless people at any one point in time. Also, many disaster-prone communities are located in areas that invite tourism. Drabek's previous work on the tourist industry's reluctance to engage in emergency planning leads quite logically to an examination of the evacuation behavior of transient populations. In effect, the monograph raises the question, When people are away from home and affected by disaster, how do they respond? More specifically, the study centers around five objectives: (1) to describe the behavior that culminates in evacuation from disaster sites by nonresidents; (2) to describe the variations in evacuation behavior among different types of evacuees, events, disaster phases, and locations; (3) to identify factors that affect variations in these behavioral sequences; (4) to document perceptions of disaster victims regarding evacuation policies and procedures implemented in the tourist business as well as by government representatives; and (5) to formulate policy recommendations for local emergency managers and business managers.

To study transient populations requires hard work and persistence, but the data, drawn from 800 persons, are fresh, not simply regurgitated from someone else's data files. The data collection, based on a moving target in unexpected events, was innovative and described in detail.

Drabek concludes his detailed analysis from these data by indicating certain policy challenges and goes further to suggest an action agenda. In his preface, Gary Kreps summarizes that conclusion: "Simply put, customers expect the tourist industry to be prepared for natural or technological hazards and failure to be prepared during an actual emergency will not be good for business. Disaster planning, therefore, is a sound investment for the private as well as the public sectors" (p. xvi).

The present work illustrates Drabek's ability to reach multiple audiences. His research is mainstream, rooted in his knowledge of the research base and his use of familiar constructs, but it is conducted with an eye toward policy implications. Such work is an asset to the discipline since his research is both significant and interesting.

*Possible Worlds: The Social Dynamic of Virtual Reality Technology.* By Ralph Schroeder. Boulder, Colo.: Westview Press, 1996. Pp. x+203. \$24.00 (cloth); \$16.95 (paper).

Ken Hillis

*University of Colorado—Boulder*

In his introduction to this overview of the applications and implications of virtual reality (VR) technology, Ralph Schroeder identifies the text's two goals. First, contributing to the sociology of technology, he aims for an account of the emergence and significance of "VR systems," in part, by describing case studies of VR applications and the contexts within which they operate. Second, Schroeder identifies a lack of interdisciplinary consensus on how communications media, information technology, and technological advances might best be explained and how this lack impedes the development of these explanations. He, therefore, uses VR as an organizing device for modeling the "social relation" he will identify between technology and social forces.

To contextualize his exploration of VR's social implications, the first half of chapter 2 is an abbreviated history of key technical developments, institutions, and individuals central to the technology's genesis. An overview of the technical aspects of VR systems follows. Chapter 3 describes the research and development context within which VR has developed. Arguing that specific institutions—such as the Human Interface Technology Lab at the University of Washington at Seattle—shape the directions of VR development, Schroeder outlines the competitive research strategies, which transcend national boundaries and distinctions among the academy, the military, and private enterprise. Such strategies synthesize basic research and commercial applications in the belief that VR will transform information technology and communications networks. Chapter 4 describes the educational and entertainment contexts of immersive and screen-based VR applications. Citing American and British examples, Schroeder seems bemused that educational applications' potential to free users' imaginations for building their own virtual worlds remains underutilized, noting that the majority of applications are predesigned entertainment environments. He suggests that failure to capitalize on VR's power to let users design worlds may prove an important drawback, as future forms of VR applications will be heavily influenced by existing configurations and uses. Turning to the contents of virtual worlds, chapter 5 offers an "inventory" of the "several dominant forms" (p. 122) comprising VR. Schroeder provides a good account of the VR game industry and how its products are developed and achieve marketplace acceptance.

Schroeder's descriptive accounts of the emergence and significance of VR exemplify the interplay or feedback between society and technology. There is in *Possible Worlds*, however, a third goal not fully developed: to critique social constructivist or postmodern "antirealist" theories cur-

rently enjoying academic vogue. The author claims equal discomfort with technological determinist approaches, which accord technology the status of savior of culture, and "antirealist" or social constructionist approaches, which, according to him, are "part and parcel" of a range of cultural phenomena springing up around VR (p. 4). Claiming a "realist" and a "Weberian" approach (never explained for nonsociologists), Schroeder suggests that both VR and academic postmodernism blur nature and culture, thereby promoting a belief that the world is increasingly an unreal place.

Chapter 6 attempts to bridge the empirical accounts preceding it and the critique of "antirealist" approaches the author wishes to make. To do so, Schroeder links the lifestyles and value systems of the computer hackers and jammers and the San Francisco and London "smart drug"-taking cyberclubbers, who believe that "advances in computer technology will provide the vehicle for new ways of life" (p. 125) and cultural renovation. The author moves to shakier ground when he critiques such theorists as Donna Haraway and Allucquere Rosanne Stone for mirroring the utopian yearnings that permeate the "cybercultures" just described. His use of these authors suggests a misreading. Stone, for example, pointedly critiques the anomie and social fragmentation of Silicon Valley ("Will the Real Body Please Stand Up" in *Cyberspace: First Steps*, edited by Michael Benedikt [MIT Press, 1992]). Haraway's famous call, "I'd rather be a cyborg than a goddess," is her refusal of depoliticized mysticism. It asserts her belief that feminists reject science at their own peril, and not—as Schroeder infers—that she would fuse her body with technology if the means existed.

Achieving a comprehensive understanding of a complex technology requires analysis. Description constitutes a requisite step in this process. Though numerous lists comparing and contrasting technologies and technical advances assist readers in arriving at their own conclusions, Schroeder hesitates to provide his own analyses. Sandwiching case studies of VR between chapters probing connections between this technology and postmodern theory lends the work an overly sketchy yet repetitive quality and an unresolved dualism of purpose. References and endnotes are detailed and useful and photographs well keyed to the text they enhance. Though short on considering the *meaning* of VR, *Possible Worlds* is rich in empirical descriptions. This alone recommends it as a good introductory text for social scientists wanting to learn more about VR. It would be a useful component for courses on the sociology of technology and on game playing, globalization, communications studies, and new media.



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